Considerations for a Two-sex Demography

When, why and how should both sexes matter to demography?

by

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Declaration

Except where otherwise indicated,
this thesis is my own work

....

(António Alberto da Silva Francisco)

Dedication

At the end of this doctoral thesis my heart goes out to those who would have most deserved that this work address some of their concerns. First, my thought goes to my country, Mozambique. Initially I wanted to concentrate my Ph.D thesis on the demographics of the Mozambican population and study it from a two-sex viewpoint. I soon realized that for the following two main reasons I should postpone my project: lack of data and, above all, lack of adequate theory. I am happy that the troubles which have affected the Mozambican population for so long cannot be blamed on either of them. While there is a shortage everywhere of adequate empirical data needed to explain demographic phenomena from the point of view of complementarity between the sexes, demographers have never been prevented from doing it theoretically. Beyond that, I believe that one cannot decide on the quality of demographic data independent of theory; nor did I find good enough to use my economics background as an excuse for not trying to understand demographic phenomena in their proper terms. This is how I came to understand demographers' contempt towards searching demographic issues in terms of the interaction between the sexes. In some interesting cases data on both sexes have been collected, though their usefulness was often not explored. This is how I explain that the disturbances through which the Mozambican population has passed in recent decades cannot be blamed for the lack of data needed to investigate the demography of Mozambique from a two-sex perspective. I hope that this work will shed some light for operational research designs aiming to identify and explore the sort of demographic phenomena which cannot be well understood unless demographers clearly define when, why and how both sexes should matter to the scientific study of population.

I also want to dedicate this thesis to my children, Cristiano, Vladimir, Pedro and Daniel. During the three years that I have been working in this project my wife and children have been wonderful companions, and above all very understanding when they did not get more of my attention because of this thesis. While I return to the former in the Acknowledgments, as to my children I will never forget the puzzlement lying behind the following question from my youngest son: 'Why Daddies need so many papers to write another paper?'. I dedicate this work to my children hoping that regardless of what they will do in their adult life they will try to do it with the same spirit that allowed me to complete this thesis: dedication and enjoyment. Beyond that, I just hope that my children develop confidence in their ability to learn:

> *Any path is only a path, and there is no affront, to oneself or to others, in dropping it if that is what your heart tell you ... look at every path closely and deliberately. Try it as many times as you think necessary. Then ask yourself, and yourself alone, one question ... Does this path have a heart? If it does, the path is good; if it doesn't it is of no use*
>
> *Carlos Castaneda, 1970* The Teaching of Don Juan.

Acknowledgments

Several people have contributed decisively towards making the beginning and completion of this thesis possible. First of all, I would like to thank The Australian National University for the scholarship without which I would not be able to produce this work. In particular, I am grateful for the fact that the ANU does not share the idea that Ph.D students have too much work to do, and thus it is advisable they leave their family in their home countries. I was confronted with such issues at the time I did my Masters. I am thankful to the ANU and those in its staff who helped and supported my work in the company of my family.

There is another reason, surely more directly related with this work, that confirms that I made the right decision in choosing the Demography Program of the ANU for my Ph.D. I have been fortunate in having the supervision and advice of some of the most prominent contemporary demographers. Professor Geoffrey McNicoll has been my main supervisor throughout the whole work. Anyone who has ever read McNicoll's demographic work should be used to the care he imposes on his writing. But in having the opportunity to work closely with Professor McNicoll I had the fortune to see in person and in practice the meaning of the two quotations heading the Introduction of this thesis. Moreover, throughout the past three years I have had several advisers, including Professor John. Caldwell, Dr Chis Young, Professor Gavin Jones, and Dr. David Lucas. Each of these advisers has given me useful comments and suggestions; but perhaps the most interesting aspect of work with my supervisor and advisers was that they have helped me to believe in Demography in its own full meaning. While the core of my thesis does not stem from the work of my supervisors, their comments, suggestions, and critics contributed significantly to the overall quality of this work.

Ms. Wendy Cosford knows better than I where I tend to get the spelling, the grammar and flow of ideas confused. Wendy's careful editing of my drafts has been extremely important, not only to communicate with the supervisors but to be confident that the overall thesis reads well; and whatever mistakes remain are not her oversight.

Other people have followed and supported this work in a variety of ways. Most of all, my wife Cristina; she has followed this work closely, and I have shared with her many of the ideas developed here. Although Cristina has no training in demography she understands remarkably well why demographers often appeal to the authority of common sense rather than to any elaborated population theory. Cristina has taken my thesis so seriously that often she gave more priority to it than to her own priorities. I am sure she shares my happiness for the completion of this thesis.

Secondly, I owe to Mr. Ian Ingram a special thanks for the programming work he did for me. Mr Ingram is a computer expert; he has no interest in demography, but he is one of those people who can transform abstract ideas into beautiful computer models and pictures. Our program awaits to be finished for two good reasons: first, I found it important to know first the work that has been done on two-sex modelling (some of which are in the form of unpublished Ph.D thesises); secondly, I needed to decide where my modelling would fit in the context of existing two-sex models, and for that is better to get this thesis finished.

Thirdly, I am thankful to the staff in the Demography Program of ANU, most particularly the Coordinator Professor Gavin Jones, Dr Mirka Prazak, Dr Ricardo Neupert, Dr. Daniel Goodkind, Dr. Lincoln Day, Dr. G. Carmichael, Professor Peter McDonald, Dr. A. Hayes, Ms Milisa Haberschusz (Program Administrator), Ms Pat Quiggin and Ms Jenny White (Librarian/Research Assistant), Ms Diana Crow (Programmer), Mr Gavin Longmuir (Programmer), and several administrative assistants who worked in the Department during the past three years.

Fourth, I acknowledge also the interesting conversations I had with my colleagues, particularly with whom I exchanged ideas related to our individual projects of research: C. Echavez, P. Foley, N. Jackson, J. Oni, J. Bryant, L. Stoukal, J. Pitso, B. and I. Utomo, and X. Wen.

I would like also to thank the authors of the three Ph.D theses which I used in the preparation of my own. Karmel’s Ph.D thesis became a special one for this work, because I got to know of its existence not through the literature but when I approached the author after being told that he lived in Canberra. I am convinced that the conference papers I prepared in the past three years in the context of this thesis have been the first to ever mention Karmel’s remarkable thesis submitted in October 1948. Dr Griffith Feeney and Dr Stan Wijewickrema also kindly responded to my request, and they both sent me their own unpublished Ph.D theses and allowed me to use them in this thesis.

Finally, I would like to thank my University in Maputo, the Universidade Eduardo Mondlane (UEM), and particularly the Economics Faculty. Despite my long absence, I have continued to be regarded as part of the staff of the UEM, and as soon as possible I hope to reciprocate such an privilege. From Maputo I have also received useful support and encouragement from some friends and my brothers; to all of them goes my *Kanimbambo*, which means thanks in the local language.

Preface

This thesis was submitted for examination late in February 1996. On 27 May 1996 an oral examination, requested by one examiner was held in Canberra; it was attended by one of the three examiners and an assessor only. Based on the three examiners' reports, in September 1996 the Chairman of the ANU's Graduate Degrees Committee recommended my admission to the degree of Doctor of Philosophy, subject to a few amendments being made in the Library copy of the thesis, to the satisfaction of the Head of Department and the Director of Research School of Social Sciences.

None of the three examiners requested any substantial amendment in the main text and, in particular, concerning the unifying theme of this work. The oral examination requested by one of the examiners focused almost exclusively on my unpublished paper on Graunt's *Observations* (Francisco, 1995), which initially I attached in full as one of the two Appendix. Although that paper emerged in the context of this thesis and has significantly influenced its writing, I agree that it stands as a separate paper; indeed, much of its content is devoted to issues not germaine to the debate on the envisaged two-sex demography.

To the three examiners I owe more than can be conveyed in a mere statement of acknowledgment; their scholarly remarks, generous encouragement and useful advice for improvement of the body of this work have been invaluable. In accordance with the ANU policy, the name of the examiners on this thesis should remain anonymous. In any case, I take this opportunity to thank the three examiners for the time, attention and interest they have all dedicated to this work.

Abstract

Demography, so the cynics say, is long on methods but short on ideas. Or is it? This Ph.D thesis takes up the challenge posed by that negative view. It shows that although cynicism can seriously damage the reputation of conventional demography, its sway does not survive a careful scrutiny of the history of demographic ideas, to say nothing about expanding them further. Indeed, demographers do not need to regard themselves as scholars who are just testing hypotheses produced in other fields, or have no alternative to simply borrowing a two-sex approach from elsewhere. However, one cannot simply assume that the community of demographers needs yet another analytical framework, this time called 'two-sex demography'. Some of the specific questions addressed in the thesis include: Why has the simple fact that population is constituted of two sexes never been enough to develop a demographic analysis standing on the complementarity between the sexes, rather than on the conflation of both sexes into neuter aggregates or the separation of one sex from the other? Why has the so-called two-sex problem tackled for more than half century in mathematical demography never become a central issue in general demography? Can students of population aspire to an adequate theory of fertility determinants without even deciding upon when, why, and how should the complementarity between the sexes be taken into consideration? If demographers do not study population from a two-sex perspective, how can they advise policy-makers to formulate policies which should inevitably affect both men and women? These and several other questions are concerned with conceptual and methodological fundamentals, rather than technical and administrative issues. In short, the thesis discusses how demographers could set out on the search for a coherent and systematic two-sex explanatory demography within demography itself. And in particular, the thesis shows that thinking about demographic phenomena in terms of the complementarity between the sexes stands as perhaps the most persistent challenge to demography in the future.

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1.

Introduction: towards a two-sex demography

Agnosticism is healthy, but sound theory can reduce its sway (Demeny, 1995: 8).

There is nothing so practical as a good theory (Lewin, in de Bruijn, 1993: 2)

The fundamental question for a debate on a two-sex demography

The title of this thesis states its main task: to discuss to what extent demographers can aspire to the creation of a coherent body of analysis called 'two-sex demography' within demography itself. But the subtitle states the important question raised and discussed throughout the thesis: 'when, why and how should the complementarity between the sexes matter to demographers?' Demographers have always been aware, if only implicitly, of the theoretical implications of this question. However, even when some authors have explicitly attempted to develop two-sex models for demographic analysis they took the central question signalled in the subtitle of this thesis, for granted.

Everybody knows that sexual reproduction needs two parents; but the science of demography has developed, for more than three centuries, because the simple understanding that both males and females are indispensable to demographic reproduction has not helped much to get to satisfactory answers for many empirical puzzles concerning the characteristics and nature of demographic change. Indeed, the bulk of the results provided by conventional demographic science often does not require the consideration of what in this thesis I will call the complementarity between the sexes. Many issues have been answered in disregard of even basic individual traits, such as age, sex, and marriage. Other issues require the presupposition that population reproduction stands not on a single but two demographic natures, male and female. But, still, demographers have realized that in many cases they could reasonably ascertain many demographic phenomena on the basis of the counterintuitive assumption that only the female component of population really matters. In its fundamental way, such cases comprise the description of what has happened to a given population, namely the description of the size, levels and patterns of population, as well as the characteristics and changes in its components and structure.

Yet a rather more difficult but inevitable issue for demographers emerges when they need to move from simple description of what has happened to the explanation of the causation and the mechanisms of demographic change. This is when the complementarity between the sexes needs to be taken into consideration and, in particular, when neither sex can *a priori* be said to be more important and representative than the other.

This debate concerns conceptual and methodological fundamentals, rather than technical and administrative questions. Above all, the most important challenge for demographers is not accepting the common sense conception of population reality that both sexes matter, but how a two-sex demographic analysis can add depth or, better, provide the indispensable theoretical scheme for the explanation of empirical puzzles which conventional demography is not in position to handle through its neuter and one-sex approaches.

How can demographers set out on the search?

One cannot simply assume that the community of demographers needs yet another analytical framework, this time called 'two-sex demography'. Each time demographers reach for what appears to be a more fundamental explanation of population reality they have to ask whether, in fact, it is more fundamental, and whether it adds substance to the understanding attained through other methods. Thinking about demographic phenomena in terms of the demographic relationships of complementarity between the sexes stands as perhaps the most persistent challenge to demography in the future. Can demographers accommodate it? How should one set out on the search?

Before moving any further into what might be a more fundamental justification for the envisaged two-sex demography, it seems important that readers do not simply take for granted that demographers have, in some way, always tried to practise some sort of a two-sex approach. Of course, in a very loose but misleading sense some could think that a measure such as the crude birth rate is a two-sex measure because it relates births to the whole population. Or yet, that a simple comparison of total fertility rates (TFRs) based on female population with similar measures based on male population is the way to move to a two-sex demographic analysis. However, this thesis shares the implicit but widespread view among demographers that students of population would not learn much if they were taught about male TFRs, as much as they are taught about female TFRs.

There is little doubt that commonsense works when it is congruent with reality. But it is also widely known in demography, as in all other sciences, that commonsense concepts of reality repeatedly leads to very misleading interpretations; or when it does not, the insights that it provides usually shy away from any adequate attempt to explain their methods. Thus, the idea that a two-sex approach is all too evident to commonsense views is neither self-explanatory nor even scientifically correct. After all, demography has grown for more than three centuries precisely because commonsense showed itself to be a poor guide to any adequate description and explanation of demographic change.

Most of the content of this thesis is the result of a serious intellectual effort aiming at overcoming two tempting and thorny attitudes towards the complementarity between the sexes. The first, and perhaps the most attractive, is the tendency to appeal to commonsense, that is the way people think in their daily life. Everybody knows that real populations are constituted of two major sexes; so, intentionally or unintentionally, one is often tempted to entertain the idea that the scientific study of population should always take into consideration the contribution of both sexes simultaneously. However obvious this thought seems to be, only someone totally unaware of the demographic techniques and theories can genuinely entertain the idea that well known demographers have attained their reputation by developing methods consistent with the complementarity between the sexes. From Graunt to Malthus, and from Kuczynsky to Bongaarts, the majority of demographers of all ages have so far secured their name in the history of demography mainly because they developed concepts, methods, measures and theories of a 'neuter' or a 'one-sex' nature.

The role played by the principle of complementarity between the sexes in demographic reproduction is often left implicit, if not deliberately or explicitly ignored. This is not because demographers are unaware of complementarity in demographic phenomena, but because a great deal of demography can generally stand on principles which take the relations of complementarity between the sexes as given. The bulk of demography either assumes population as a neuter entity, or simply studies its characteristics, growth and development assuming that only the 'producing sex', as Knibbs called it, matters.

A second and apparently more convincing justification for the envisaged two-sex demography draws on the idea that conventional one-sex demographic methods produce inconsistent or contradictory results. This view has already led to a remarkable search for two-sex models and, indeed, the expectation that the canonical neuter and one-sex basis of demographic analysis should be completely replaced by a two-sex framework. I refer specifically to the field that has come to be known as the 'two-sex problem', an unconventional field of research that since the middle of twentieth century has secured its place in mathematical demography.

However, this thesis points out serious inadequacies in the justification and conceptualization of the field known as 'two-sex problem'. The departure of this thesis from orthodoxy in the ‘two-sex problem’ is to argue that neuter and one-sex conventional demographic approaches are not an anomaly; they can and will most probably remain as powerful as they have shown to be whenever demographers simply aim at describing what happened to a population in a given point or period of time.

At some stage I have become puzzled by the contempt and disdain that the two-sex problem continues to provoke among demographers. The widespread neglect of the 'two-sex problem' became compelling when I turned to conventional demographic curricula, textbooks and literature to find that such a subject is not even part of the priorities for demographic teaching and research. Even in textbooks of formal demography the two-sex issues that time and again have captured the attention a few eminent demographers are generally ignored. In short, the 'two-sex problem' has been treated by all branches of demography as a technical curiosity or a useless jigsaw puzzle, which nonetheless has no use for empirical research and policy strategies. Only when I became aware of this did I begin to admit the possibility that something has definitely gone awry in the way the two-sex modelling has developed over the past five decades. This particular aspect has changed the nature of my research concerning the feasibility, validity and usefulness of the two-sex demographic approach outline in the present work.

One could blame conventional demographers for the isolation of two-sex research from the mainstream body of demography; but this in itself offers little satisfaction, for it does not exclude the possibility that the 'two-sex problem' itself has in fact been badly handled and, for that reason, most demographers have lost interest on the subject. In other words, it needs to be admitted that the reduction of the research on two-sex modelling to a technical problem has led many demographers to believe that the possibilities of studying demographic issues from a two-sex perspective are limited. Very often the two-sex models are said to deal with complicated issues; or their apparatus is said to require complicated and tedious numerical computations for which data are scanty, nor is there much guarantee that they lead to new insights.

By uncritically adopting any of the above two views, demographers can seriously misunderstand the development of earlier and recent demographic ideas that are leading to the two-sex demography, and are not well situated to comprehend what lies ahead. Such misconceptions may delay the process of identification and definition of the central research question set for by this thesis and make its investigation more difficult. This became particularly apparent, in the process leading to the present thesis, when I discussed the subtleties of my research topic with my supervisors and some colleagues. Some have been taken by surprise and puzzled, for instance, with my insistence that no gender or generational approach can claim to provide an adequate demographic analysis if it ignores, as it has so far happened, the theoretical implications of the efforts made in mathematical demography to develop two-sex models. Others have thought, at least for some time, that the present work was motivated by a dissatisfaction with the widespread neglect of males in demographic analysis. And, at least a few became yet more puzzled when they realized that my concern was not to convince demographers to give an 'equal opportunity', say, to male TFRs; indeed, this thesis is not aimed at 'bringing men in' to the conventional female-only demography.

One of the main thrust of this thesis is that the 'two-sex problem' can no longer be seen as a euphemism for the problems of one-sex methods only. The field of two-sex modelling should move beyond the debate about whether the one-sex methods, when they are used, produce false or misleading results. Such a development is in part happening in the 'two-sex problem', and this is witnessed for instance in the following reference to the two-sex modelling found in the 1982 *International Encyclopedia of Population*:

> The contemporary period has reached beyond Lotka in several directions. It has gone further with cohorts as well as with time periods; it has tackled the two-sex problem (although that seems to be beyond any simple solution) ... Acceptance of dominance avoids the essential difficulty of the two-sex problem, on which there is a large literature. Any linear model that takes account of both parents runs into difficulties when the sex ratio departs substantially from unity; if births depend on the mean number of men and women, then if one sex drops to zero the births are reduced only by half where they should drop to zero. Yet nonlinear models seem impossible to handle mathematically. Aside from technical difficulties, the number of offspring depends on behavior that is not embraced by any mathematics using presently available data. Number of offspring of any species, including the human species, usually depends more on the number of females than on the number of males, but how much more is determined by how active the males are (Keyfitz, 1982: 438, 441).

This summary of the two-sex problem entails a certain acknowledgment of the envisaged two-sex demography, though an acknowledgment in a state of chronic denial. First, Keyfitz considered the two-sex problem as part of the important developments that have moved beyond Lotka or, more specifically, beyond his neuter and one-sex frameworks of stable population theory. Secondly, Keyfitz pointed out how the one-sex principle of separation of the sexes, or as he put it 'acceptance of dominance', avoids the technical difficulties in which two-sex have run to. And thirdly, Keyfitz conceded that two-sex modelling involves not technical difficulties only. 'Aside from technical difficulties', Keyfitz wrote, 'the number of offspring *depends on behavior* that is not embraced by any mathematics using presently available data' [Emphasis added]. In the terminology proposed in the present work, *demographic outputs*, such as the size, levels, patterns and other characteristics of population, depend on behavior, that is on *demographic outcomes* from both sexes. Even if one assumes that demographic outcomes 'usually depends more on the number of females than on the number of males', as Keyfitz admitted, the behaviour and practices - 'how active the males are - does matter.

In any case, the fact that the above remarks from Keyfitz constitute the only reference to the two-sex modelling in a two-volume *Encyclopedia of Population* is in itself telling. The moral afforded by such an isolated reference to two-sex models is twofold. In a certain way, the expression 'two-sex problem' was initially intended to highlight the problems of the 'one-sex methods'. However, the expression ‘two-sex problem’ has long stopped being just an euphemism for the alleged contradictory results produced by one-sex conventional population theories. The two-sex modelling itself has become a 'problem' in its own right; a problem that this thesis demonstrates is caused more by the weak taxonomic level of inquiry of two-sex modelling than technical difficulties. Thus, another important finding emerging from the investigation exposed in this work is that the greatest challenge for the envisaged two-sex demography concerns conceptual and methodological fundamentals, rather than purely technical difficulties. In particular, at issue is the need to identify the purpose and role of a two-sex approach as opposed to the conventional neuter and one-sex bodies of demography.

In short, the thrust of this thesis is that a two-sex demography is needed not to improve or substitute demographic measures of populations that have been settled with sufficient precision by conventional neuter and one-sex methods. Instead, a two-sex demography stems from the need to move towards a systematic body of explanatory theory able to dig deeper into the causal mechanisms which underlie the descriptive accounts that the neuter and one-sex demographic frameworks offer. Being able to estimate and describe accurately the size, structure, components, levels and patterns of population is not at all enough to understand its demographics. The reason is that descriptions and predictions, however accurate they may be, are simply not substitutes for explanations of population change. Demographers can only be sure to have adequately understood population reality if they are able to reveal and explain the demographic characteristics and changes in terms of their underlying causal mechanisms, including causes that are not directly experienced (or observed) by people, such as the complementarity between the sexes.

What is a two-sex demography? - a preliminary definition

Rather than expecting readers to grasp the meaning of a two-sex demography as the discussion proceeds, at least a preliminary definition seems useful now. It is important to make it explicit from the onset that in this thesis the consideration of the feasibility, usefulness and validity of a two-sex demography will not be reduced to a methodological issue.

The two-sex demography envisaged here comprises the seam of and epistemological setting and a methodological framework, which can be expressed schematically as follows:

Two-sex demography = {{2-sex conceptual setting} + {2-sex methodology}}

This analytical body of demographic analysis is expected to be directed at the underlying causes and mechanisms that have made the magnitude and changes in the direction and pace of size, territorial distribution, and structure of population what they are.

A definition like this should prevent demographers from appealing to the authority of a concept of realism, drawn either from simple commonsense or from any other philosophical persuasion that demographers use more or less explicitly, including those which reduce demographic reality to what is observable or measurable and those which wish to 'construct' demographic reality out of mind, concepts, language, methods, theories or whatever. I will return to the question of demographic realism in this Introduction.

The core epistemological basis for a debate on the usefulness, feasibility and validity of a two-sex approach is set in the subtitle of this thesis by the conditions 'when' and 'why'. That is, a two-sex demography needs to be defined in its own right, both in terms of the history of demographic ideas and the important theoretical principles in which demography stands. The scope of the epistemological grounds for a two-sex demography should become clear as demographers search not only for methodologies aiming to reconcile male and female indicators in association with well identified and defined empirical puzzles and research problems demanding two-sex methods. Thus, the question is: what kind of issues, when, and why should demographic research problems require two-sex concepts, measures, models, methods and broad theories?

The short answer to this question as signalled in the subtitle is that a two-sex approach is needed whenever demographic phenomena need to be explained as single outcomes from the relationships of complementarity between the sexes. That is, demographic phenomena should not be studied only either as the product of the conflation of both sexes, or the output of one sex assumed independent from the other. The thesis maintains that the broad conceptual setting in a two-sex demography can be called 'intergender-generational approach' (IGG approach). The reasons for this designation should become clear below and, for instance, when standard demographic concepts such as 'population composition' are discussed; or when the thesis shows that the categories 'gender' and 'generation' should be treated neither as synonyms nor as substitutes for the standard demographic variables sex and age (see Chapter 8).

In turn, just as the complementarity between the sexes has become for this thesis a matter of principle rather than a rhetorical or technical convenience, the debate on a two-sex demography needs to consider 'how' to do it. This decision should not be left to the free will of each researcher, nor be determined by the priorities of the funding agencies. The consideration of a two-sex demography from a conceptual point of view is closely related to the debate of its methodological foundations, namely how research has been or should be carried out.

The principle of complementarity and demographic concepts

At the heart of this thesis is a principle I will call complementarity between the sexes. This principle reflects and responds to the set of contingencies and organizational mechanisms in demographic phenomena which exists in population reality whether people experience it directly or not, or whether demographers are aware of it or not.

Complementarity between the sexes is part of the objective reality of population, just as the twofold universal principles Malthus identified in terms of two basic human needs: survivorship, which he reduced to the need of food or subsistence; and reproduction, which he expressed as a function of the passion between the sexes (see more on this in Chapter 5).

As a philosophical approach, complementarity between the sexes entails a dualism between population reality and demographic theory. Such a dualism is indispensable whenever one needs to distinguish ontological realism from epistemological realism; that is, the distinction between what there is in population and how demographers can know it. In this thesis I take the former as given, for I assume that demographic concepts and measures refer to entities that exist in population reality independent of people's (including demographers or other population analysts) beliefs about them, and even if those entities are unobservable and for whatever reason still not measurable. However, with regard to epistemological realism, throughout the thesis I will highlight the importance of epistemological issues as a way to counter the widespread tendency of demographers to make their research of population realism dependent on practical needs and methodological issues.

The term 'complementarity between the sexes' itself has never had any currency in demography, though it seems to be appropriate to bring together and embrace several concepts which are already part of the lexicon of our discipline. These concepts can be found scattered in a vast literature in demography and its allied disciplines. Some of them have been used in a very technical way and either in association with specific formal mathematical models or applied in empirical measures; others are mere abstract concepts which so far have not been immediately measurable. Despite being used in a great diversity of issues all these concepts share the core feature depicted by what is called here the complementarity between the sexes: that each sex is mediated by the other and thus both sexes matter; together they hold the key to understand the causal mechanisms which underlie the picture which descriptive demography portrays. In its fundamental way, if one of the sexes is taken in exclusion of the other, whenever one needs to explain rather than just describe population reality, what is left is pure indetermination.[1]

This does not mean that everything else, that is all the concepts and measures currently used in conventional demography which are not consistent with the principle of complementarity, should be regarded as indeterminate or inconsistent. The bulk of demography stands on rather different theoretical principles, namely what can be called the principle of differentiation between individuals (corresponding to the 'neuter demography'; see Chapter 9), and the principle of the separation of the sexes (corresponding to the 'one-sex demography'; see Chapter 10).

The demographic concepts and measures can be classified in three main types. Some concepts and measures represent the demographic relations by conflating all ages of both sexes. They emphasize the unity among individuals on the basis of their human nature; so they take no account of their differentiation set by the standard variables sex and age. Some examples of neuter concepts and measures are the crude birth rate, population size, natural increase, the life table for persons, and dependency ratios.

A second array of concepts and measures depict demographic relations by taking into consideration the differentiation among individuals on the basis of sex and age. Each

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[1] Indeed, this is what allows demographers to alter their interpretations, for instance, of demographic transition theory; without regard to the complementarity between the sexes, demographers can conjure up virtually any explanation they like (for more about the demographic transition as as a neuter framework see Chapter 9).

sex can be studied separately from the other with the objective to capture certain aspects of demographic reality. This either-or approach has developed into a powerful one-sex body of analysis which deals with demographic phenomena as demographic outputs determined mainly by the female component of population, for this is the producing sex in any demographic system. Examples of one-sex concepts and measures are the age-specific fertility rates, gross and net reproduction rates, life-tables by age and sex, child-woman ratio, and the singulate mean age at marriage.

A third array of concepts and measures deal with certain aspects of demographic phenomena as an outcome of the relations of complementarity between the sexes. Such concepts, among others closely related to them, are depicted in Figure 1.1: the sex ratio, including the regularity between the sexes in terms of its stability and deviations; the passion between the sexes, a principle of population first stressed by Malthus; the interactions in terms of numbers, behaviours and combinations of the ages of both sexes, the nuptiality relationships, the marriage and mating attraction and the balance of the sexes. Although the term 'complementarity' is the least familiar among the concepts illustrated by Figure 1.1, it is clearly the most adequate to reconcile the general content of the envisaged two-sex perspective, including its conceptual and methodological setting, as well as its language of communication and the utility of its results.

No wonder that more often than not the above three types of concepts and measures are taken for granted and not made explicit; in their own way, they are consistent within the domain of validity defined by the theoretical principles in which they stand. However implicit and hidden such theoretical principles may be, they set the domain of validity of the demographic content that developed consistently with them.

In this context, if the complementarity between the sexes is used to bring together concepts and measures consistent with this principle, it becomes apparent that the content of a possible two-sex demography is not as obscure and abstract as might be thought. The field of two-sex theorizing is undergoing a rapid development in demography, though still in a scattered and irregular way. The difficulty of showing the significance of a two-sex demography is in part aggravated by the fact that the definition of complementarity between the sexes has been sketched here before its necessity and usefulness have been discussed. This is the problem of starting any scientific debate with the definition of its fundamental principles: on the one hand, definitions usually provide the easiest way to grasp the content and limits of new concepts in science; on the other hand, because definitions abstract from the specific properties and relations they refer to, they are far from being self-evident.

There are at least three ways which can minimize the limitations of a purely abstract definition, in this case of the principle of complementarity between the sexes: be specific, be historical, and avoid clichés. The specific properties and relations which a new scientific concept is said to represent, as well as the context in which it has developed, should contribute to a good understanding of its development as part of the evolution of the discipline in general. Even though the complementarity between the sexes is portrayed here as the Philosopher's Stone of a possible two-sex demography, a significant part of its content is far from strange to earlier and contemporary demography. This assertion should become clear throughout the chapters that comprise Part I.

The third way to prevent possible misinterpretations and distractions is to avoid terms which reflect philosophies of science not necessarily consistent with the views underlying this thesis. For instance, however fashionable the term 'paradigm' has become in current times there are two reasons for not using it here. One reason is that the term 'paradigm' has been used too loosely and too frequently in recent times; it is increasingly turning into a cliché and, as an African proverb put it nicely, 'a word that is always in the mouth turns into slobber'. In such a situation, any tag attached to a new concept, far from adding importance and more meaning to it, is likely to distort and distract from its own content.

The second reason has more to do with the philosophy of science which, in recent decades, cradled the concept paradigm itself, that is Kuhnian philosophy. Neither the principle of complementarity between the sexes nor the idea of a two-sex demography is seen here as being the result or leading to a confrontation and a 'paradigm clash' with the principles of 'normal' demography. However revolutionary the complementarity principle may be, I see no reason why it should overthrow the canonical demographic principles, nor even expect the latter to die out or simply fade away. I doubt that, in science, this sort of *coup d'état* ever happens, at least in the revolutionary and radical fashion implied by the notion of 'paradigm clash'. On the contrary, what a new scientific principle usually does is to widen the domain of validity of scientific research, but the old and less comprehensive set of principles continue to be used within their own domain of validity.

In short, in this thesis the complementarity between the sexes is called nothing but a principle, and it is as such expected to guide and dictate the kinds of problems that are important to address, and the kinds of conceptual explanations and methodologies that demand a two-sex approach. The content of demography can be classified according to certain kinds of scientific principles and these principles should set the domain of validity of the concepts and theories used. Thus, the acceptance of the principle of complementarity in a two-sex demography does not require abandoning the set of principles that preceded it.

The role of theory in demography as a social science

An understanding of two-sex demography in the broad perspective proposed above opens two areas of investigation, areas to be explored throughout this thesis: (1) the relationships in demographic phenomena consistent with the complementarity between the sexes; and (2) the issue of when, why and how the complementarity between the sexes demands a two-sex conceptual and methodological approach.

Before exploring these two areas of investigation in the chapters that comprise the two main parts of the thesis, in the remainder of this Introduction I will briefly place the unifying theme of this work in the wider context of the historical and philosophical approaches chosen for the thesis. But first of all, a note on the quotes placed as epigraph of the eleven chapters comprising the thesis.

With the exception of a few quotes chosen for aesthetic reasons (i.e. the quote in Chapters 2 and 10), the quotes have been chosen with the purpose to stress: (a) the pivotal role of theory in demography as a social science (i.e. in Chapter 1, 5, 6, 7, 9, 11); (b) issues which have often been reduced to matters of fact, such as the case of the sex ratio, but in theory construction play the important role of explanatory resource as well (Chapter 3, 4); (c) the need to distinguish and interrelate the historical reconstruction with logical analysis. Above all, the main objective of the quotes placed as epigraph is to counter the typically instrumentalist attitude contemporary demographers show towards the role of theory in demographic research.

Such attitudes are of different philosophical persuasions. Some are deeply empiricist, positivist and even naively pragmatic; for these, theory is like an adhesive dressing, or a bandaid, that at some stage is stuck to descriptions and predictions, either by demographers or other population analysts, if for nothing else at least to make them more intelligible and entertaining to others.[2]

Others repudiate not only empiricism and positivism but also scientific realism, and they can be broadly designated as cultural relativists. Rather than seeking to discover what has happened in population reality, these scholars contend that demographers should merely investigate how people judge things are, or still how researchers judge that people judge they are. There are various forms of cultural relativism, from the most extreme sociological and anthropological forms to several types of solipsism. The former wish to make demographers believe that population reality is not objective and does not exist independent of people's thoughts, beliefs, concepts, experience and language. Demographic reality is only considered to be 'reality' for a particular society and what is real and hence true for one population may not be for another. The latter claim that reality is an individual's construct; researchers and people in general cannot be sure of what they know about reality because each person may construct his or her own world, with no or very little overlap with that of others. In recent years, while the 'rock-solid' foundations of empiricism and positivism have been exposed and disputed, cultural relativism has managed to become very fashionable in several academic and scientific fields; and demography is certainly not going to resist or escape from the fashion of the time.[3]

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[2] As Caldwell (1988: 13) put it: 'Sociologists often regard demography as an area where quantification takes precedence over theory'. In a more recent paper Caldwell (1996: 331) lamented: 'Unfortunately, demographers are prone to conclude that a phenomenon does not exist if they can find no satisfactory way to measure it'.

[3] The disparagement of demography's strong reliance on mathematics is running unhindered and, indeed, it has so far been received with remarkable complacency from mainstream demographers. To

Any extensive discussion on the implications of either the dominant or the fashionable philosophies of science for demographic research is beyond the scope of this thesis. However, I should make it explicit that the chosen philosophy of science for this thesis is called realism and, thus, the whole purpose of demography as a social science is considered to be the pursuit of truth as some form of correspondence with reality. I return to the question of realism in a moment, but in short it may be advanced that in choosing the philosophy of realism there are at least three main attitudes I try to avoid in this thesis. First, the tendency to totally separate and oppose, rather than simply distinguish, facts or data about demographic phenomena from concepts, interpretations or any theoretical scheme. Second, the frequent reduction of demographic research only to what is observable and measurable. Third, the widespread belief that the main purpose of demography is to describe and predict demographic phenomena, rather than explain them in terms of the underlying and deeper reality of the causes and mechanisms that determine those demographic events.

In any case, appeal to the authority of eminent scientists or eloquent works is not satisfactory when used for purposes of persuasion. First, authority is usually more intimidating than persuasive. Second, the views on scientific and historical aspects expressed in the thesis are not necessarily akin to those of the statements used as epigraph. And third, one cannot assume that the authors of the quotes have drawn on similar philosophies of sciences and history. Thus, the remaining two sections in this Introduction contain additional remarks on the chosen philosophy of science and philosophy of history for this thesis. These two aspects are briefly discussed around two main dualisms which underline, explicitly or implicitly, this work. One is the dualism between historical reconstruction and logical analysis, and the other the dualism between demographic theory and population reality.

Placing the two-sex demography in the history of demographic ideas

Broadly speaking, history is the collection of information and description of the unfolding of events concerning either the development of some historical events, or the interpretations, ideas and critical investigation of their context in terms of time, place, correlation, and coherence. How is 'history' used in this thesis to help understand the unfolding of two-sex demography within demography in general? How does the historical overview of demographic ideas, set around the work of the main figures in the

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mention just a few examples, see Bledsoe and Pison (1994), Greenhalgh (1995), Greenhalgh and Li (1995), Kertzer (1995). Moreover, in recent years mainstream international demographic journals appear to have become in the grip of cultural relativism. Articles bedevilled by anthropological constructionism are increasingly published in journals such as *Population Studies*; the following examples are quite suggestive: 'Whose reality? Local perceptions of fertility versus demographic analysis' (Randall, 1996); 'Anchored narratives: the story and findings of half a century of research into the determinants of fertility' (Van de Kaa, 1996).

field on a number of key concepts, help to elucidate the development of a two-sex demography? How can a two-sex perspective inspire new inquiries into past and earlier sources? The purpose of this section is to provide a brief answer to these questions.

At first glance, the origin of a two-sex demography could be traced just as far back as 1946-48: that is, when the French demographer Paul Vincent (1946) first addressed the issue and, independently and more fully, the Australian demographer and economist Peter Karmel (1947) set the grounds for what is now widely known as the 'two-sex problem', and proposed the first mathematical two-sex model. As I have pointed out above, the term 'two-sex problem' has already become not just an euphemism for the 'one-sex problem' but also an expression of the specific difficulties in creating adequate two-sex models. Moreover, a two-sex demography cannot be seen as a '50-year problem' in the way the so-called 'two-sex problem' can be. Indeed, ever since Graunt the problem of the scientific study of population has been twofold: how to study a population which includes two sexes, and how not to study it as if it were asexual or had one sex only.

Any suggestion to bypass a broader historical search for the earlier anticipations of a two-sex demography may sound convenient, at least on the grounds that it would save time and energy; but in science short-cut alternatives end up being untenable because in the long run they become scientifically self-defeating.

I consider the dualism between historical reconstruction of actual events and logical analysis, or any analytical criterion used to select and interpret historical processes, a position in line with the best of demographic tradition that is consistent with a philosophical and scientific realism. In principle there should be no contradiction between integrating selective and relevant historical material with a system of categories ordered logically rather than historically. In this context, in writing the historical part of the thesis I have tried to keep a balance between historical events or eminent figures in the field and the interpretation of their relevance for the growth of demographic ideas. The dialogue between the interrogating contemporary demography and its interrogated past draws on a view of historiography similar to the one outlined in the following statement:

> Historiography is a dialogue between an interrogating present and an interrogated past. Separated forever from the living past, the interrogating historian in following his proper art can reconstruct only from what he sees and understands. The questions put to the sources may change with knowledge and viewpoint as one questioning present is succeeded by another. Each may offer fresh insights. We are alerted to the past by experience of the present (Crombie, 1994: 8).

The three main aspects stressed in this statement play an important role in the historical approach chosen for this thesis: that it is possible to establish a dialogue between an interrogating present and an interrogated past; that the interrogating historian follows his or her ‘proper art’; and that one can reconstruct history not only from whatever evidence exists about past events, but also from new interpretations, ideas and understanding of them.

The purpose of the historical overview in the present work is to identify concepts that can work as an explanatory resource or an organizing principle in inquiries about the unfolding of two-sex demographic perspectives. Indeed, if Popper is right, 'although history has no meaning, we can give it a meaning'. What is the underlying criteria and meaning for the particular view of the history of demography exposed in this work?

First, I make extensive use of materials that rely heavily on the conventional chronological and narrative approaches to history, namely Dupâquier and Dupâquier's (1985) *Histoire de la Démographie* and Hald’s (1990) *History of Probability and Statistics .. before 1750.* Of course, considering the purpose of this work, my historical overview is brief and heavily reliant on secondary sources. This is so, not so much because of the time constrains set for this thesis, but because of the nature of its topic. Rather than intending to write a comprehensive review of the discipline, the present work is aimed at elucidating the meaning and role of main analytical bodies in demographic literature as a whole. In this context, the reliance of the thesis on secondary sources reliable enough to support the reinterpretation offered here seems more appropriate for the purpose of this thesis. As Hald wrote in his book just mentioned:

> Since history consists of facts and their interpretations, history continually changes because new facts are found in letters, and books, and new interpretations are offered in the light of deeper understanding' (Hald, 1990: 1).

Second, my insistence on the pivotal role of theory over observation may be considered too biased towards, as one of its examiners asserted, a prescriptive philosophy of history and science. 'Under what circumstances', the examiner asked, 'may it be legitimate to take a prescriptive philosophy of science as a model of how thought on certain problems has, and has not, developed?' This question seems to presuppose that there are circumstances in which historians are able to talk about reality and historical events without conceptualizing them. But the same examiner insisted: 'How important is an understanding of the social and intellectual contexts of knowledge, which went through several major historical transformations between Graunt and Knibbs, to our understanding of the evolution of population inquiry?' Although the examiner did not clarify his deeper motivations and concerns, he indicated to be dissatisfied with the selection of main figures in the field on a number of concepts used to elucidate the unfolding of two-sex demographic ideas; in his opinion I should have concentrated on the social and intellectual context of how demographic knowledge is constructed, rather than on how demographic reality is captured and expressed by demographic concepts.

Thirdly, what has led me to use and adapt a paper from Wunsch (1984) on the logic of demographic discovery were logical rather than historical reasons (see Figure A2 in Appendix A). My interest in Wunsch's paper stems basically from two features he stressed. First, Wunsch (1984: 2) wrote: 'Science starts off with a question, a problem, a puzzle'. Second, Wunsch (1984: 2, 17) asserted that 'research in demography implies moving from theoretical concepts to auxiliary theory, and then from auxiliary theory to statistical model'. While Wunsch did not get into how, for instance, the logic of discovery in demography interacts with the unfolding of demographic ideas, I use his suggested logic below to avoid creating the impression that historical reconstruction does not entail any logical scheme.[4]

Moreover, I have no objection if the historical overview provided in this thesis is said to have assimilated much of the Popperian philosophy of history, but only in the perspective proposed by Wilkins rather than in Popper's own terms. As Wilkins contended:

> Popper has erred not in giving too much to science but in giving too little to history, and that this error results in the paradox of Popper's subscribing in effect to the Myth of the Framework in historical inquiry while repudiating it elsewhere (Wilkins, 1978: 29)

In this context, Chapters 2 to 7 discuss the strands from which one can hope to weave a two-sex demography. First the sex ratio is discussed in Chapter 2, 3 and 4 as the most elementary and simple expression of complementarity between the sexes. Table 1.1 provides a snapshot of the two-sex demography in the wider context of the history of demographic ideas. The table includes only some of the important authors and events, and throughout the thesis more details on other authors and sources are given.

As I show below, the chronological periodization of earlier anticipations of the two-sex demography summarized in Table 1.1 supports the logic behind the concepts depicted in Figure 1.1, which is drawn on the principle of complementarity between the sexes. In particular, Table 1.1 depicts the two features in the sex ratio that need to be considered, that is as a measure of matters of fact and as an explanatory resource in theory construction. In this regard, there is no better work than Graunt's *Observations* to introduce the significance of the sex ratio in demography. Initially, I intended to include as an appendix a paper I wrote in anticipation of this work on Graunt's *Observations* (Francisco, 1995). However, I agree with the examiners that the paper should be removed, in part because much of its content has become an extended survey devoted to issues which go beyond the unifying subject of the thesis; and in part because the paper can stand as a separate document.[5]

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[4] In his 1984 paper, Wunsch seemed bedeviled by universal theories; he insisted that a rigorous test of conceptualizations of demographic processes is best pursued within the framework of the 'hypothetico-deductive method of explanation', as developed by Karl Popper and Carl Hempel. This is not the appropriate place to get into any further remarks about the advantages and limitations of Wunsch's application of the logic of demographic discovery within the specific framework of Hempel's hypothetico-deductive method of explanation.

[5] My paper on Graunt's *Observations* (Francisco, 1995) remains unpublished, but copies of it can be obtained either from the author or the Library of the ANU Department where this thesis has been writen. However, in short, the paper discusses the inception of Demography; why Graunt has been under-estimatted or too narrowly acknowleged in contemporary demography (i.e simply mentioned in the study of mortality); the main portrayed of Graunt's book found in contemporary literature; an alternative and new reading of Graunt's long-lasting contribution to demography, in which Graunt's book is portraited as a model of demography's whole design.

Table 1.1 A chronological periodization of earlier anticipations of two-sex demography

Designation	Period	Names	Description
Period of foundation of demography	**1662-1855**		The Janus-like nature of the sex ratio:
	1662	Graunt	The *Observations* set the model of demography's whole design around the standard concept of population as a statistical aggregate of individuals. Graunt's investigation on population fostered at least four distinctive streams of investigation (see Figure A1.1). His main two-sex concept is the sex ratio regarded as a measure of matters of fact.
	1710	Arbuthnot	'An argument for Divine Providence, taken from the constant regularity observed in the births of both sexes'. This paper provided the first test of significance of a statistical hypothesis. So, Arbuthnot set the direction for the use of sex ratio as an explanatory resource in theory construction rather than just a measure of matters of fact.
	1798	Malthus	The passion between the sexes - the *Principle of Population* outlined a demographic approach remarkably different from the innocent approach set by Graunt's *Observations*. Malthus demonstrated that population is more than simple order, patterns and regularity, because it is designed by a twofold power: survivorship (that food is necessary to the existence of man) and sexual reproduction (that the passion between the sexes is necessary and will remain nearly in its present state).
Period when the current concept of fertility (as output) received assent in demographic research	**1800-1889**		
	1853-1876		The 9 international congresses of statistics between 1853 and 1876 were paramount in establishing the procedures of collecting adequate empirical data on fertility and transforming this concept into an operational indicator.
	1855	Guillard	Guillard created the name demography.
	1869	Quételet	An aspiration for a two-sex empirical research - After contributing to the definition of fertility in association to the actual number of live births relative to the number of women of reproductive age, he attempted to expand the analysis for both sexes.
	1884	Böckh	The creation of the net reproduction rate
The neuter versus the one-sex demography: rise and consolidation of the one-sex demography	**1890-1925**		
	1895	Körösi	Monogenous versus bigenous natality: fertility according to the age combination of both parents.
	1907	Kuczynski	The creation of the total fertility rate
	1907-25	Lotka, Sharpe & Dublin	A mathematical model for the neuter stable theory. The one-sex stable population theory. On the true rate of natural increase.
	1929	Thompson	'Population' - a sketch of 'demographic transition'
	1932	Kuczynski	'Fertility and reproduction' - an illustration of the alleged inconsistent results produced by one-sex models.
The birth and the cradle of the two-sex demography	**1917-1948**		
	1917	Knibbs	A pilgrim of a new world in demographic theory. The cradle of the two-sex demography: why Australia? Knibbs's sketch of a two-sex approach on nuptiality: theoretical, formal and empirical; the gamic conditions: 'General theory of protogamic and gamic surfaces'; a 'theory of the probability of marriages in age-groups'
	1946	Vincent	Searching for the *tendance profonde* in monogamic populations
	1947-8	Karmel	'The measurement of reproductivity in relation to the conflict between male and female measures'.
	1948	A. Pollard	'The measurement of reproductivity'
Towards a two-sex approach on fertility	1957-67		The Princeton Fertility Survey
	1973-1995	J. Pollard	'The two-sex problem'; 'Modelling the interaction between the sexes'
		R. Schoen	'The magnitude of marriage attraction'
			Why, when and how should both sexes matter to demographers?

In any case, my paper on Graunt's *Observations* was inspired by my search for the origins of the sex ratio as a two-sex concept and measure relevant for the two-sex demography (see Chapter 2). Moreover, some readers may be somewhat puzzled by the decision to trace the issue of a two-sex demography as far back as the origin and earlier development of demography. This is so because the conventional view on the 'two-sex problem' has maintained that it is enough to trace the origin of two-sex models to Karmel's work of 1947-48 and the like; or at best, to what motivated this subject, the one-sex model in Lotka's stable population theory and its alleged inconsistencies. In turn, others may agree that demographers should not isolate contemporary demography from its earlier development. However, one cannot assume that readers in general will immediately accept that the importance of Graunt's book goes beyond the scope of mortality as, indeed, is generally suggested in conventional demographic teaching. For these reasons, a perusal through my paper on Graunt's *Observations* may help in understanding the very particular view of the history of demography that the two-sex demographic perspective has already motivated in the process of the investigation for this thesis.

The historical overview provided between Chapters 2 and 7 has three main immediate objectives. First, to place the envisaged two-sex demography in the wider context of the development of demographic theory since its birth and earlier growth. Of course, this is a selective historical overview with the objective to identify some of the main figures in the field of key concepts that are relevant for the debate on the two-sex demography. Thus, the historical overview, particularly the development of demography between Graunt (1662) and Knibbs (1917) is aimed at placing the envisaged two-sex demography in the wider context of earlier and recent demographic ideas. Secondly, the historical part is expected to provide a background of the development of demographic ideas useful to comprehend what lies ahead. Thirdly, Part I is expected to challenge two important misconceptions. One is the idea that a two-sex demographic approach can adequately be traced to the middle of the twentieth century only, mainly when Karmel provided in 1947-48 the most comprehensive justification of the idea that the one-sex model in classical stable population theory needs to be replaced by a two-sex model. Yet a careful search through the history of demographic theory suggests that the development of demography over more than three hundred years can, perhaps, be seen as a fascinating process of decision-making between alternatives which have either approached and anticipated, or distanced and delayed, the emergence of a two-sex demography.

The second misconception refers to the conjecture that demographers have no alternative to simply borrowing a two-sex approach from elsewhere; or as the cynics like to say, that demography is long on methods but short on ideas. This claim is in itself misleading and short of vision: it implies that methods and models are mindless, rather than part of a broader idea or theory construction; it is unable as well to explain, for instance, why conventional demography teaches Lotka's renewal equation and not Geiger counters; or why mainstream courses and textbooks, in demography and elsewhere, still do not even inform students of the research in mathematical demography called 'two-sex problem'. Beyond that, although cynicism alone can seriously damage the reputation of the conventional demography, it is unlikely that it can expand the ideas of demography. In the end, Part I demonstrates that cynical interpretations of demographic ideas do not survive the scrutiny of the history of demography.

Chapter 5 deals with the passion between the sexes, a concept that Malthus used to highlight the role of sexual reproduction in demographic change as part of his dual principle of population. Malthus's dual principle of population has generated a great deal of controversy and investigation. But between the two postulates that comprise the Malthusian principle of population, only the former has already gathered a great deal of attention and investigation, while the latter continues to be dismissed as romantic and thoroughly erroneous. Like the sex ratio, the passion between the sexes can be seen as an important anticipation of a two-sex demography. The two concepts can also be expected to become important theoretical devices in a two-sex perspective because of their association with the two important mechanisms in demographic change: the sex ratio at birth and the sexual nature of reproduction.

Moreover, Chapter 5 also maintains that contemporary demography should not proceed as if 1798, the year of the publication of *An Essay on the Principle of Population*, had never happened. The issue is closely associated with the new reading of John Graunt's *Observations* which approached population as a statistical aggregate with some order, structure, patterns and regularities but no principle of its own. In turn, Malthus's *Principle of Population* can be said to set the grounds for a departure from the innocent approach set forth by Graunt's *Observations*. One of the most important features in Malthus's demographic approach seems to be the view that population is more than simple order, patterns and regularity, because it is designed by a twofold power: survivorship (that food is necessary) and reproduction (that the passion between the sexes is necessary). In this regard, it may be argued that demographers of today remain rather more Grauntian than they themselves seem to be aware of; indeed, more Grauntian than Malthusian, if by Malthusian one means not the support of population control only but the view that descriptions and predictions are simply not substitutes for explanations. In this context, the envisaged two-sex demography outlined here provides a good opportunity for demographers to consider the need to move beyond current ideas on Malthus's controversial postulate on the passion between the sexes; and more generally, to assess the deeper implications of the reinterpretation of Malthusian principle of population for the growth of demography as a science.

Chapter 6 is the pivotal chapter of the thesis for the importance of not treating fertility as a matter of fact but a concept which has been as much invented as discovered. The fascinating history of the evolution of the demographic concept of fertility is explored in Chapter 6 from a perspective never attempted before. In particular, it is shown that the reason the foundation of demography can be said to have taken about two centuries to outgrow its infancy is not because it lacked a proper name. Not until the end of the eighteenth century, with the first publication of Malthus's book, were the core principles of population stated explicitly. Even Malthus avoided sordid details of sex and fertility, and the emphasis of his analysis was concentrated on the 'couple' and the productivity of marriages rather than on women only. This is clear evidence that by the beginning of the nineteenth century neither the concept of fertility nor the one-sex analytical framework had yet gained assent in demographic research. The decisive period of conceptualization and definition of the fertility concept, particularly the agreement on how to deal with fertility empirically, seems to have only occurred in the series of nine international congresses of statistics held between 1853 and 1876.

The periodization of the evolution of the concept of fertility proposed in Chapter 6 provides grounds for the debate, in Part II, of the central issue of the thesis: When, why and how should both sexes matter to demography?. Readers can find in such an evolution the historical evidence to understand, for instance, why demographers have found so little connection between the search for the ‘determinants of fertility’ and the endeavour to develop useful two-sex models in formal demography. In particular, it is shown that three demographic meanings can be said to be embodied by the demographic concept of fertility: fecundity, fertility output, and fertility outcome. Curiously, these three conceptual bifurcations have developed over a long historical process roughly separated by about three hundred years each.

Chapter 7 closes Part II with the most important anticipation of a two-sex demography, which can be found in Knibbs’s (1917) *Mathematical Theory of Population*. Following Quételet and Körösi, Knibbs considered more fully and completely the possibility and feasibility of taking into consideration the role of both sexes in studies of population. It is in his work published in 1917 that the first two-sex theory of the probability of marriages in age-groups can be found. In a certain way, this finding spoils the canonical view that the two-sex problem can be just traced to the two-sex modelling as they are perceived in what came to be known as the two-sex problem. But although Knibbs did not advance much into a conceptualization of fertility in the way proposed in Chapter 6, his *Mathematical Theory of Population* clearly anticipates at least the necessary condition for a two-sex demography: that for certain purposes the methodological frameworks should explicitly take into consideration the numbers and behaviour of both males and females. Moreover, Knibbs’s invention of the concept of ‘marriage function’ can be seen as an ingenious way of resuming the centrality of marriage once so cherished by authors like Graunt and Malthus. Indeed, the notions of marriage and mating functions are indispensable for the transformation of a relatively abstract concept such as the 'passion between the sexes' into operation indicators of the interaction between the sexes. Seen as a function, Knibbs seems to have been the first to make marriage an object of modelling, a development that some decades later would lead to two-sex modelling.

Placing two-sex demography in the context of philosophy of science

Realism has usually provided the backdrop for demographic analysis; not only in contemporary times, but in all ages since John Graunt first estimated and described London's population by several different methods. Despite this, there is a snag whenever one tries to learn about realism from demographers' own advice. Overall, demographers shy away from any attempt to explain what they actually mean by realism. To bother about what it means by realism in demographic research seems too boring for some; too inconsistent with the commonsense conception of realism for others; and still for others a matter that is beyond the scope of demography. In short, demographers take realism as given and natural; indeed, many of them seem to have internalized the constructions of their own imagination so deeply that they treat them not as creations of their thoughts but as demographic reality itself.

Fortunately there are a few saving uncharacteristic forays into realism in demography. The quotation that follows provides one of the best acknowledgments that demographers of different persuasions perceive realism in a bewildering variety of senses.

> Demographers are more closely tied to the real world. They believe that most propositions - or at least important parts of them - can be quantified, and that, once this is done, tests of validity can be devised. This is not always right, and is a weakness as well as strength, but it does prevent too many flights of fancy and it makes most demographers intelligible to others. Demographers, even now when they have been released from their bondage to masses of aggregate data, feel that only phenomena that occur on a sizable scale are significant ... In some ways demographers represent nineteenth century down-to-earthness in contrast to the ideologies of the later twentieth century. They are the inheritors of the positivism of the previous century ... Demographers do have a quantitative Achilles' heel, but this is neither their feeling that quantification helps make phenomena understandable nor their suspicion that the numbers presented may not be the real quantities. Their real weakness is that they often confuse statistical categories with underlying social reality (Caldwell, 1994: 9-10).

These remarks depict the difficulty to deal with the question of realism in demography. First, in a more recent paper Caldwell wrote: 'It is the approach that defines the demographer'; or 'demographers are defined by their attitude, and, to a lesser degree, their methods' (Caldwell, 1996: 328). However, just as in the quote above, Caldwell seems unapologetically biased towards one, or better two, very naive senses of realism, namely positivism and empiricism. This is how one can understand his contention as to demographers' down-to-earthness than other social scientists. Secondly, Caldwell refers to demographers as if they constituted a monolithic community and, indeed, as if they were not bedevilled by a variety of philosophical approaches. In spite of that, he considers that the 'real weakness' of (most) demographers is that they often confuse statistical categories with underlying social reality. In his recent paper wondering whether demography is in fact a social science, Caldwell lamented: 'Unfortunately, demographers are prone to conclude that a phenomenon does not exist if they can find no satisfactory way to measure it' (Caldwell, 1996: 331). Yet, is there any better direction to a stronger realism that empiricism and positivism have offered? If there is, Caldwell does not point it out in his writings; nor does he make it explicit if what he designated as 'ideologies of the later twentieth century' include what here I call cultural relativism.

The issue of realism is of particular relevance for the subject matter tackled in this thesis. The notion of realism chosen for this work draws on the philosophical doctrine of realism as the view that reality is objective and logically independent of all beliefs and conceptions of it (Trigg, 1985: 211). As Trigg put it in an earlier work:

> We cannot talk or think about reality without talking or thinking about it. This is a truism which seems almost too boring to bother about. We cannot have a conception of something without employing the conceptual scheme we have at our disposal. Yet this obvious point very often provides the starting point for major philosophical doctrines about the relationship between thought and reality, between what there is and what we think there is (Trigg, 1980: 1).

By the evidence provided in the thesis, there is little doubt that previous attempts to develop two-sex concepts, measures and models in demography have been motivated by the desire of some scholars to be more realistic than the neuter and one-sex approaches allow. One of the latest statements on this can be found in Pollard's (1995) recent paper, in which he contended that the sporadic bursts of interest in the two-sex modelling are aimed at providing more realistic models for demographic analysis.

Curiously, the aspiration for more realism that has inspired the two-sex modelling has been met with a widespread disdain justified in terms of realism as well. Can demographers who are adept of philosophical approaches, such as empiricism, positivism, constructivist, idealism, or relativism be all equally and truly realistic? Is the endeavour to develop two-sex models a flight of fancy into the unintelligible? If so, is it because the apparatus required by two-sex models is necessarily cumbersome? Or, are two-sex models indispensable to making several aspects of demographic reality not directly experienced and observed by people yet more intelligible than the neuter and one-sex models allow?

In the end, the answers to these questions depend on the philosophical approach demographers embrace and, in particular, their approaches on the notion of realism. It needs to be admitted that over the years, as the term 'realism' has provided the backdrop for demography, it has also become a blanket concept that demographers conveniently use to cover up the philosophical and intellectual influences on their own approaches to population reality. Although realism entails a philosophical position, the majority of demographers prefer to shrug their shoulders and say that it does not really matter what one thinks philosophically. In turn, as it has already been stated above I accept not only the pivotal role of theory but also the dualism between population reality and demographic theorizing.[6]

However evident the concept of population may at first glance appear, demographers cannot explain what the reality of population is, independent of theory: that is, without an elegant body of analysis that makes it possible to describe and explain a certain class of observations, as well as to generalize from them and predict new observations. In the thesis I maintain that the overall content of demographic theory can be classified in three main analytical bodies; each of them is said to have developed around a fundamental theoretical principle and to display its own array of concepts, measures methods and specific theories. The whole point of being realistic in demography and, elsewhere in science, depends on whether one conceives of population reality without ever making it dependent on thought, concepts, measures, methods or theories. But rather than defending that the existence of population depends on theory, my view is that demographers' own knowledge on population depends on the adequacy and quality of their theories.

Thus, Part II moves from an historical approach to a more in-depth logical discussion on the broad theoretical framework for the envisaged two-sex demography. At issue in the remaining chapter is the central question of the thesis: when, why and how should both sexes matter to demography? Chapter 8 proposes a reconceptualization of the demographic concept of 'population composition', one that seems more suitable for a two-sex demography. Moreover, Chapter 8 places the principle of complementarity between the sexes in the wider context of the principles that preceded it and constitute the basis of mainstream demography. Hereafter the whole discussion in the thesis stands on a three-dimensional epistemological set of principles: differentiation-separation-complementarity (DSC). Together, these three principles support the content and issues that may be known as demographic phenomena. Simultaneously, to the DSC setting corresponds a threefold methodological approach that I shall call the neuter-one-two-sex methodology. This is consistent with the view that the overall content of demographic theory can be classified in terms of the core characteristic in demographic reproduction, sexuality. In

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[6] I believe that the present work contains enough material for any reader to consider the philosophical issues that are necessary to be discussed, and why I put the emphasis on them. This was not the opinion of one of the examiners of the thesis, who chided me for not being more philosophical but declined to comment about the main subject matter of the thesis. Although he excused himself that the other examiners were better prepared to comment about the envisaged two-sex demography, the examiner's report indicated that he simply did not find a better way to challenge the unifying theme of this work than avoid it. In particular, he was confronted with a compelling evidence that demographers do not really need to behave as if their field was totally deprived of intellectual resources. 'Unfortunately', the examiner lamented, 'like Wunsch's paper, this approach assumes a definition of demography that completely ignores the multi-disciplinary nature of the subject and its history'. But to claim that demography is a multi-disciplinary, rather than a relatively independent subject matter with wide interdisciplinary links, is the same as acknowledging no proper demographic subject at all. In the end, the examiner decided to concentrate on two aspects only. First, an appendix which he proposed to be removed because 'much of it [was] devoted to materials not germaine to the focus of a thesis on 'two-sex demography''. Secondly, broad philosophical and historical issues as if the key objective of the thesis was to offer an assessment of influential philosophies of science and philosophies of history in the development of demography.

demographic terms the sexual nature of population determines its structure and organization, as well as its growth and evolution. Hence, in this thesis the use of the category ‘sex’ is expanded from its traditional role as a standard variable to a standard device, both epistemological and methodological, in demographic theory construction.

Chapter 9 focuses on 'neuter demography and demographic outputs' and draws on the principle of absolute differentiation among individuals. Whatever similarities may exist among people, individuals in a population can all be regarded as different in behaviour and physical appearance. As long as individuals are seen as unique and independent entities within the whole demographic aggregate, population is thus studied in disregard even of age, sexuality and mating relations. More than three hundred years ago, John Graunt set the model of demography's whole design around this standard concept of population. The legacy of such a model remains remarkably powerful today, to the extent that many demographers still believe they should be committed only to the naive seeing and the dispassionate noting and accounting inherited from Graunt's *Observations*. But Chapter 9 characterizes the kernel and domain of validity of a neuter demography, stressing in particular its strengths and limitations. For that purpose I concentrate on just two of the most important strands within a neuter demography: the centrality of Lotka's neuter stable population theory in demography in general, and the importance of the demographic transition theory in the scientific study of population theory.

Chapter 10 focuses on the most successful and dominant body of analysis in twentieth century demography: the 'one-sex demography and fertility outputs'. As with regard to the ‘neuter’ approach, Chapter 10 discusses the limitations and strengths of the ‘one-sex approach’ as a body of analysis that makes it possible to describe certain classes of observations, as well as to generalize from them and predict new observations. The core principle behind the one-sex demography is the strict separation of the sexes, which is consistent with the idea that in reproduction there are not one but two demographic natures: male and female. For about two hundred years demographers have been increasingly aware that although the neuter approach captures a certain demographic reality and provides immediate and useful results, its domain of validity is limited; it can often lead to misleading interpretations because it does not take into account the demographic structure of population. So, a body of demographic theory has grown around the view that the number of births, deaths and moves is determined and determines population structure defined by sex and age. Methodologically, the one-sex demography relies on a one-sex approach and places fertility at the centre of its analysis. In this context, the creation of the demographic concept of fertility may have been the most significant breakthrough since the birth of demography.

Chapter 11 focuses on 'two-sex demography and demographic outcomes'. The two main conditions for a two-sex approach are discussed: the necessary versus the sufficient. It is shown that a concept and a measure, as well as a model and a theory, can be said to be of a two-sex nature as long as they deal with the demographic phenomena determined by complementarity between the sexes as demographic outcomes. The concept of demographic outcome, the core working concept derived from the principle of complementarity between the sexes, is compared with the concept of demographic outputs. Moreover, the preliminary definition of two-sex demography given in the Introduction is elaborated and detailed further. Then, the two main components of the definition of two-sex demography are placed in the context of the twentieth-century literature directly relevant for the development of a two-sex demography. Two areas in demographic research are reviewed: first, the literature that for about half a century has explicitly aimed at developing two-sex models in association with the so-called 'two-sex problem'; secondly, the literature which during the same period has identified and attempted to explain the causes and mechanisms of demographic change; this literature corresponds mostly to what has become known as the 'determinants of fertility'. These two areas of demographic research have their own strengths and limitations for the two conditions considered important for a systematic two-sex approach. But without a clear understanding of their strengths and shortcomings one cannot comprehend what lies ahead in terms of the possible development of a two-sex demography in the future. The review provided in the last two sections drew on the chronological bibliography found in Appendix B.

A critical discussion of the strengths and limitations found in the two areas of demographic research considered indispensable for the envisaged two-sex demography should shed light for further work. Following the overall discussion in the thesis Chapter 11 demonstrates that demographers can and should aspire to the creation of a two-sex demography, not with the expectation that the set of principles that preceded it will die or simply fade away.

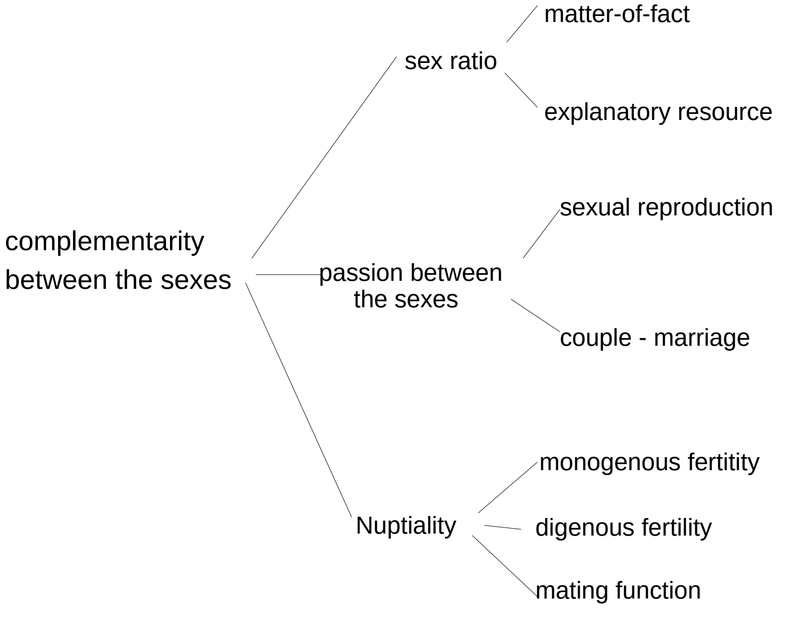
The two-sex demography should be seen as part of the development of demographic science in general, and in particular a manifestation of growth and transformation of a basically descriptive scientific study of population into a mature explanatory demography. In these terms, the two-sex demography is expected to expand the domain of validity of the dominant principles in conventional demography. It can provide a new theoretical basis for a more adequate debate of several controversial issues, such as the widely accepted idea that demography lacks an adequate theory of fertility determinants; and the so-called ‘two-sex problem’. One sentence conveys the vision put forward by this thesis on the envisaged two-sex demography. There may not be a better way to decide on the feasibility, validity and usefulness of a two-sex demography than discussing the central issue raised by this thesis: when, why and how the complementarity between the sexes should be taken into consideration by demography.

PART I.

Where was the beginning? What was it?

Guiding the search for earlier anticipations of a two-sex demography is the principle depicted in Figure 1.1 called complementarity between the sexes. This principle is seen in the thesis as the cornerstone of the envisaged two-sex demography; it is expected to provide the criterion to select and bring together the strands from which one can hope to weave a two-sex demography. The chronological periodization of earlier anticipations of the two-sex demography in Table 1.1 supports the logic behind the concepts in Figure 1.1. The concepts and measures reviewed in Part I have been organized in a logically coherent manner to avoid bringing them together haphazardly and thus propose a two-sex demography in an *ad hoc* fashion.

Figure 1.1 A summary of the strands discussed between Chapters 2 and 7



2.

Sex ratio: the Janus-like nature of the simplest two-sex measure

> Where was the beginning?
> How long has it been?
> Is it in each one who seeks Twoness
> And in each two who seek Oneness? (Grigg, 1989)
>
> those who cannot apprehend the reason of these enquiries are unfit to trouble themselves to ask them (Graunt, 1662: 51)

The sex ratio is perhaps the most odd indicator used in demography. Although it was the first measure ever created in our discipline, students of population are never informed of that, to say nothing about when and who did it first; this contrasts sharply, as has already been shown in Part 1, with the references usually made to the origin of mortality measures. Moreover, the sex ratio is so straightforward as a quantitative measure that at first glance it is hard to think that it could ever been used for anything else than an innocent measure of matter of facts, such as the relative number of males and females in a population. Indeed, demographer's common sense seems to have been fooled by all such features which concur to the view that the sex ratio is a very elementary and simple indicators from which little can be extracted.

The main reason the sex ratio is regarded as a core concept and measure in this thesis is because of its consistency with the principle of complementarity. The sex ratio is a relative measure in which neither of its components can be abstracted and left aside: if the numerator were assumed zero, the ratio would turn into zero; and if the denominator were zero, the ratio would become infinite and thus indeterminate. In other words, neither sex constituting a population can be considered more important than the other to the extent of dispensing it; both males and females are equally and simultaneously fundamental for the existence of this indicator.

Throughout the thesis the originality of the sex ratio as compared with other demographic measures will certainly be appreciated better. But, for the time being, it can be advanced that the main difference concerns the fact that in general conventional concepts and measures used in demography either conflate the sexes into seaminess or neuter aggregates, such as the case of crude birth rates which include both sexes in the denominator, or they separate the sexes from one another, as it is the case of fertility and reproduction rates. Conversely, the sex ratio deals with the two sexes in their interaction and thus, by its nature, it neither conflates nor separates males from females. Thus, while the sex ratio is known as relatively simple measure even as a measure of matter of fact it can be used here to illustrate what the two-sex approach discussed here is all about.

Yet, there is another reason why the sex ratio is important here. Demographers, among other scientists, have for long used the sex ratio in a very different objective from simply measuring matters of fact. This role is essentially theoretical, which only for those who are especially keen to show themselves as pragmatic and concerned with 'facts' may seem trivial. Indeed, this face of the sex ratio has generally been dismissed or unnoticed by demographers. As Teitelbaum wrote:

> ... if the live-birth sex ratio is not a constant, as has often been assumed, it must be considered as a possibly significant factor in the reproductivity of a population or part of it. Demographers have not, of course, been blind to this point; for example, the demographic measures *gross reproduction rate* and *net reproduction rate* were devised with the sex-ratio in mind, at least implicitly (Teitelbaum, 1972: 91)

The customary way of using the sex ratio in demography, that is as a measure of matters of fact only, certainly helps to feed the illusion among some researchers that they can explain what population reality is independent of theory. In part, this ideal love affair between researchers and the natural phenomena is reminiscent of earlier modern science, that is the time when the scientific views of natural philosophers became highly influential. As Boyle wrote in a letter to a friend:

> The other humane studies I apply myself to are natural philosophy, the mechanics and husbandry, according to the principles of our new philosophical colledge [sic.] that values no knowledge but *as it hath a tendency to use*. And therefore I shall make it one of our suits to you that you should take the pains to enquire a little more thoroughly into the ways of husbandry ... which will make you extremely welcome to our invisible colledge [sic.] (cited by Hogben, 1938: 15).

Echoes of the distrust of knowledge as opposed to the 'tendency to use' continue to be felt nowadays in several sciences, including demography. For this reason, it can easily be understood that the role of the sex ratio as a device in a specific theoretical model has generally been dismissed.

The Janus-like nature of the sex ratio in demography is discussed in this chapter and Chapter 2. Beneath this discussion is the idea that just as Janus, the ancient Roman deity that presided over doors and gates and over beginnings and endings, was represented with two faces looking in opposite directions, the sex ratio presides over beginnings and endings and the axioms and outcomes of demographic models and theories. Both faces of the sex ratio are as important in demography. On the one hand, the sex ratio can and should continue to be seen as a measure of matters of fact; in this respect, the sex ratio is simply the proportion of the number of one sex to that of the other. On the other hand, the other face of the sex ratio that needs attention is as an explanatory resource, or an algorithmic process in the way proposed by Dennett (1995b) (see Chapter 4).

This type of discussion resembles, somehow, the increasingly popular deconstructionist approaches. However, the aim of this section is not deconstructionist, at least in the sense that the sex ratio could be scrutinized with the objective of finding their conflicting and subtle meanings.

In a time when deconstructionist theories seem to have increased the intolerance of several scholars with regard to the things scientists have often taken for granted, even a short comment needs to made. In recent years some authors have already taken issue with certain concepts used in demography in a perspective that can, in fact, be said to be deconstructionist. For instance, the view that the sex ratio is an innocent measure of the proportion of the number of one sex to that of the other has been challenged. In particular, the aftermath of the sex ratio, namely the so-called masculinity and femininity ratios, have been questioned. Especially the term 'masculinity ratio', which displays males in the numerator and females in the denominator, as been portrayed as an insidiously androcentric concept. Watkins, in a discussion of the things that demographers take for granted, asserts: 'Typically but not invariably, tables in *Demography* show men on top, women on bottom (as in the sex ratio, sometimes called the "masculinity ratio")' (Watkins, 1993: 553).

Other authors have gone even further, and consider the sex ratio too slanted towards the sin of the biological and, thus, unclear with regard to the role of cultural differences. According to Lucas, this has led Australian feminists to propose replacing the term 'sex ratio' by ‘gender ratio' (Matthews, 1984, cited by Lucas, 1985: 7). Disturbingly, despite Lucas's advice that demographers should not encourage this sort of bizarre alternative, the number of demographers who are avoiding the term ‘sex’ in their papers altogether is increasing.

In short, however interesting the search for conflicting meanings and hidden bias in the language of communication used in science, the above two examples show how a challenge of basic assumptions and central concepts can easily become distracting and misleading. As has been asserted in Chapter 2, metaphors and allegories are important devices for communicating and expressing abstract concepts. However, as in this case, to reduce a critique of the role of the sex ratio to the subtle connotations that can be found in the 'top-bottom' representations of the sexes is distracting; especially, because one misses the opportunity to go beyond the placid surface of the matter and try to identify the essential features that need to be taken into consideration in demographic theory construction.

The failure of demographers to recognize the two theoretical dimensions of the sex ratio in demographic theorizing appears to have two main reasons. First, those who usually make decisions on demographic teaching seem to have believed that Graunt's investigation on the sex ratio was too elementary and not important enough to deserve any special emphasis in demographic teaching. For example, although Greenwood (1948: 35) acknowledged that Graunt's findings on the sex ratio were among 'the most famous of all his deductions', he did not 'rank this section high among Graunt's researches'. Glass also wrote about this section of Graunt's investigation as 'his best-known contribution', but he agreed 'with Greenwood that, in itself, this is less stimulating than some other observations of Graunt'. The remarks of these two authors are interesting, especially in the context of the intellectual chain-reaction which Graunt's investigations provoked, of which Greenwood and Glass were well aware.

Secondly, the type of discussion of the sex ratio and its aftermath offered by Matthews (see Lucas, 1985: 7) and Watkins (1993), can hardly convince the average demographer that there is more to them than meets the eye. At best, they show that Greenwood and Glass underestimated the stimulating potential of the sex ratio; but just as a chalk, rather than writing, sometimes just scratches the blackboard, the existing deconstructionist alternatives have so far provided more anxiety than new insights.

There is certainly a need for a detailed discussion concerning the validity of the terms ‘sex’ and ‘gender’ in demography, though it will be left for another occasion. In part, the challenge of the limited perception of the sex ratio in demography should prepare the ground for such a debate. This thesis makes no apology for using both the terms sex and gender; I believe that the term sex instead of being excluded and replaced should be complemented by the term gender. This belief is drawn from the hope that in future most demographers will be aware and understand that both terms can be equally valid and useful for them in their work.

Graunt's discovery of the statistical stability of the sexes

When Graunt wrote his book there were obviously no demographic concepts as such; otherwise the *Observations* would not be seen today as the 'big bang' of the scientific study of population. The concepts used by Graunt in his investigation were concepts drawn from the common language; for example, christenings and births, burials and deaths, the number of inhabitants, the teeming-women, conceptions, and marriages. These were the primitive and earlier conceptual tools of demography; they became the seeds of the set of categories which have gradually made up the content of the population body of theory.

Among the concepts that Graunt used, the sex ratio was clearly the most persistent and had long-lasting implications. He did not call it sex ratio, but the term 'proportion between the number of males and females' clearly corresponds to its meaning. In the whole text of the *Observations* there is no other concept used so extensively; nor, which is probably even more important, is there a concept which was so successfully transformed into an operational definition and measurable indicator. To recall the logic of demographic discovery as proposed by Wunsch (see Annex A), the sex ratio is perhaps the best example of a demographic concept, which is still used nowadays, that Graunt succeeded in transforming from an abstract concept into an observational indicator.[1] This is the reason I consider that the sex ratio should be explicitly acknowledged as the first true demographic measure ever used in more then three hundred years of demography.

Before the publication of Graunt's *Observations* people had expressed intuitive guesses about the regularities between the sexes; but these were just qualitative guesses based on personal and subjective observations. Graunt was the first to apply elementary mathematics to an independent and relatively large data set with the objective of producing a solid knowledge about the differences between males and females from birth to death. The results he presented in his book were very much a surprise; at the time the common view held was that the population ratio was about three women to one man (Graunt, 1962: 46). Graunt's discussion on the sex ratio can be found mainly in Chapters 3, 7, 8, 12 and the annexed tables at the end of the *Observations*. Table 1.2.1 summarizes the data from annexed tables in Graunt's book and highlights the two important features in Graunt's investigation.

Table 1.2.1 Summary of the sex ratios in Graunt's *Observations*, 1662

	Christenings				Burials			
	Sex ratio	Maximum	Minimum	Ratio	Sex ratio	Maximum	Minimum	Ratio
London (1629-1636)	107	10034	8524	1.2	111	23359	8392	2.8
London (1637-1640)	107	10850	9160	1.2	115	13624	9862	1.4
London (1541-1648)	106	10670	6544	1.6	109	14059	9894	1.4
London (1649-1656)	110	7050	5612	1.3	111	13921	8764	1.6
London (1657-1664)	110	11722	5990	2.0	109	19735	12434	1.6
Total	108				111			
Romsey (1569-1578)	100	70	50	1.4	97	66	34	1.9
Romsey (1579-1588)	106	90	45	2.0	96	87	39	2.2
Romsey (1589-1598)	125	71	48	1.5	97	117	38	3.1
Romsey (1599-1608)	97	93	60	1.6	103	71	30	2.4
Romsey (1609-1618)	117	87	61	1.4	99	116	51	2.3
Romsey (1619-1628)	99	85	63	1.3	89	89	46	1.9
Romsey (1629-1638)	101	103	66	1.6	93	156	29	5.4
Romsey (1639-1648)	98	87	55	1.6	91	137	46	3.0
Romsey (1649-1658)	111	86	52	1.7	87	80	28	2.9
Total	105				94			
Tiverton (1560-1664)	101				91			91.7
Cranbrooke 1564-1649	105				104			12.3
Total parishes	103				95			
Total Country	108				110			

Source: Graunt's *Observations*, 5th edition, 1665, pp. 411-421.
(a) The maximum, minimum and ratios for Romsey are different from those given by Hald's (1990: 94) Table; when the original table is checked it seems that Hald did not pick up the correct numbers; for example, for christenings in 1589-1598 the minimum is 48 instead of 52; in 1639-1648 the minimum is 55 instead of 62; for burials in 1599-1608 the maximum is 71 instead of 53; and in 1619-1628 the minimum is 46 instead of 50.
(b) The age span in the case of London is based on the original tables and thus differs from those created by Hald (1990: 94)

[1] As Figure 1 shows, Graunt managed to transform several other concepts into relatively measurable indicators. However, while it cannot be said that he grasped the notion of crude death and birth rates, or fertility rates, he explored the quantitative dimensions of the concept of 'proportion between males and females' in ways that remain almost completely unchanged.

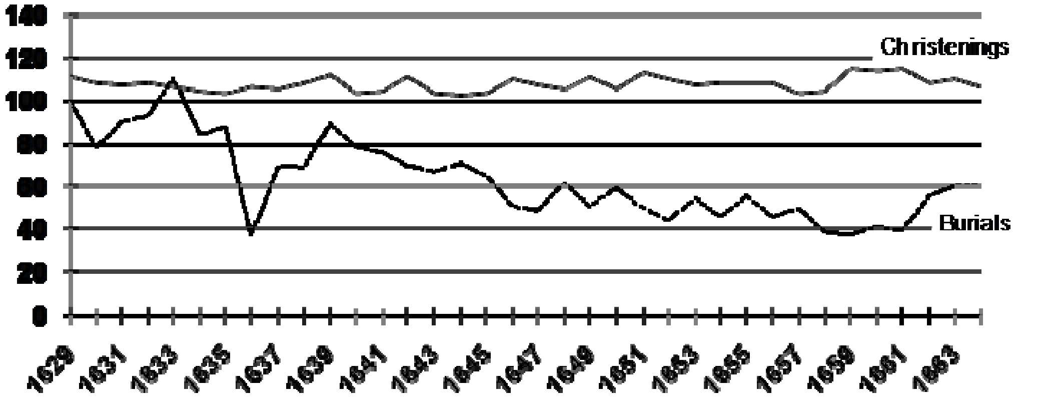
The first feature observed by Graunt was the statistical stability of the number of males' and females' births and deaths; he noticed, in particular, the relative excess of males' over females' births and deaths as well as the approximate numerical equality of the sexes throughout their life.

Graunt did not represent graphically; but on reading the *Observations* one gets the impression that its author analysed the data so carefully and in such detail that he might have sketched in his mind the image presented in Figure 1.2.1. Curiously, in contrast to Graunt's life table to my knowledge nobody seems to have ever depicted graphically Graunt's data on the sex ratio.[2]

The second important feature in Table 1.2.1 refers to Graunt' s considerations on the greatest and least yearly numbers of burials and christenings in London. As Hald (1990: 94) noted recently, 'It is remarkable that Graunt also investigates the stability of the fluctuations in the time series of christenings and burials. For Romsey he has grouped his data in decades'. Graunt did not tabulate the ratios as presented in Table 1.2.1, but he used some examples in his discussion. In Chapter 12, some of the greatest and least numbers are used (see the 'Index of propositions ...' in annex, paragraph 93) in a comparison between the patterns of the sex ratio in Romsey and London.

[2] Graphical representation of factual data, as distinct from graphical plotting of mathematical functions, was not a method of presenting data until Graunt's *Observations* first motivated Huygens to draw a graph of Graunt's life table (Hald, 1990: 109). Thus, if Graunt's *Observations* was the cradle of statistics Huygens's graph might have been the first graphical representation of statistical data. It needs to be said, though, that the principle of coordinates to determine the location of a point in space dates to the ancient Greeks at least; the use of a coordinate system as a field of operation for the study of curved lines occurs in the Middle Ages; analytic geometry was invented by Descartes in 1637; for more on the origin of graphic representation see Funkhouser (1936), Royston (1970).

Figure 1.2.1 Graunt's sex ratios for christenings and burials in London from 1629 to 1664



Graunt pointed out that in Romsey the ratios between maximum and minimum burials were generally above 2, whereas for christenings they are below 2. In his comparison with London he wrote: 'It follows that the proportion between the greatest and the least mortalities in the country are far greater than at London' (p. 49); besides, in London

> the number of burials upon other accounts within no decade of years hath been double, whereas in the country it hath been quintuple not only within the whole 90 years, but also within the same decade: for in the year 1633 there died but 29, and in the year 1638 the above-mentioned number of 156. Moreover, as in London, in no decade, the burials of one year are double to those of another: so in the country they are seldom not more than so. As by this Table appears,

	Number of burials	
Decade	greatest	least
1	66	34
2	87	39
3	117	38
4	53	30
5	116	51
6	89	50
7	156	35
8	137	46
9	80	28

(Graunt, 1662: 49)

Graunt noticed more substantial biases towards males in London than in the country parishes of Romsey. For the period 1629-1664 he found for London 156,740 males and 145,191 females christened, or 108 males to 100 females. In terms of burials he found 239,433 burials of males to 216,654 of females or 111 to 100. In Romsey, a parish in the country, in the period 1569-1658 Graunt estimated 3256 males and 3093 females christened, that is a sex ratio of 105 males to 100 females; in terms of burials he estimated 2167 males to 2300 females. In two other country parishes Graunt found in Tiverton, for the period 1560-1664, a sex ratio at birth of 101, and 91 males to 100 females at death; and in Cranbrook, for the period 1560-1649 a sex ratio of 105 males to 100 females christened, and 104 males to 100 females at death.

Most of these observations have been scrutinized by several authors and, as Greenwood wrote (1948: 34), were sometimes 'severely criticized'. At issue has been the fact that Graunt did not grasp the effect of the age structure in his comparisons of births and deaths for both sexes and his considerations upon the mortality differentials in the country and in London (Hull, 1899: lxxvii; Greenwood, 1948: 33-34; Glass, 1963: 9-14; Sutherland, 1963a: 548-549; Hald, 1990: 95).

Hull even considered Graunt's assertion of the regional differentials in terms of the fresher air in the country than in London as 'the greatest statistical mistake that can be charged against Graunt' (Hull, 1899: lxxvii). However, apparently only Greenwood (1948: 34) and Hald (1990: 95) have discussed the limitations of Graunt's conjectures with specific and detailed statistical arguments; they both considered the variations of the ratios for christenings and burials in terms of coefficients of variation and Poisson distributions. On these grounds Greenwood inferred:

> Prof. Hull was wrong in supposing that the wide range in the Romsey rates was due to the narrowness of the field of observation in a statistical sense ... Something more than small numbers is involved. Still, it must be confessed that Graunt did not anticipate the reasoning of James Bernoulli, although an intuition of genius may have led him to think that something more than 'chance' had play here (Greenwood, 1948: 34)

In turn, Hald admitted that Graunt understood 'that trends are more easily discernible from totals than from individual years, but he has no clear conception of the relation between the size of random variation and the size of the sample'. In his comment on Graunt's numbers Hald asserted that 'the variation of the ratios for christenings is not much larger than for Poisson processes, whereas the ratios for the burials vary considerably more'. This can be explained, Hald continued, by the fact that 'the occurrence of epidemics creates a larger variation in the deaths than in the births'. Since the population increase in London was estimated to be at a rate of 18 per cent per decade, this factor combined with the coefficient of variation of 1.00 per cent for burials and 1.13 per cent for christenings, can only explain the variations of the ratios for the latter but not for the former. And thus Hald concluded:

> It is, however, clear that the larger random variation of the yearly numbers in Romsey plays a large role in explaining that the ratios in Romsey are larger than in London (Hald, 1990: 95)

This is as far as Graunt's first analysis on the statistical regularities of sex ratios can go. Regardless of its limitations, being the first statistical endeavour Graunt's achievement is far from small. Although this sort of statistical study is taken for granted in modern times, Graunt was the first to make in a study of a specific demographic phenomenon. To play down the significance of this achievement is misleading. Greenwood was correct to dismiss the validity of Graunt's conjecture that the reduction of the male excess he described could be due to the judicial hangings and the Fellows of Colleges who remained unmarried.

Despite this misinterpretation, one should not miss the significance of the investigation of the sex ratio as a matter of fact. Several authors have praised Graunt's achievement for the fact that he discovered the approximate numerical equality of the sexes and the excess of males over females at birth; but Hald (1990: 93) seems to have been among the few, if not the only one, to place due emphasis on the fact that Graunt also anticipated the study of statistical variations between the maxima and minima.

Graunt's speculations on the sex ratio

Seen from the distance of more than three centuries, Graunt's discoveries on the sex ratio emerge as the first significant empirical investigation ever attempted from the point of view of social sciences. No wonder that his discovery that males tend to exceed females at birth has never achieved the same canonical reputation in the history of science and in academic teaching as, for instance, Boyle's air-pump experiments;[3] as time has passed, and particularly in modern times, scientists appear to be in a position to appreciate the scientific implications of the way Graunt used the experimental method embraced by the English Royal Society and applied it to investigate social phenomena.

Beyond the statistical significance of Graunt's investigation his interpretations and speculations on the stability of the sex ratio provide an interesting background to the perceptions of migration, fertility, marriage, and sexuality in his time.

In his assessment of the data, Graunt did not admit that males predominated in London because the City was the 'great stage and shop of business, wherein the masculine sex bears the greatest part' (p. 38). As Pearson (1978: 35) observed, 'He cites the christenings and says practically that we have to deal with a natural phenomenon'. Table 1.2.1 shows that those who were born and christened in the country did not die there, whereas in London there was an excess of male burials over male christenings; that is, a larger proportion of males were buried than were born there. This led Graunt to comment on the constant flow of migration towards the City and conclude that the burials in general exceeded the christenings. Moreover, he insisted that the migration from the country to London, predominantly of males, made up for the deaths due to plague and also war.

Before divagating much further on the causes of his findings, Graunt passed on to say that 'we shall desire that travellers would enquire whether it be the same in other

[3] About Boyle's research in pneumatics and his employment of the air-pump in that enterprise see Shapin and Schaffer (1985).

countries' (p. 38). He showed little doubt that things could be much different elsewhere, though he longed for more empirical confirmation and admitted that there was a need for an account of how, in every age, the proportions of the sex ratio changed. However, probably because such a task would imply an additional 'laborious buzzling and groping' (p. 51) through his data, he passed on to draw his interpretations on the phenomenon investigated.

First, Graunt used the statistically demonstrated approximate equality of the sexes for his strong support for the Christian prohibition of polygamy. As he wrote, it

> is more agreeable to the Law of Nature, that is, the Law of God, than Mahometism, and others, that allow it; for one man his having many women, or wives, by Law, signifies nothing, unless there were many women to one man in Nature also (p. 39).

By implication, God did not intend men to have several wives, otherwise there should be more women than men. But why should there be more males than females at birth, though throughout life one woman should have just one man? Here Graunt expresses his own views on fertility, marriage and sexuality.

His defence of the institution of monogamy as opposed to polygamy may appear extravagant for the reader of modern times. But once again, rather than vilifying his personal views it is perhaps more important to notice that his argumentation was sound and logically convincing. Graunt admitted the possible objection 'that one horse, bull, or ram, having each of them many females, do promote increase' (p. 39). But, to this, he responded 'that although perhaps there be naturally, even of these species, more males than females, yet artificially, that is, by making geldings, oxen and wethers, there are fewer' (p. 39). Thus, by experience people learn

> how many ewes (suppose twenty) one ram will serve, we may know what proportion of male lambs to castrate, or geld, viz. nineteen, or thereabouts: for if you emasculate fewer, viz. but ten, you shall by promiscuous copulation of each of those ten with two females hinder the increase so far as the admittance of two males will to it: but, if you castrate none at all, it is highly probable that every of the twenty males copulating with every of the twenty females, there will be little or no conception in any of them all (p. 39).

In other words, to Graunt the fertility level in animal as well as in human populations was inversely correlated with the level of female sexual promiscuity.

> And this I take to be the truest reason why foxes, wolves, and other vermin animals that are not gelt, increase not faster than sheep, when as so many thousands of these are daily butchered, and very few of the other die otherwise than of themselves (p. 39).

But why then more males than females at birth, namely that the former exceeds the latter 'by about a thirteenth part'?

> so that although more men die violent deaths than women, that is, more are slain in wars, killed by mischance, drowned at sea, and die by the hand of Justice. Moreover, more men go to colonies and travel into forein parts, than women. And lastly, more remain unmarried, than of women, as Fellows of Colleges, and apprentices above eighteen, etc. Yet the said thirteenth part difference bringeth the

> business but to such a pass, that every woman may have an husband, without the allowance of polygamy (p. 39).

He insisted further that 'although a man be prolific forty years, and a woman but five and twenty, which makes the males to be as 560 to 325 females, yet the causes above-named, and the later marriage of men, reduced all to an equality' (p. 39).

Moreover, while Graunt disapproved of sexual intercourse outside the marriage in the case of strictly monogamous societies, for those places where polygamy was allowed he saw a reason of making eunuchs. As he put it, polygamy is

> useless as to multiplication without the former, as was said before in the case of sheep and other animals, usually gelt in these countries ... By consequence, this practice of castration serves as well to promote increase as to meliorate the flesh of those beasts that suffer it ... In Popish countries, where polygamy is forbidden, if a greater number of males oblige themselves to celibate, than the natural overplus, or difference between them and females amounts unto; then multiplication is hindered: for if there be eight men to ten women, all of which eight men are married to eight of the ten women, then the other two bear no children, as either admitting no man at all, or else admitting men as whores (that is, more than one) which commonly procreates no more than if none at all had been used: or else such unlawful copulations beget conceptions, but to frustrate them by procured abortions or secret murders all which returns to the same reckoning. Now, if the same proportion of women oblige themselves to a single life likewise, then such obligation makes no change in this matter of increase (p. 40-41).

This conveys the impression that Graunt understood male celibacy as the threshold of human reproduction in a monogamous population. According to his argument the level of male celibacy determines the number of women who will not bear children and, more specifically, the sex ratio represents 'the true *ratio formalis* of the evil of adulteries and fornications' (p. 11). This is so because outside the conventional monogamous marriage there is no place for children, nor even opportunity for sexual intercourse.

From what has been said Graunt saw it as sufficient to justify that 'the Law is and ought to be so strict against fornications and adulteries, for if there were universal liberty, the increase of mankind would be but like of foxes at best' (p. 41). But, still, 'why although in the country the christenings exceed the burials, yet in London they do not' (p. 37). Graunt answered this question in Chapter 7:

> The general reason of this must be that in London the proportion of those subject to die unto those capable of breeding is greater than in the Country ... London is more unhealthful, or that it inclines men and women more to barrenness, than the country, which by comparing the burials and christenings of Hackney, Newington, and the other country-parishes, with the most smoky and stinking parts of the City, is scarce discernible in any considerable degree (Graunt, p. 37).

The fact that London showed proportionally fewer breeders than the country was attributed to the following factors: first, immigration to London was predominantly male: 'All that have business ... and all countrymen coming up to bring provisions to the City or to buy foreign commodities ... do for the most part leave their wives in the country'. Secondly, 'Persons coming to live in London out of curiosity and pleasure, as also such as would retire and live privately, do the same if they have any' (p. 37). Thirdly, 'Such as come up to be cured of diseases, do scarce use their wives *pro tempore*'. Fourth, 'many apprentices of London, who are bound seven or nine years from marriage, do often stay longer voluntarily' (p. 37). Fifth, 'many seamen of London used to leave their wives behind them, who are more subject to die in the absence of their husbands than to breed either without men or with the use of many promiscuously'. As yet, the city appears increasingly more unhealthful than the country: 'new-comers and children' are exposed to smoke, stinks, and close air less healthful than that of the country; otherwise', Graunt asks, 'why do sickly persons remove into the country air? And why are there more old men in the country than in London, *pro rata?'*.

Finally, 'As to the causes of Barrenness in *London*', Graunt added:

> although there should be none extraordinary in the native air of the place, yet the intemperance in feeding, and especially the adulteries and fornications, supposed more frequent in London than elsewhere, do certainly hinder breeding. For a woman, admitting 10 men, is so far from having ten times as many children, that she hath none at all ... Add to this, that the minds of men in London are more thoughtful, and full of business than in the country, where their work is corporal labour and exercises; All which promote breeding, whereas anxieties of the mind hinder it (Graunt, p. 38).

So much for Graunt's speculations between God and the opportunities of life for both sexes. Let us leave for now any further consideration on Graunt's views for some sort of fertility perception at the time. What should be stressed is the main feature of treating a concept and a measure as a matter of fact. Based on the results provided by the data Graunt passed on immediately 'to examine the conceits, opinions and conjectures ... [and] did also admit new ones' (p. 14).

To the alert and inquiring intellects of the seventeenth and eighteenth centuries, Graunt's investigation on the statistical stability of the sex ratio could not pass unnoticed. Like Graunt, most of the reputed thinkers who became interested believed that the world and, in particular, human populations must have been the work of a Deity. But this in itself deserved to be demonstrated in a more convincing manner. So, they questioned nature further and, in this case, wondered how to make sense of the balance between the sexes. For about two centuries and, perhaps, until Darwin published his *Origin of the Species* in 1859, the advocates of Creationism reacted strongly against the barren teleological explanations maintaining that there is no such word in the post-reformation period. Yet, the average scientist certainly understood that rather than holding on dogmatic principles it was better to use the tools of mathematical statistics and demographic data to find support for their own belief, whether they were explicitly religious or not.

3.

Between divine creation and chance: an argument from design

> the Species may never fail, nor perish, since every Male may have its Female, and of a proportionable Age. This Equality of Males and Females is not the Effect of Chance but Divine Providence (Arbuthnot, 1710: 186)

Arbuthnot (1667-1735): an argument for Divine Providence

Christiaan Huygens seems to have been (see Annex A) the first to have noticed the novelty and power of Graunt's method. In his correspondence with his brother Lodewijk, Christiaan discussed the usefulness of the statistical data found in the *Observations*, applied the theory of probability to Graunt's statistical data and discussed issues on the expected and median lifetime (for more on this see Hald, 1990: 106-115).

However, it was John Arbuthnot, an amateur of mathematics and also esteemed satirist as well as Queen Anne's favourite physician, who resumed Graunt's investigation on the statistical stability of the sex ratio some fifty years after his death. In 1710, Arbuthnot read a five-page paper to the Royal Society entitled 'An argument for Divine Providence, taken from the constant regularity observed in the births of both sexes'. He provided a table like Table 1.3.1 containing yearly data for males and females christened in London for the period 1629-1710, that is 82 years.

Arbuthnot did not admit that the data for the period between 1629 and 1664 were taken from Graunt's book. Moreover, in its essence Arbuthnot's argument was the same as Graunt's in two ways, namely its main view and its objective. Like Graunt's, Arbuthnot's view was that the relative numerical equality between males and females 'is not the Effect of Chance but Divine Providence, working for a good End' (Arbuthnot, 1710: 186). And from here Arbuthnot presents the originality of his argument as compared to Graunt's, especially when he states: 'which I thus demonstrate'. Before turning to the details of his demonstration it is important to recap the major similarities in the argument of both authors.

Table 1.3.1 Arbuthnot's data on the yearly number of males' and females' christenings in London from 1629 to 1710

Year	Males	Females	Year	Males	Females
1629	5218	4683	**1671**	6449	6061
1630	4858	4457	**1672**	6443	6120
1631	4422	4102	**1673**	6073	5822
1632	4994	4590	**1674**	6113	5738
1633	5158	4839	**1675**	6058	5717
1634	5035	4820	**1676**	6552	5847
1635	5106	4928	**1677**	6423	6203
1636	4917	4605	**1678**	6568	6033
1637	4703	4457	**1679**	6247	6041
1638	5359	4952	**1680**	6548	6299
1639	5366	4784	**1681**	6822	6533
1640	5518	5332	**1682**	6909	6744
1641	5470	5200	**1683**	7577	7158
1642	5460	4910	**1684**	7575	7127
1643	4793	4617	**1685**	7484	7246
1644	4107	3997	**1686**	7575	7119
1645	4047	3919	**1687**	7737	7214
1646	3768	3395	**1688**	7487	7101
1647	3796	3536	**1689**	7604	7167
1648	3363	3181	**1690**	7909	7302
1649	3079	2746	**1691**	7662	7392
1650	2890	2722	**1692**	7602	7316
1651	3231	2840	**1693**	7676	7483
1652	3220	2908	**1694**	6985	6647
1653	3196	2959	**1695**	7263	6713
1654	3441	3179	**1696**	7632	7229
1655	3655	3349	**1697**	8062	7767
1656	3668	3382	**1698**	8426	7626
1657	3396	3289	**1699**	7911	7452
1658	3157	3013	**1700**	7578	7061
1659	3209	2781	**1701**	8102	7514
1660	3724	3247	**1702**	8031	7656
1661	4748	4107	**1703**	7765	7683
1662	5216	4803	**1704**	6113	5738
1663	5411	4881	**1705**	8366	7779
1664	6041	5681	**1706**	7952	7417
1665	5114	4858	**1707**	8379	7687
1666	4678	4319	**1708**	8239	7623
1667	5616	5322	**1709**	7840	7380
1668	6073	5560	**1710**	7640	7288
1669	6506	5829			
1670	6278	5719			

Source: *Phil. Trans.*, 1710, Vol. 27: 186-90.

Arbuthnot asserted that the greater supply of males at birth is an indication of art rather than chance. Moreover, the reason why males are more numerous than females is to repair their loss due to the fact that men, 'who must seek their Food with danger', are more subject to accidents and diseases. The wise Creator repairs the loss by bringing forth more males than females 'in almost a constant proportion', and thus guarantees 'that every Male may have a Female of the same Country and suitable Age' (p. 188). As can be remembered, this is a very similar argument to Graunt's explanation described above.

In summing up his paper, once again like Graunt, Arbuthnot used his argument to attack the practice of polygamy. 'There seems', he wrote, 'no more probable Cause to be assigned in *Physicks* for this Equality of the Births, than that in our first Parents Seed there were at first formed an equal Number of both Sexes'. Beyond that, Arbuthnot concluded:

> Polygamy is contrary to the Law of Nature and Justice, and to the Propagation of Human Race; for where Males and Females are in equal number, if one Man takes Twenty Wives, Nineteen Men must live in Celibacy, which is repugnant to the Design of Nature; nor is it probable that Twenty Women will be so well impregnated by one Man as by Twenty (Arbuthnot, 1710: 189).

When compared with Graunt's argument, Arbuthnot's shows nothing new about the phenomenon of the sex ratio. Arbuthnot was not really interested in dealing with the sex ratio as a matter of fact. While about half of the data were public domain since the *Observations* were first published, Arbuthnot's new data simply corroborate the data provided by Graunt.

However, there is a major and important difference between the two authors. Graunt used the data as matters of fact and on this basis speculated on the reasons for the phenomenon in discussion. In turn, Arbuthnot used the sex ratio as an explanatory resource and sketched a theoretical model to test and demonstrate his argument and specific hypotheses. This is a brilliant example of the difference between plumbing and science. No matter whether one judges Arbuthnot’s hypothesis silly or intelligent. Compared with Graunt, Arbuthnot’s reasoning represents a significant step forward. Regardless of the fact that he tried to explain a demographic phenomenon as a consequence of the Providential intervention, there is no doubt that he went beyond the placid surface of commonsense explanations of natural and social phenomena. Among his contemporaries, such a sophisticated demographic analysis might only be comparable to Halley's mathematical model of a life table.

The design of the sex ratio: between God and Chance

In the past, several authors have discussed in detail both the theological and the statistical arguments of Arbuthnot. Pearson (1978: 131-2) stressed, earlier in this century, that Arbuthnot was the first to use binomial distribution to express birth ratios. Hacking (1965: 75-81; 1975: 166-171) discussed in particular his test from the point of view of modern testing theory. Shoesmith (1985: 255-259; 1987: 133-146) examined critically certain features of the debates that followed the publication of Arbuthnot, mainly between Nicholas Bernoulli (1687-1759), Burnet and Craig of the English Royal Society, and 'sGravesande (1688-1742) the Dutch scholar and Professor of Mathematics at the University of Leiden; at the same time that he provided details on the theological background of the statistical discussions, he discussed in particular Bernoulli's statistical model and early notions about significance testing. More recently, Hald (1990: 275-285) resumes the matter by comparing 'sGravesande's test of significance and reconstructing Bernoulli's comparison of the observed distribution with the binomial.

Overall these authors show their good appreciation of the novelty of Arbuthnot's paper, especially his attempt to provide a mathematical or statistical demonstration of his assertions, based on a quantitative argument drawn from a probabilistic concept of chance. There is no need to go into much detail here on Arbuthnot's statistical and theological logic, for that has been already dealt with satisfactorily by the above authors. The objective here is to abbreviate the essentials of Arbuthnot's 'proof' base to make clear the difference established between the sex ratio treated as a matter of fact and used as an explanatory resource.

Arbuthnot started by stating his notion of chance and sketching his model as follows:

> Let there be a Die of Two sides, M and F, (which denote Cross and Pile), now to find all the Chances of any determinate Number of such Dice, let the Binome M+F be raised to the Power, whose Exponent is the Number of Dice given; the Coeffcients of the Terms will show all the Chances sought. For Example, in Two Dice of Two sides M+F the Chances are $M^2 + 2MF + F^2$, that is, One Chance for M double, One for F double, and Two for M single and F single ... and universally, if the Number of Dice be *n*, all their Chances will be expressed in this Series
>
> $$M^n + M^0 + \tfrac{n}{1} \times M^{n-1}F + \tfrac{n}{1} \times \tfrac{n-1}{2} \times M^{n-2}F^2 + \tfrac{n}{1} \times \tfrac{n-1}{2} \times \tfrac{n-2}{3} \times M^{n-3}F^3 +, \&c.$$
>
> (Arbuthnot, 1710: 186-187)

Arbuthnot discussed then the possibility of getting as many M's and F's by throwing of *n* times an even dice. The binomial coefficients in the expansion of $(M + F)^n$ gives the probabilities of outcomes of the *n* tosses, which are likened to each birth, or more accurately each christening. In particular he discussed the way to find coefficients of the middle term in any given power or number of dice, and infers 'that with a very great Number of Dice ... there would be but a small part of all possible Chances, for its happening at any assignable time, that an equal Number of Males and Females should be born'.

However, Arbuthnot (1710: 187) added: 'It is indeed to be confessed that this Equality of Males and Females is not Mathematical but Physical, which alters much the foregoing Calculation'. He asserted that some terms needed to be included next to the middle one and the probability chances in the tails of the distribution cannot be overlooked.

> But it is very improbable (if mere Chance govern'd) that they [the Terms next the middle one] would never reach as far as the Extremities: But this Event is wisely

> prevented by the wise Economy of Nature; and to judge of the wisdom of the Contrivance, we must observe that the external Accidents to which Males are subject (who must seek their Food with danger) do make a great havock of them, and that this loss exceeds far that of the other Sex, occasioned by Diseases incident, as Experience convinces us. To repair that Loss, provident Nature, by the Disposal of its wise Creator, brings forth more Males than Females; and that in almost a constant proportion (Arbuthnot, 1710: 188).

In other words, it is very improbable that there would not sometimes be a significant preponderance of males over females, or vice-versa females over males. Yet he refers to the data annexed (see Table 1.3.1), 'which contain Observations for 82 Years of the Births in *London*' and shows that the two-way possibility does not occur. This, in itself, was evidence that the fact that more males than females are born, in almost a constant proportion, cannot be 'the Effect of Chance but Divine Providence, working for a good End'. But still, Arbuthnot was not content with such an intellectual leap; he may have had anticipated its weakness. As Hacking (1975: 167) put it,

> This argument is invalid. It is true that it is 'very improbable' that the outcomes "would never reach as far as the extremities". But unlike Bernoulli, Arbuthnot was unable to quantify the qualitative 'very improbable'. If he had, he would have found, as Nicholas Bernoulli subsequently showed, that the constant regularity observed is exactly what one would expect if the chance of a male birth is 18/35 (Hacking, 1975: 168-169).

But like many demographers and statisticians, Arbuthnot's model was not an innocent device, and irrespective of what else needed doing his purpose really mattered. So, he went on and wrote:

> Now, to reduce the Whole to a Calculation, I propose this *Problem*. A lays against B, that every Year there shall be born more Males than Females: To find A's Lot, or the Value of his Expectation (Arbuthnot, 1710: 188).

If the chance of a male birth were exactly $\frac{1}{2}$, what would be the probability that in every year, more males should be born than females? As he answered, 'A's Lot for each Year is less than $\frac{1}{2}$'. That is, for every year in the 82 years the probability 'will be $\left(\frac{1}{2}\right)^{82}$', that is equal to $\frac{1}{4.836 \times 10^{24}}$.

> But if A wager with B, not only that the Number of Males shall exceed that of Females, every Year, but that this Excess shall happen in a constant Proportion, and the Difference lye within fix'd limits; and this not only for 82 Years, but for Ages of Ages, and not only at *London*, but all over the World; (which 'tis highly probable is Fact, and designed that every Male may have a Female of the same Country and suitable Age) then A's Chance will be near an infinitely small Quantity, at least less than any assignable Fraction. From whence it follows, that it is Art, not Chance, that governs (Arbuthnot, 1710: 188-189).

As Hald (1990: 278) pointed out, in modern terminology Arbuthnot wanted to test the hypothesis that the probability, p, of a male birth equals $\frac{1}{2}$ against the alternative that $p > \frac{1}{2}$. By means of the binomial distribution he first proved that for any number of births $\Pr\{M > F | p = \frac{1}{2}\} \leq \frac{1}{2}$. He then used this result to transform the original hypothesis and its alternative to the hypothesis that $\Pr\{M > F\} \leq \frac{1}{2}$ and the alternative $\Pr\{M > F\} > \frac{1}{2}$ for the yearly number of births. In that way he rejected the hypothesis of equal chance, avoided the difficulties resulting from the varying numbers of births and transformed the comparison of the 82 binomial distribution into a sign test.

Hacking (1975: 168) considered this second reasoning statistically valid, as opposed to the first step which he says that based on inadequate understanding of the limiting properties of chances. He identified a third, a 'metastatistical' argument in Arbuthnot's paper. This is related with the conclusion that since the constant proportion cannot be due to equal chance, so births are governed by 'Art, not Chance'.

In short, Arbuthnot's model comprised two steps of statistical reasoning and an inference about the action of Divine Providence in natural phenomena. The first was associated with the properties of chance, which Hacking considered depicted an invalid argument. The second was his test of the significance of his statistical hypothesis; and the third, what Hacking called the 'metastatistical argument' referring to the nature of statistical stability. Most of this has little to do with matters of fact, for at issue is the theoretical modelling and reasoning through mathematical or statistical means. This means that any conclusion and generalization about the facts that the data represent depend on some sort of theoretical framework. Beyond that, we cannot distinguish what is real or apparent, nor what is more or less improbable, without theory. It is exactly for this reason that the sex ratio should be considered in its twofold character, or its two faces: one looking over the empirical observations concerning matters of fact about the numbers of males and females, and the other presiding over beginnings and endings of specific statistical models.

4.

Forward from Graunt and Arbuthnot: 'Boy or girl - not just chance?'

> But even though quite naturally we are induced to take notice of deviations from the average, and to ask how these irregularities can be explained, it is just as natural that our predecessors first of all were struck by the regularity and cared less for the deviations (Westergaard, 1932: 72)

The question 'Boy or girl - not just chance?' was the topic of an article in one of the 1994 editions of *New Scientist* called 'Why presidents have more sons' (Ridley, 1994: 28-31). The author of this article argued that the sex of children may not be random at all, and although he has at his disposal much more information than Graunt and Arbuthnot had, it is clear that much mystery surrounds the subject. So, without going much earlier in history, the underlying causes for the constant regularity observed in the births of both sexes remain a scientific mystery at least since Graunt depicted it empirically; Arbuthnot attempted to prove statistically that the regularity of the sex ratio at birth is non-random and accidental.

Since Arbuthnot the sex ratio has been used in science in its twofold character, as a matter of fact and a theoretical resource to construct statistical and demographic theories. In the case of demography the sex ratio has been paramount in the process of construction theories of fertility, reproduction, population growth and nuptiality. Most of the existing theories and models would hardly be imaginable if they were not standing on the assumptions made, explicitly or implicitly, about the sex ratio.

No theory which focuses, conceptually and methodologically, on one of the sexes but not on both simultaneously, would be scientifically possible without their assumptions, whether implicitly or explicitly stated, about the sex ratio at birth and the sexual nature of reproduction. So, after Graunt and Arbuthnot the sex ratio has been used in its twofold character; in the following pages I provide a brief review of the way the Janus-like nature of the sex ratio secured assent in scientific research until the time Darwin outlined a radically revolutionary way to search for the causes of the design of sexual reproduction and regularities between the sexes.

William Derham: 1657-1735

The Rev. William Derham, Rector at Upminster, was like Arbuthnot a very active member of the Royal Society and an influential scholar among statisticians and demographers of the seventeenth and eighteenth centuries. According to Pearson,

> Derham links up the beginning of statistics in Graunt, Petty and King with the theologico-philosophical conceptions of Newton, and his books having wide popularity ... exercised a great influence over general thought, directly prepared the path for Süssmilch's work, and so by way of Quetelet to modern statistics. It is in fact from Derham that we must trace Florence Nightingale's attitude which links statistics and theology; that is, to grasp the deity's moral purpose in the universe we must study statistics - in particular we must interpret the stability of statistical ratios (Pearson, 1978: 281).

Following Newton's influential theological approach on the natural laws, Derham attempted to get closer to the mind of God by confining himself to the harmony and perfection of the animal organisms. Derham extended the notion involved in stable physical laws of the Universe to the statistical regularities depicted by the sex ratios; he sought in the statistical relations of humans and animals to their environment further evidence of the perfection of the divine ordinances.

For Derham the exact balancing of the number of individuals of each species was necessary for the stability of creation, and numerical stability was a grand act of Divine Wisdom. In the role of Boyle lecturer, in 1711 and 1712 Derham was minded to improve philosophical matters to theological uses; his lectures were published in 1713 under the title *Physico-Theology: or, a Demonstration of the Being and Attributes of God, from His Works of Creation* (Pearson, 1978: 289; Dupâquier and Dupâquier, 1985: 157-158; Shoesmith, 1987: 136; Hald, 1990: 279)

Derham elaborated on the 'argument from design' and compared the well arranged world in each of its parts to an intricate piece of clockwork. According to Hacking (1975: 169-170) Derham was well qualified to recognize clockwork: his first published book was a survey of different methods of clock-making [1696]. Writing his lectures, he happened upon Graunt's book, an event which Pearson described in a lively manner:

> Derham is not afraid of his hypothesis, for while he admits that war and pestilence may be just punishments for the sins of men, he says that they are also a wise means of keeping the balance of mankind even (p. 267), and cites the fertile countries of Asia where although great plagues and wars sweep away prodigious multitudes, yet these countries so far from being wasted remain full of people. In considering the sex ratio Derham quotes with approval Graunt's 14 to 13 or 1.08. He says he has investigated 100 years of his own registers and found 709 males to 675 females or 1.05 to 1. He says that the burials were nearly in equality, i. e. 636 males to 623 females, but this is really 1.02 to 1. Following Graunt he remarks that there is a man for every woman and no excuse for polygamy. Thus the deity indicates his views. The surplus males are very useful for the supplies of war, the seas, and other such expenses of men above women (Pearson, 1978: 293-294).

Hacking (1975: 170) mentions another passage of Derham's lectures in which he cites Arbuthnot as well: 'That this is the work of Divine Providence and not a matter of chance, is well made out by the very laws of chance by a person able to do it, the ingenious and learned Dr Arbuthnot [1713, p. 176, n. 8]' (Hacking, 1975: 170). As well, Dupâquier and Dupâquier (1985: 158) consider Derham as much a follower of Graunt, Petty and King, as a predecessor of Malthus and Darwin. 'He completed his theories', write the authors of the *Histoire*, 'by calculating a table relating marriages and births, births and deaths in several places of Europe' (see the table in Dupâquier and Dupâquier, 1985: 159).

Bernoulli (1687-1759) versus 'sGravesande (1688-1742)

The argument that constant statistical stability cannot be the effect of chance, as Arbuthnot argued and Derham agreed, was not accepted by all scholars at that time. For several years this debate involved a number of reputed mathematicians, including Bernoulli, 'sGravesande, a Dutch scientist who in 1718 became professor of mathematics, astronomy, and philosophy at Leiden, and Bernard Nieuwentyt (1654-1718), a Dutch physician and mathematician (Shoesmith, 1985, 1987; Hald, 1990: 279-280).

The two Dutch mathematicians, 'sGravesande and Nieuwentyt, felt it necessary to improve on Arbuthnot's statistical test of the work of divine providence. As Hald (1990: 279-280) and Shoesmith (1987: 138-141) described, Arbuthnot used only the evidence that for each of the 82 years, the number of males was greater than the number of females. But for 'sGravesande, Shoesmith explains,

> the probability of the observed data, given the hypothesis that ‘chance’ governed the sex of children born, was the probability of observing, in each of 82 consecutive years, the number of male births falling between two specific values, defined by reference to the extreme of the observed data (Shoesmith, 1987: 139).

Hald describes 'sGravesande's reasoning. Although this description is of interest more for statisticians, the fact that the sex ratio was the centre of the scientific debate is by itself interesting to know:

> 'sGravesande further makes use of the fact that the relative number of male births varies between 7765/15,448 = 0.5027 in 1703 and 4748/8855 = 0.5362 in 1661. Because of the different yearly number of births, the numbers of males are not directly comparable, and 'sGravesande therefore transforms the observations above by multiplying the relative frequencies by the average number of births for the 82 years, which he finds to be 11,429. This gives him a fictitious minimum and maximum number of male births: 5745 and 6128. He then considers the data as 82 observations from the same binomial distribution with n= 11,429 and all the observations contained in the interval [5745, 6128].
>
> To find the probability of this event under Arbuthnot's hypothesis, he calculates the terms of the binomial for $p=\frac{1}{2}$ and n = 11, 429 and sums the 384 terms corresponding to the interval in question. Actually, he uses the recursion

$\binom{n}{x+1} = \binom{n}{x}\frac{n-x}{x+1}$, and tabulates $10^5\binom{n}{x}/\binom{n}{5715}$ from x = 5715 to 5973, after which the tabular values become smaller than $\frac{1}{2}$. He finds that $\Pr\{5745 \le x \le 6128 | p = \frac{1}{2}\} = 3{,}849{,}150 / 13{,}196{,}800$, and remarks that he has added a small amount to the numerator to make sure that the probability is not undervalued because terms in the tail smaller than $\frac{1}{2} \times 10^{-5}$ have been neglected. ('sGravesande probability equals 0.292, and the normal approximation gives 0.287 (Hald, 1990: 279-280).

During the latter part of the year 1712, Bernoulli met 'sGravesande in The Hague on his tour to the Netherlands, England, and France. They discussed Arbuthnot's paper, and continued the debate by correspondence after returning to the Continent. Bernoulli took issue particularly with Arbuthnot's and 'sGravesande's inference from the data, and argued that an alternative interpretation was more reasonable. As Hald put it,

> Bernoulli does not seem much interested in the theological debate; his attitude is pragmatic, like that of a modern statistician. The main points of his letters are (1) to estimate the probability of a male birth from the observations; (2) to compare the distribution of the observations with the binomial distribution to determine whether the observed variation can be explained by this model; and (3) to provide the mathematical tool of this comparison by finding an approximation to the binomial for large *n* (Hald, 1990: 281).[1]

In short, Bernoulli argued that there was no need to call on divine providence to account for either the persistent superiority of male over female births, or the narrow variation in the male:female birth ratio. In turn, 'sGravesande responded that he misunderstood Arbuthnot, who only intended to prove that p is larger than $\frac{1}{2}$. Bernoulli replied that Arbuthnot's paper also implies that the variation in the number of male births is smaller than could be expected, and it was only this assertion that he was interested to refute. In the end, 'Bernoulli and 'sGravesande were not at issue over mathematics,' as Shoesmith concluded, 'nor indeed over statistical logic. What was at issue was the interpretation of "chance" in this context, the nature of statistical stability, and perhaps a reputation or two' (Shoesmith, 1987: 144).

The verdict of de Moivre (1667-1754)

Abraham de Moivre, a close friend of Newton, followed and supported Arbuthnot's work. Although he focused most of his work on annuities, as Pearson (1978:

[1] For details on Bernoulli's reasoning see Hald (1990: 280-284) and Shoesmith (1985: 256-257; 1987: 141-144).

155) writes, 'De Moivre was a really powerful and original mathematician and he brought his powers of analysis to bear on the old problem of chance'.

Like Graunt and Arbuthnot, De Moivre attributed the stability of sex ratios to the Deity, but with growing interest for the statistical and the study of binomial distributions. As he put it, 'The Deity fixed the "means" and "chance" provided the fluctuations'. Although he supported 'sGravesande's interpretation, de Moivre approved his compromise with Bernoulli with regard to the two facets of Arbuthnot's argument. Pearson's review of this controversy is of interest for its remarks upon the scientific spirit in research inquiry:

> The old neolithic myth of creation stopped geological and biological inquiry until Darwinian evolution crashed down the barrier. Assume an 18 to 17 ratio to be a wise creation and you will stop short of asking the physiological reasons why that ratio holds. Hence I prefer Bernoulli at this point to De Moivre. Statistical ratios are only stable as long as there is a fairly even balance between man and his environment; upset that and a new position of equilibrium must be reached. Does climate, does race, does relative age, does order of birth affect the sex ratio? These are all useful problems, if we consider the origin of the sex-ratio; but their investigation may be checked by De Moivre's dogma as to statistical ratios. Still, that dogma produced widesweeping effects for a century after De Moivre (Pearson, 1978: 162-163).

Johann Peter Süssmilch: 1707-1767

The conception of population reproduction as being ordered by the Divine Order was to become for a time the ruling view of the Lutheran clergyman and quondam chaplain to Frederick the Great, Johann Süssmilch. He died when Malthus was about one year old and half a century before Darwin was born. Thomlinson (1976) considered Süssmilch's book, published in 1741, *The Divine Order in the Changes of the Human Race Shown by Its Birth, Death, and Propagation*, the second major landmark in demographic history.

Hecht (1987: 34) describes Süssmilch as 'a German prophet in foreign countries'; from this description one gets the impression that Süssmilch represents the eve of the turning point that Malthus was about to introduce in demography. On the one hand, Süssmilch was well aware of the work of the great names in statistics and demography, such as Graunt, Petty, King, Halley, Davenant, Arbuthnot, Derham, De Moivre, and s'Gravesande (Hecht, 1987: 34). On the other hand, he became an essential link in the international development of demographic analysis, including demographers in Netherlands (Nieuwentyt and Struyck), England (Short), Switzerland (Gessner 1709-1790 and Muret 1715-1796), Denmark (Niklas von Oelreich 1699-1770 and Sweden (Wargentin) (Hecht, 1987).

Süssmilch developed his ideas on the basis of on Derham's view, 'but with all a German's assiduity he piles up masses of statistics' (Pearson, 1978: 296); he delved further into the biological laws of the universe and acknowledged the contribution of his predecessors Graunt, Petty, King, Arbuthnot, Derham and Nieuwentyt. For Süssmilch the study of births and deaths leads to the proper understanding of the divine ordinance proclaimed in the verse of *Genesis*.

Overall, Süssmilch's ideas were far from original, since they corresponded to those of the 18th century theologians and of many 18th century men of science (Pearson, 1978: 314). Moreover, although the Germans acclaimed him as the first author devoted to the discussion of population, he basically explained demographic phenomena in terms of the divine ordering.

He believed that to maintain a balance the deity must reduce the marriage-rate and the fertility-rate, or increase the death-rate, that is, reduce average longevity. Anticipating closely what some decades later Malthus would argue upon the positive checks to population, Süssmilch maintained that wherever there is a balance of population it arises from one or other of three sources: the marriage rate, the fertility rate, or increase in the death rate; and all these factors are settled by the Deity so as to preserve the balance (Pearson, 1978: 314).

With regard to the sex ratio at birth, Süssmilch asserted that the 1.05 ratio could be considered the ratio of equality by the marriage age. The divine providence in arranging monogamy was, for him, 'the only satisfactory way for carrying out the *Göttliche Ordnung* of increasing and multiplication' (Pearson, 1978: 316). Once again, while Süssmilch did not produce arguments to show that polygamy reduces fertility, he followed Graunt's interpretation of his discovery regarding the near equality of the sex ratio.

According to Hecht (1987: 43), 'During the first half of the nineteenth century two important trends in thought affected the appreciation of Süssmilch's ideas': the strengthening of the anti-theological movement in France and Switzerland, and the dispute about the value of his life which began in England and spread to the Continent. 'Both these focused attention on Süssmilch's work', Hecht (1987: 43) continues, 'and contributed directly to the advancement of the new discipline of demography'.

In particular, from Schräder to Ferrière, the 'natural' order replaced the 'divine' explanations. The title of Schräder's book is suggestive of the changes taking place: 'Practical application of the *laws of nature* (our italics) to the life and death of mankind' (Hecht, 1987: 43). In France authors such as Eichoff agreed with Süssmilch 'that birth, reproduction and death occur among men, not according to chance, but according to a settled, universal constant order, though he does not anywhere refer to this order as being "divine".' And then Ferrière, editor of the *Annales de Statistique*, 'eschews any theological considerations and refers only to the laws of order that nature imposes upon herself' (Hecht, 1987: 43-44).

Adolphe Quételet: 1796-1874

In several parts of his Lectures, Pearson (1978: 129, 160, 296) remarks that it was not until the conception of evolution spread from Darwin that the argument for Divine Design of the statistical stability of the sex-ratio died out. Hecht (1987: 44) also considers that 'The publication of Quételet's *Physique sociale* marks the final incarnation of the divine order'. However, this interpretation does little justice to Quételet's extensive search for the natural, social, and individual determinants of the stability of the sex ratio.[2]

Quételet deserves a special attention, not only because of his significant role in demographic research and debates during the nineteenth century, a point which will be made clear in Chapter 8; but because he was among the authors who contributed to the establishment of demographic research on the sex ratio as it is still mostly treated today: as a matter of fact. In this context, contrary to Graunt, Arbuthnot, De Moivre, Derham, and Süssmilch, Quételet's book *Sur L'Homme, et le Développement de ses Facultés,*[3] first published in 1835, shows a remarkable effort to overcome the intellectual constraints set by the deeply-seated theological dogmas.[4]

The fundamental concept in the *Treatise on Man* is the Average Man. This concept gathered wide sympathy among several statisticians of Quételet’s time, mainly because he attempted to demonstrate that physical and moral qualities tend to be normally distributed around a statistical mean. As Westergaard (1932) put it, this 'distribution of the observations around the mean was a good illustration of the law of error'. Quételet examined the anthropometric properties of the average man, and 'all which relates to the life of man, his reproduction, and mortality ... the development of his stature, weight, strength, and his physical qualities in general' (Quételet, 1968: 10). He started with a discussion on births and fecundity, the latter being defined as 'the annual number of births of a country'. In present demographic terminology his definition of fecundity would

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[2] Being a disciple of Claud Henri de Saint-Simon, the proponent of a science of man called 'social physics', Quételet followed similar directions to A. Comte, who preferred the term 'sociology', and J. J. Rousseau, who focused on the nature of man in an anthropological perspective (Dupâquier and Dupâquier, 1985: 419). Thomlinson (1976: 11) certainly did not exaggerate when he wrote that 'Adolphe Quételet (1796-1874) - merits citation as an extreme case of successful enthusiasm for statistical studies'; and, elsewhere, 'Quételet was instrumental in the organization of the International Statistical Congress ... He may have been the most effective catalyst that population statistics has ever known'. Differently from Comte and Rousseau, Quételet developed a demographic alternative within the so-called social-physics. Besides, he wrote extensively on a wide range of demographic issues, including the sex ratio, the life table, causes of deaths, birth and marriage rates, and unemployment.

[3] The version used here is the first translated into English, in 1968: *A Treatise on Man and the Development of his Faculties*.

[4] Westergaard asserts that contrary to previous authors, Quételet

> was more inclined to take the standpoint of naturalistic philosophy. Quetelet speaks of a *physique sociale*; he considered the statistical phenomena as products of physical laws, as results of environment, the human will having no part in the events. Society contains all germs of the coming crimes, the guilty person being only an instrument of execution (Westergaard, 1932: 169).

correspond more to the notion of natality, which is the term mainly concerned with births and the birth rate (United Nations, 1958: 35).

Chapter 2 of his *Treatise on Man*, 'Of the influence of natural causes on the number of births', addresses the question of the balance of the sexes:

> THERE is a very remarkable fact, which has been long ago observed, although we do not yet know the true causes of it. It is this - that more boys are born annually than girls. Now, since the proportion of male to female births does not differ much from unity, or is almost the same for the different countries for which it has been calculated, it has been necessary to have recourse to numerous observations to determine it with some precision. After more than fourteen and a half millions of observations made in France, from 1817 to 1831, the value of this ratio has been as 106.38 to 100; and its average value has varied but little, taking one year with another (Quételet, 1968: 11).

From here, Quételet presents a detailed discussion of the available data on the relationship between the number of births and the influence of a wide range of possible determining factors: the sexes, the age of husbands, wives, widowers and widows, places and climate, the years, the seasons, the hours of day, and other causes: professions, trades, modes of life and morality. His discussion of the statistical data and other sources is sound and well supported. To allow the reader to have a flavour of such a debate of more than a century and an half ago, and in particular to see how Quételet went beyond the simplistic argument for divine providence of previous authors, it may be worthwhile to summarize his discussion upon the 'influence of the sexes'.

Quételet starts by considering the influence of climate on the sex ratio, focusing his attention on the existing data gathered by M. Bickes from the principal European states. Bickes collected more than seventy million observations and estimated the average sex-ratio at birth in Europe to be 106 around 1830. Hence, Quételet comments:

> Some travellers have thought that hot climates are more favourable to female births; but numbers have not confirmed this opinion, at least from what we have just seen in Europe. However, more observations than we possess are necessary, and especially observations collected near the equator, before we can affirm that the influence of climates is absolutely insensible (Quételet, 1968: 11).

He then focuses on the results from the observations amongst the white people and the slave population in Cape of Good Hope (South Africa). While the data on the births amongst the whites show a sex ratio of 97.3 per 100 females, those for the slaves or black population indicate a sex ratio of 103.9. Curiously, the translator of Quételet's book intervenes here with his own footnote, asserting that the predominance of female over male births amongst the whites 'is not so much owing to climate as to the peculiarity of race'.[5]

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[5] As the translator of Quételet's book into English, Dr R. Knox, argues:

> the free white population of the Cape are, as near as may be, purely Saxon, descended from the old Dutch families, who originally settled there about two hundred and seventy years ago. They have preserved the purity of their blood with great care, intermingling as little as possible with the dark races, whether Caffre or Hottentot. Generally speaking, they hold the mulatto in great dislike and contempt; so that, amongst the pure Dutch of the Cape, a mulatto, however slightly tinged, has hitherto had little chance of acquiring a proper status in society. With respect to M. Quetelet's table of

As to the influence of residence in town or country, Quételet writes that 'is not without its influence on the ratio of births of the two sexes'. Drawing upon some data from the Belgian population, he remarks:

> The number of boys, compared with that of girls, has then been smaller in town than in the country ... This influence of town residence, tending to diminish the proportional number of births, is also observed in other countries (Quételet, 1968: 12)

Upon researches concerning the ratio of male to female births according to legitimacy status, the author refers the work of Poisson and Babbage. In quoting the data elaborated by these researchers Quételet writes:

> M. Prévost observes that, independently of the physiological cause which gives a greater facility to male births, there exists an accessory cause in legitimate births especially, which still further increases this facility, and which he attributes to a sort of preference generally given to children of the male sex; 'Is not the end of this preference', said he, 'to prevent, after male births, the increase of the family, and consequently to increase the proportional ratio of the latter? Parents have one son: if different causes impede the increase of their family, they will perhaps be less uneasy at this privation, when their first wish is accomplished, than they would have been if they had not male children. Would not this diminution of births, after one or two sons, tend to increase the ratio of male births?' (Quételet, 1968: 12).

Quételet finds this moral restraint insufficient to support other results. He passes on to refer to Giron de Buzareignes' findings on the births of children of both sexes in France, which were divided into three classes: first, births to persons whose occupations tend to develop the physical qualities; second, to persons whose occupations tend to weaken these powers; and third, to persons whose occupations are of a mixed kind. According to the results, the proportional number of male births in the first class was greater than the one for France in general. In contrast, the second class gave opposite results to the former, while the third class gave an even number of the sexes.

Bickes, who according to Quételet was 'much inclined to question the opinion advanced by M. Giron de Buzareignes', had put forward a new explanation of the causes of variation in the ratio of the sexes:

> It is in the blood (the constitution, the race) of people or nations, who differ more or less from each other in this respect, that the powers or causes reside, whatever they may be, which determine the production of many boys. Political and civil institutions, customs, habitual occupations, mode of life, wealth, indigence, &c. - all these things have no influence on the respective ratio according to which the two sexes come into the world (Bickes, cited by Quételet, 1968: 12).

Overall, the author of the *Treatise of Man* casts some doubts upon all the above hypotheses on the blood, the legitimacy, and the residential place, mainly as to whether they could be good enough to explain the ratio of births of the two sexes amongst people

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births, it seems probable that an excess of boys over girls is a law chiefly with the Celtic and Sarmatian races, and that in respect to the pure Saxon race, there exists either an opposite law, namely, the excess of females over males, or, perhaps, as near as may be, an equality; but the translator inclines to the opinion that the excess will be in the females with respect to the Saxon race (Knox, in Quételet, 1968: 11-12) of a homogeneous population. And from here, he considers the hypothesis of the influence of the age of parents on male and female births, put forward by Professor J. D. Hofacker in Germany, and Sadler in England (see on this Westergaard, 1932: 164). Quételet brought together the different results of Hofacker in a table like Table 1.4.1.

The conclusion from these results was apparent: when the mother is older than the father, fewer boys than girls are found to be born; the same happens when the parents are of equal ages; but the more the father's age exceeded that of the mother, so was the ratio of boys greater. Yet Quételet remained cautious. He regretted that these results were not deduced from sufficiently numerous observations, and so accurate as to deserve entire confidence, and to be verified in other countries. If all these conditions were fulfilled, the type of results of the above table would present a very powerful argument in favour of the hypothesis, 'that the births of one or the other sex can be made to predominate at will'. However, few proper documents to elucidate this delicate question were still available; but he succeeded in finding some evidence in the work of Sadler on the *Law of Population*.

Table 1.4.1 The influence of age of parents on male and female births

Ages of the Man and Woman	Boys to 100 Girls
The man being younger than the woman,	90.6
.. .. as old as the woman,	90.0
.. .. older from 3 to 6 years	103.4
.. .. .. from 6 to 9 years	124.7
.. .. .. from 9 to 18 years	143.7
.. .. .. by 18 years and upwards	200.0
The man from 24 to 36 - the woman from 16 to 26 years	116.6
.. .. 36 to 46 ..	95.4
.. .. 36 to 48 years, .. young,	176.9
.. .. middle-aged,	114.3
.. .. older	109.2
.. .. 48 to 60 years, .. middle-aged	190.0
.. .. older	104.3

Source: Hofacker, 1832. 'Annales de Hygiène', p. 450, in Quételet, 1968: 13

Quételet observes that Sadler's results agreed perfectly with those of Hofacker. After calculating the fecundity of the marriage based on Sadler's tables, he concluded that the data were consistent with the idea of a value depending on the respective ages of the espoused:

> In examining the influence of the age of the parents on births, Mr Sadler has been led to the following conclusions:- The ratio in which the sexes are born is regulated by the difference of age of the parents, in such a manner that the sex of the father or the mother will preponderate beyond the average of the total number of births, according to the party which has the excess of age. On the other hand, the sex which is in excess will have a mortality depending on the period which separates the age of the parents, so that the sexes will be balanced in numbers, towards the ordinary period of marriage.

> It is thus that Mr Sadler explains how the proportional number of male births is not so great in the manufacturing towns of England as in the country, where men marry later, and present a greater difference of age to the women whom they espouse. He also extends his explanation to the difference which is observed between legitimate and illegitimate births (Quételet, 1968: 13).

Once again, Knox, the translator, added here his opinion in a footnote, saying 'It is a fact which appears well established by several statisticians, and by Mr Milne in particular (Traité des Annuités, vol ii, 493), that precocious marriages generally produce a greater number of daughters'. But Quételet continues with Sadler's findings now based on registers of marriages:

> Mr Sadler, moreover, finds, that in considering the age of the father or the mother separately, we do not observe any difference of facility in producing infants of one sex rather than of another. This facility, according to him, only depends on the relative ages of the parents ... Lastly, in extending his researches to widows and widowers, Mr Sadler further finds, from the registers of English couples, that the widowers tend to produce more female children ... The ratio is so marked, that we find it almost corresponds to the different ages (Quételet, 1968: 13).

In summing up his examination of the probable causes which may produce the inequality between the births of male and female children, Quételet remarks:

> the most influential, if we may trust to the few documents which science at present possesses, is evidently that which the difference of age of the parents produces: we might even think that the other causes which have been pointed out, are in some manner the effects of it. Indeed, it generally happens throughout Europe, that men, when they marry, are five or six years older than women, so that the preponderance of male births will be almost the same, as is established by the researches of Hofacker and Sadler, who give, as the ratio of births of both sexes, the number 103.5 nearly, when the father is from 1 to 6 years older than the mother. Now, we think that this ratio will be larger or smaller, according as the difference of age of the parents is greater or less in the different nations, in town or country, among the persons whose connexions are legitimate or illegitimate; and, lastly, according to all the circumstances which may cause the ages to vary at which production takes place; so that the age of the parents will be the principal regulator which determines the magnitude of the ratio between the births of the two sexes. Hence we see how important it is to direct our researches to the age at which marriage takes place, especially since the greater or less mortality of children depends on these ages (Quételet, [1841]1968: 13-14)

The discovery of evolution by natural selection would knock down the Cartesian and Newtonian conceptions of the world as a machine fully constructed from the hands of its Creator (Capra, 1988: 59, 105; Tannahill, 1989: 2; Ridley, 1993: 6-7; Dennett, 1995b). Quételet wrote his analysis summarized above a quarter of a century before Darwin published his theory of evolution, in 1859, in his famous book *The Origin of Species by Means of Natural Selection*. Although Quételet did not benefit from the new revolutionary ideas that since Darwin have modified the life sciences, one should not miss the fact that he searched for the 'principal regulator which determines the magnitude of the ratio between the births of the two sexes'.

In the end ... 'What stabilizes the sex ratio?'

It is beyond the scope of the present work to provide a more extensive review of how the investigation and knowledge about the sex ratio have developed until the present moment. The purpose of the above three chapters was to draw the meaning of earlier anticipations of the two-sex demography around the body of literature about the Janus-like nature of the sex ratio. In Chapter 7 I return once more to this overview, but simply to stress how Knibbs used the sex ratio both as matter of fact and as an explanatory resource. However, the main objective of the above overview on the sex ratio, as the first and core demographic concept from which one can hope to weave a two-sex perspective, seem adequately fulfilled.

In any case, it seems important to avoid the idea that during the twentieth century the investigation about the principal regulator which determines the magnitude and stability of the ratio between the births of two sexes has been abandoned or has made no substantial progress. If one considers how conventional demographers have restricted their uses of the sex ratio, it should not be surprising that few are aware of the numerous studies on Arbuthnot's research problem. As the reference, at the beginning of the present chapter, to the topic of a 1994 article published in *New Scientist* suggests, the investigation that Arbuthnot began has not been satisfactorily concluded. In his article, Ridley (1994) described and confronted several research findings and theories about the stability and the increasing evidence that the sex ratio might be neither random nor necessarily the work of Divine Providence.

Over the twentieth century, many scholars have carried out new and extensive research (Goodman, 1961; Gray, 1991; James, 1971, 1987a, 1987b, 1995; Karlin and Lessard, 1986; Kumm et al., 1994; Martin, 1994; Pollard, 1969; Teitelbaum et al., 1971; Teitelbaum, 1972). Some of them reviewed and summarized the literature about the variation of the human sex ratio. For instance, Teitelbaum (1972) reviewed the importance of the sex ratio to demographic, biological and genetic models; he listed about thirty factors associated with the human live-birth sex ratio, along with the appropriate references: birth order, family size, sex of first-born, maternal age, paternal age, relative ages of father and mother, general genetic factors, race and colour, inbreeding and outbreeding, radiation damage, ancestral longevity, physique and temperament, baldness of father, cigarette-smoking, coffee-drinking, blood groups, birth control, artificial insemination, frequency of intercourse, time of conception during menstrual cycle, seasonal and monthly variation, geographical and climatic conditions, illegitimacy, parental occupation, socio-economic status and conditions, war and post-war periods, urban/rural and other differences, high-speed stresses, and sex of last prior pregnancy (Teitelbaum, 1972: 98-99).

In 1986 Karlin and Lessard published a book called *Theoretical Studies on Sex Ratio Evolution*. According to Karlin and Lessard:

> There are two main approaches to understanding the causes and effects of sex ratio. One emphasizes the optimization and adaptive functions of sex allocation, the other

> the consequences of genetic sex-determination mechanisms (Karlin and Lessard, 1986: xiii).

Karlin and Lessard (1986: xv) carried out their sophisticated study motivated by the desire 'to understand in what way a 1:1 sex ratio could be considered optimal and then why biased sex ratio occur'.

In 1987 James sumarized much that is known about the variation of the human sex ratio at birth; related these findings to a hypothesis designed to explain them; and outlined some of the research needed to test his hypothesis. James has for years been building a theory based on parental hormone. In his 1995 paper entitled 'What stabilizes the sex ratio?', James contended that the human sex ratio at birth is stabilized only to a minor extent by the direct processes of natural selection. Instead, James proposed, the major factors stabilizing sex ratio seem to be behavioural (coital rates) and psychological (parental perceptions of adult sex ratios). In particular, James argues that parental hormone levels are (a) a consequence of perceived adult sex ratios, and (b) a cause of sex ratio in the next generation, thus providing the basis for a negative feedback process stabilizing the sex ratio. 'So, as an alternative (or supplement) to Fisher's solution,' James concluded,

> it seems reasonable to propose that sex ratio at birth is stabilized by: (i) coital rates, and (ii) parental perceptions of adult sex ratios, both processes being mediated by parental hormone levels at the time of conception (James, 1995: 247).

Wherever the investigation on the stability of the human sex ratio may lead, it seems very important to consider how an investigation set around a two-sex concept and measure, such as the sex ratio, may illuminate the search for the underlying strategies of the interactions and balance between the sexes ? This is particularly important for the development of a two-sex demographic transition theory. More specific issues are, for instance the following: to what extent will the sex ratio spoil the one-child policy in China? And, vice-versa, to what extent will the one-child policy of the Chinese Government disrepute the sex ratio of China's population? Are the stabilizing mechanisms of the sex ratio an underlying reality that people do not experience directly but may contain the explanation for what came to be known as the proximate determinant of fertility (output)? Are the stabilizing mechanisms of the sex ratio important to understand the causal mechanisms of fertility change?

5.

Malthus's principle of population and sexual reproduction

> As with any writer of his stature, Malthus can be seen as a source of diverse theoretical developments. 'Forward from Malthus', even to speak only of 'Population Malthus', thus does not single out a particular direction, not even perhaps a single quadrant (McNicoll, 1988: 145).

I finished my review of Graunt's *Observations* wondering about an issue that seems to be much overlooked when the classics of demography are discussed: why did some streams of classical thought remain dormant, while others inspired a great deal of useful ideas and debate? This issue has emerged as I became aware of the conflicting and somewhat distorted portraits of Graunt's *Observations* that still dominate our field (see Annex A). While for Graunt's case the discussion provided in Annex A also in the previous chapters seems enough to answer the question outline in this paragraph, this same matter seem relevant to other earlier demographers as well. This is not the appropriate place to do it, though in this chapter I will be deal with some features concerning an important stream of Malthus's theory construction, which has provoked some controversy but remained dormant as a source of new theoretical developments. I will challenge the contempt which prevails even in conventional demography towards Malthus's principle of population and, in particular, propose a way to overcome the intellectual embarrassment which its second postulate has enhanced ever since it was first formulated.

I start by considering the relevance of the twofold character of Malthus's principle of population for demography in general. This seems to be a good, if not the only, way to make sense of the cherished view among today's demographers that 1798 represented an important 'turning point' (see Thomlinson below) in the development of demography. I then call attention for the important step forward provided by Malthus's *Principle of Population* in relation to Graunt's model of demography's whole design. Thirdly, I face the intriguing question of why Malthus's principle of population has been so successful. Persuing the latter question further, and like in the case of sex ratio I will look at Malthus's fundamental principle as an explanatory resource. This approach is contrasted with two arguably less apt but still widely prevalent perspectives: the standard dichotomy between the deductive and inductive methods attributed to Malthus's theory construction; and the misuse of the term paradigm in recent overviews of demographic theory.

After this general review of Malthus's principle of population I return to the postulate of chief interest for this thesis: on the passion between the sexes. Then it should becomes clear that despite Malthus's withdrawal of the initial statement of his two postulata, his vision about passion between the sexes was not motivated by 'the flush of exuberant youth' (Wrigley, 1986: 53). This will be demonstrated by putting Malthus side-by-side with his critics and then discussing the passion between the sexes as a synonym of sexual reproduction.

Why was 1798 a turning point in the development of demography?

There is no doubt that Malthus enjoys a more honourable place than Graunt in the history of demography. This is so despite, or because of, the stir that Malthus brought to the field of population studies. As his first biographer put it, 'He was the "best-abused man of the age" ... Malthus from the first was not ignored. For thirty years it rained refutations' (Bonar, cited by Flew, 1970: 48). Moreover, even though Graunt's book has probably been less read than Malthus's *Principle of Population,*[1] Petersen (1979: 58) is unlikely to have exaggerated when he writes: 'If we adopt the cynical definition of a classic, a work that everyone cites and no one reads, then the *Essay on Population* must be designated a superclassic' (Petersen, 1979: 58).

In any case, it is a fact that Malthus's *Principle of Population* is widely acknowledged as the inception of modern demography. The reasons for this are hardly well explained, and statements such as the following from Thomlinson (1976: 12) do not help at all: 'Malthus was the first man to take demography as his professional speciality'.

Instead of disputing the assertion that Malthus's *Principle of Population* represents a turning point in demography, it seems more important to try to understand the meaning of such a strong proposition. Unfortunately, once again one cannot turn safely to the existing textbooks and expect to find a convincing explanations. To give just an example, let us consider the following explanation from Thomlinson:

> Having outgrown the phase of amateurism (characteristic of most disciplines in their early period of development) and political arithmetic, and having acquired the trappings of quantification, demography was now ready to expand into a full-blown science. A turning point occurred in 1798 with the publication of Malthus's *Essay on Population*. Demography had now come of age. Here was a detailed, organized compilation of existing statistics, bound together by analyses of their causes and consequences (Thomlinson, 1976: 11).

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[1] Mostly Malthus's *Principle of Population* is referred to as Malthus's *Essay*, or his 'First Essay' to distinguish it from the 1803 edition and also to distinguish it from his *Principle of Political Economy*. My departure from orthodoxy is explained below.

But Annex A shows, Graunt's book made history in demography for exactly this reasons: Graunt's book is precisely 'a detailed, organized compilation of existing statistics, bound together by analyses of their causes and consequences'.[2] So, what is the nature of the turning point in 1798?

Thomlinson's explanation is wrong, denigrating the work of many predecessors of Malthus, particularly those to whom Malthus himself turned to borrow the available data in support of his argument. 'I have been led to this remark', Malthus wrote,

> by looking over some of the tables of Mr Suessmilch, which Dr Price has extracted in one of his notes to the postscript on the controversy respecting the population of England and Wales. They are considered as very correct, and if such tables were general, they would throw great light on the different ways by which population is repressed and prevented from increasing beyond the means of subsistence in any country. I will extract a part of the tables, with Dr Price's remarks ... For further information on this subject, I refer the reader to Mr Suessmilch's tables (Malthus, 1970:109-111).

The four small tables that Malthus provides between pages 109 and 111 are the only tables in the whole first edition; throughout the book he uses other information, quantitative and qualitative, but none of the data was produced, organized or compiled by him. In later editions Malthus provides much more statistical data, inclusive from his own trip observations and research. And yet, in statement just citation Malthus provides a glimpse of what seems to have been one of the most original features of his book: its detailed and penetrating argument, supported by the available data, but most importantly bound together by a 'Principle' never so well elaborated.

I should clarify the phrase 'never so well elaborated'; it would be wrong to give the impression that Malthus's principle of population came out of his own imagination in a special day of inspiration. As Malthus himself explains,

> The most important argument that I shall adduce is certainly not new ... It has been advanced and applied to the present subject, though not with its proper weight, or in the most forcible point of view, by Mr Wallace, and it may probably have been stated by many writers that I have never met with (Malthus, 1970: 69).

Hartwick's (1991) recent paper called 'Robert Wallace and Malthus and the ratios', provides a comprehensive review of the anticipations to Malthus's principle of population. Hartwick shows that by the end of the eighteenth century the application of the arithmetical and geometrical ratios to social phenomena was widespread. By then

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[2] A similar perspective can be found in Hauser and Duncan (1959: 13). More recently, Lucas also asserts that Malthus's *Principle of Population* became a landmark in population studies, 'partly because of his organised use of available data' (Lucas, 1994: 19-21). This contrasts, for instance, with Davis (1955: 541): 'Malthus' theories are not now and never were empirically valid, but they nevertheless were theoretically significant and, as a consequence, they hold a secure place in intellectual history'; or Lorimer's account of the first edition of Malthus's *Principle*:

> At this time he had not carried out any empirical investigations and had little familiarity with studies in this field. Thereafter, convinced of the validity of his thesis and finding himself in the midst of a lively controversy, he eagerly explored all aspects of population changes. His later writings on population, presented as successive editions of the essay, covered a wide range of information. Like Süssmilch, he had sincere respect for objective evidence. He never distorted his accounts to serve his theoretical interests (Lorimer, 1959: 140).

nobody cared who had done it for the first time, though it is unfortunate that Hartwick, in his otherwise well documented review of earlier anticipations of Malthus's ratios, does not single out the significance of Graunt's mathematical method of proportional estimates. However, Hartwick provides a reasonable basis to allow readers of today to understand why Malthus’s *Essay* provoked more response than Wallace's books of 1753 and 1761.

If the proposition should stand that 1798 was a turning point for demography, something more significant must have occurred than Thomlinson lets his readers know. It is with this expectation that I shall dig deeper into Malthus’s theoretical framework, and in particular its most significant part, the principle of population. On this, there are only two options worth considering. One is the conventional and dominant approach to Malthus’s principle of population. That is, demographers have generally selected and stuck with whatever they found consistent with the conventional framework of demographic analysis, and they dismissed, or simply ignored, all the rest. This is apparent when one considers that while contemporary demographers have found much excitement in debating issues closely related to the first of Malthus's two postulates, they continue to dismiss the second as romantic and useless.

The second option is to admit that the 'principle of population' only makes sense in the way Malthus formulated it in 1798. I have decided to trust this second option, and thus try to come to terms with the idea that 1798 might have, in fact, represented a turning point in the development of demography.

Loss of innocence: what did Malthus do to demography that Graunt did not?

As I have argued in the essay on Graunt (see Annex A) there is no danger that Malthus's place in the history of demography will ever become insecure if one challenges the misleading portraits of his predecessors that still prevail in demography today. Indeed, a reassessment of these predecessors seems to be the only reasonable way to make sense not just of the important contributions before Malthus, but also of the originality of his own work.

The turning point produced by Malthus in 1798 is epitomized by a single word in the full title of his book: *An Essay on the Principle of Population as it affects the future Improvement of Society, with Remarks on the Speculations of Mr Godwin, M. Condorcet, and Other Writers*. The word is ‘Principle’. Curiously, it is perhaps no coincidence that this word is the one that is usually left out whenever authors abbreviate the most common version of the above full title: *An Essay on the Principle of Population*. The shortest abbreviations used are *Essay* and *Essay on Population*; only seldom do a few authors use a third abbreviation, *Principle of Population*, which is the one that reflects adequately the essence of the book.

If the scientific study of population was ever a neutral exercise of innocent 'Observations', Malthus brought that age of innocence to its end. With just one stroke, demography came of age, not for the reasons indicated by Thomlinson, but because of

Malthus's convincing proposition that population has a 'Principle' of its own. ‘It is a perfectly just observation of Mr Godwin’, so Malthus remarked about one of the authors who motivated him to write his book,

> that, ‘There is a principle in human society, by which population is perpetually kept down to the level of the means of subsistence’. The sole question is, what is this principle? Is it some obscure and occult cause? Is it some mysterious interference of heaven which, at a certain period, strikes the men with impotence, and the women with barrenness? Or is it a cause, open to our researches, within our view, a cause, which has constantly been observed to operate, though with varied force, in every state in which man has been placed? Is it not a degree of misery, the necessary and inevitable result of the laws of nature, which human institutions, so far from aggravating, have tended considerably to mitigate, though they never can remove? (Malthus, 1789/1970: 139).

In the first chapter of his book, Malthus provides a short answer to this remarkable array of questions:

> I think I may fairly make two postulata.
> First, That food is necessary to the existence of man.
> Secondly, that the passion between the sexes is necessary
> and will remain nearly in its present state (Malthus, 1970: 70).

Ever since this statement became public, the expression 'principle of population' has been associated with the name of Malthus. Indeed, if one accepts that Malthus's main argument and conclusions can be traced to the above two postulata, the principle of population may have been Malthus's single most important and revolutionary contribution to demography.

To study population as a collection of isolated individuals, and then search for some regularities and patterns in its data, is what Graunt did in his *Observations*. Curiously, just like the term 'Principle' in Malthus's book, the term 'Observation' reflects perfectly the essence of Graunt's approach. Observation refers simultaneously to seeing or watching and also to making remarks on the things noticed. From this point of view population is assumed to be a purely statistical aggregate with a certain order. In fact, this approach of population, inherited from Graunt's *Observations*, remains remarkably powerful today, to the extent that many demographers still believe they should be committed only to the naive seeing and the dispassionate noting and accounting; in this respect, contemporary demographers are more Grauntians than Malthusians.

Malthus's demographic approach is substantially different from Graunt's because he considers population more as a system that orders itself in accord with a certain principle; he sees population not as a pure collection of individuals, as is generally the case when it is used in statistical terms. 'In statistical usage', Newell writes, 'particularly when talking about sampling, it [population] means the universe of units under consideration, which may be people, light bulbs, rats or whatever’ (Newell, 1988: 9).

Graunt, like other earlier students of population, never mixed up people with light bulbs or rats; but it was only Malthus who made it fully explicit that from a demographic perspective population is more than a statistical collection of individuals. Human population is a set of individuals in a given area and, most importantly, bound together by a certain principle.

The notion of a 'principle of population' should lead demographers to recognize a fundamental distinction between what may be called 'demographic order' and 'demographic design'. While the former refers to mere regularities or patterns in demographic processes, the latter refers to a designed order.[3] The latter indicates that demographers should aspire not only to providing descriptions of regularities and patterns in the population data, as Graunt did, but also to search for and explain the driving forces, or the principles, that order population, as Malthus proposed.

This seems to be already a more meaningful justification for the conjecture that 1798 was a turning point in the development of demography. In particular, it has the potential to explain better why despite Malthus's own retreat from his initial formulation of the two postulata, the idea that population is driven by a principle has never vanished either from his reasoning nor from the image that the scientific memory today retains of his contribution to the study of population.

As Malthus acknowledges, and Hartwick demonstrates even more convincingly, the idea that population is moved by a principle was already in debate for quite some time. Malthus received the full credit for what may be called a landmark scientific revelation in the social sciences, the principle of population; this was due to the broad circumstances described by Hartwick (1991: 310-311) and , in particular, as Malthus himself explains, because of his 'proper weight' and 'most forcible point of view'.

In short, Thomlinson and many other contemporary authors seem right in speaking about a significant turning point in 1798, but for the wrong reasons. Malthus's book became a landmark in population studies not because of his detailed, organized compilation of existing statistical data but because of his iconoclastic 'principle of population'. He did not assume that population evolved because of some 'occult cause' or 'mysterious interference of heaven', in which he in fact believed strongly. Instead, he provided a self-evident mechanism that makes population dynamics look inevitable. He proposed to study population as being designed and determined mainly by two powers: to survive (that food is necessary), and to reproduce (that the passion between the sexes is necessary). But Malthus went even further, in that he outlined a specific operational mechanism and specific hypotheses to be tested by empirical observation.

Why has Malthus's principle of population been so successful?

The success of Malthus's theoretical framework has puzzled many authors. Among his contemporaries, Godwin was quite open about this: 'Mr. Malthus's theory is certainly of a peculiar structure, and it is somewhat difficult to account for the success it

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[3] The distinction between order and design has been increasingly applied in other fields, particularly in recent years in discussions upon the significance of Darwin's theory (Ridley, 1993; Dennett, 1995a, 1995b).

has met with' (Godwin, 1820: 22). Over the years, 'the success' that Godwin spoke about remained unaccounted for among Malthus's foes and friends alike. Likewise, the simplicity of Malthus's theory construction as much as its powerful theoretical scheme continues to fascinate and puzzle contemporary authors (see, for instance, Banks, 1954: 12-31; Flew, 1970; Hartwick, 1991).

Flew's introduction to his edition of Malthus's *Principle of Population* refers to such simplicity with sympathy. However, Flew's discussion remains overwhelmed by the misunderstandings and misrepresentations surrounding Malthus's theory construction. He begins his discussion with a comment on the distortions of Malthus's views, 'a household word, but misunderstood'; after a brief review of Malthus's life and work as well as an explanation and examination of his conceptual structure, Flew returns to his main concern: 'Malthus and Darwin: Malthus and Marx' (Flew, 1970: 48-54), and 'the achievement of Malthus' (Flew, 1970: 54-55). Besides a reference to the inspiration that Malthus's book provided to the development of the theory of the origin of species, Flew considers that 'the main achievement of Malthus appears to be practical': in other words, 'to have brought questions of national population and individual family size within the sphere of morality and prudence, of policy and decision' (Flew, 1970: 55). Surprisingly, Flew reduces the relevance of Malthus's theory in current times just to counter the 'bad faith'; he considers Malthus's main achievement

> to have brought questions of national. population and individual family size within the sphere of morality and prudence, of policy and decision. Of course, to say this is not to say that individuals and organizations do not say still pretend that this is not so. On the contrary, it is precisely because they very often do make these pretences, and do show this form of what Sartrean existentialists would call bad faith, that Malthus remains so relevant (Flew, 1970: 55).

One is left with the impression that Malthus's principle of population has been successful more because of the stir and controversies it has created than the other way around; that it is its powerful insight, perhaps its 'peculiar structure', as Godwin put it, that continues to provoke today, as much as in the past, all sorts of interpretations and controversy.

In turn, Hartwick in his 1991 paper explicitly wonders: 'why was Malthus's *Essay* received in 1798 with more attention or fanfare than was Wallace-1735 or Wallace-1761?'. Hartwick's answer stresses, first, the stronger popular concern about population growth in Britain in 1798, as opposed to 1735 or even 1761. Secondly, 'Wallace blunted the impact of his work on population by disseminating it in two distinct books instead of one': Wallace-1753 and Wallace-1761. Thirdly, Hartwick asserts,

> Malthus introduced those special terms for which his ideas became linked - 'geometrical ratio' and 'arithmetic ratio' - lending a scientific quality to his work. The 'jingle' could be linked to the 'principle' to the mathematical terms. Moreover the pessimistic tone of the *Essay* may well have been in consonance with the mood in Britain in 1798 (Himmelfarb 1984: 130). There is an absence of millenarian rhetoric (Hartwick, 1991:311)

Demographers, like scientists in other fields, have often had difficulty to accommodate to their perception that very simple analytical frameworks should just lead to elementary ideas. In this respect, Malthus's simple theory construction has defied the intuitive expectations of demographers' commonsense. As the quotation in the epigraph of this chapter indicates, Malthus's work on population can be and has in fact been a source of diverse theoretical developments. Beyond that, although the power of Malthus's theoretical scheme should not be explained only in terms of its simplicity, more often than not this feature seems to be associated with the success of scientific theories in science in general, and in demography in particular.

Of course, the assertion made some paragraphs above that population should be studied as an ordered but also designed system is likely to have pleased some readers and irritated others, though perhaps in both cases for the wrong reasons. Those who believe on divine creation of the world may conclude that the language of design makes little sense without the existence of a Designer. This was the interpretation of most predecessors and contemporaries of Malthus, including himself; as I have shown above, Arbuthnot's statistical demonstration of the intervention of Divine Providence in the constant regularity observed in the sex ratio was motivated by Creationism. In turn, other authors are less interested in knowing whether or not the world was designed by a superior Being; some can even get easily irritated with the language of design because the notion of design seems guilty for the troubles that certain faiths have sometimes caused to science development.

Between the above two extremes, Darwin seems to have found an admirable middle ground from which demographers should benefit as much as he benefited from Malthus's *Principle of Population*. 'He was the first', so writes Ridley (1993: 6), 'to realize that you can abandon divine creation of species without abandoning the argument from design'. Or as Dennett (1995b: 50) puts it, 'Darwin's novel mixture of detailed naturalism and abstract reasoning' offered 'a skeptical world what we might call a get-rich-*slow* scheme, a scheme for creating Design out of Chaos without the aid of Mind'.

Malthus never abandoned his belief in the intervention of a Being. However, with regard to design and as far as his theory construction is concerned he appears to be closer to Darwin than to Graunt, Arbuthnot and Godwin. In contrast to the latter, Malthus offered a self-evident account for the dynamics of population; in fairness, from the point of view of Graunt's *Observations* population dynamics may be ordered but not designed. Yet the design mechanism implicit in Malthus's 'Principle' does not require 'miraculous additions at any one stage', especially when seen through the lenses of Darwin's approach.[4]

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[4] Dennett quotes a letter from Darwin to the British geologist Charles Lyell shortly after the publication of *Origin*:

> I would give absolutely nothing for the theory of Natural Selection, if it requires miraculous additions at any one stage of descent ... If I were convinced that I required such additions to the theory of natural selection, I would reject it as rubbish (Darwin, cited by Dennett, 1995a: 37).

It may be true that organisms in general, and humans in particular, 'do not seek to evolve', as Cohen and Stewart put it (1994: 431), but humans at least clearly seek to survive and reproduce. Humans thus seem to be as much goal-seekers as goal-finders.[5] Demographic change and evolution seems to be the outcome of the two powerful goals of any population, survivorship and reproduction.

This seems to have been the first important logical insight that Darwin gained from Malthus's *Principle of Population*: 'Malthus opened Darwin's eyes to the fact that human beings, too, are bound by the laws of nature' (Cohen and Stewart, 1994: 107; see also Dennett, 1995b: 40, 41). Perhaps, Darwin's new insight may now be used to search for the set of contingencies and mechanisms that design population.

Some may argue that Malthus did not make as good use of the historical approach, nor document his ideas so exhaustively, as Darwin. In spite of that, his vision was certainly clear and powerful enough for Darwin to get the necessary insight to formulate his own principle of natural selection. ‘One day’, so Darwin recalls,

> something brought to my recollection Malthus’s *Principle of Population*, which I had read about twelve years before. I thought of his clear exposition of ‘the positive checks’ to increase ... which keep down the population It then occurred to me that these causes or their equivalents are continually acting in the case of animals also ... Why do some die and some live? And the answer was clearly, that on the whole the best fitted live ... The more I thought over it the more I became convinced that I had at length found the long-sought-for law of nature that solved the problem of the origin of species (Darwin, cited in Flew, 1970: 51).[6]

Just as Darwin borrowed from Malthus a logical insight that turned out to be decisive for his own theory construction in biology, demographers of today should accommodate and update Malthus's principle of population according to the progresses of biological science that followed from Darwin's work.

The principle of population as an algorithmic process

Hartwick (1991) called the operational mechanism which Malthus derived from the principle of population, that is the arithmetic and geometric progressions, a theorem. However, the term 'theorem' means a proposition that can be proved logically from a set of basic assumptions, and this does not convey the full dimension of Malthus's theoretical scheme.

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[5] Cohen and Stewart (1994: 431) argue that although 'Darwinian evolution has a dynamic ... 'A dynamic does not necessarily imply a purpose .. The existence of attractors does not imply that dynamical systems are goal-seekers: on the contrary, they are goal-*finders*, which only recognize what the "goal" is when they have found it'. This seems to be a rather blind and absolutist view as far as the behaviour of populations dynamics is concerned.

[6] Curiously, Darwin was one of the few authors who referred to Malthus's book through the abbreviation that I consider above more adequate, *Principle of Population.*

Perhaps an explanatory device more adequate here is the concept of 'algorithm'. Recently this working concept has been used to account for the success of Darwin's theory with regard to aspects closely related to Malthus's principle of population. 'Darwin succeeded', so Dennett (1995a: 36) explains, 'not only because he documented his ideas exhaustively but also because he grounded them in a powerful framework. In modern times, he had discovered the power of an algorithm'. Dennett defines an algorithm as

> a certain sort of formal process that can be counted on - logically - to yield a certain sort of result whenever it is 'run' or instantiated. Algorithms are not new, and were not new in Darwin's day. Many familiar arithmetic procedures, such as long division or balancing your checkbook, are algorithms, and so are the decision procedures for playing perfect tic-tac-toe, and for putting a list of words into alphabetical order. What is relatively new - permitting us valuable hindsight on Darwin's discovery - is the theoretical reflection by mathematicians and logicians on the nature and power of algorithms in general, a twentieth-century development which led to the birth of the computer, which has led in turn, to a much deeper and more lively understanding of the powers of algorithms in general (Dennett, 1995b: 50).[7]

Just as Darwin can be said to have discovered the power of an algorithm for the evolution of species (Dennett, 1995a: 36), some decades before him Malthus discovered the power of an algorithm for population dynamics. Not that Malthus invented, as Hartwick very well demonstrated, the principle of population all by himself; but he operationalized and systematized an analytical framework in which the principle of population is the most powerful part. As Flew (1970: 17) put it, 'This scheme has to be mastered by anyone who wants to come to terms with what Malthus really said, and it is this which constitutes his main permanent contribution'. However, this 'main permanent contribution' cannot be well appreciated if the description of the whole structure of Malthus's theoretical scheme is reduced to an elegant and simple logical construct which may be tested by empirical observation. The notion of 'algorithmic process' makes it possible to account for the simplicity as much as the success of Malthus's principle of population, mainly because it helps one to appreciate, for instance, the unifying theoretical mechanisms in the six stages that Flew (1970: 17-48) identified in Malthus's theory construction (depicted in Figure 1.5.1).

The sequence in Malthus's theory construction illustrated in Figure 1.5.1 can be seen not as one algorithm but as a scheme of related 'sorting, winnowing and building'

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[7] As Dennett (1995b: 50) writes:

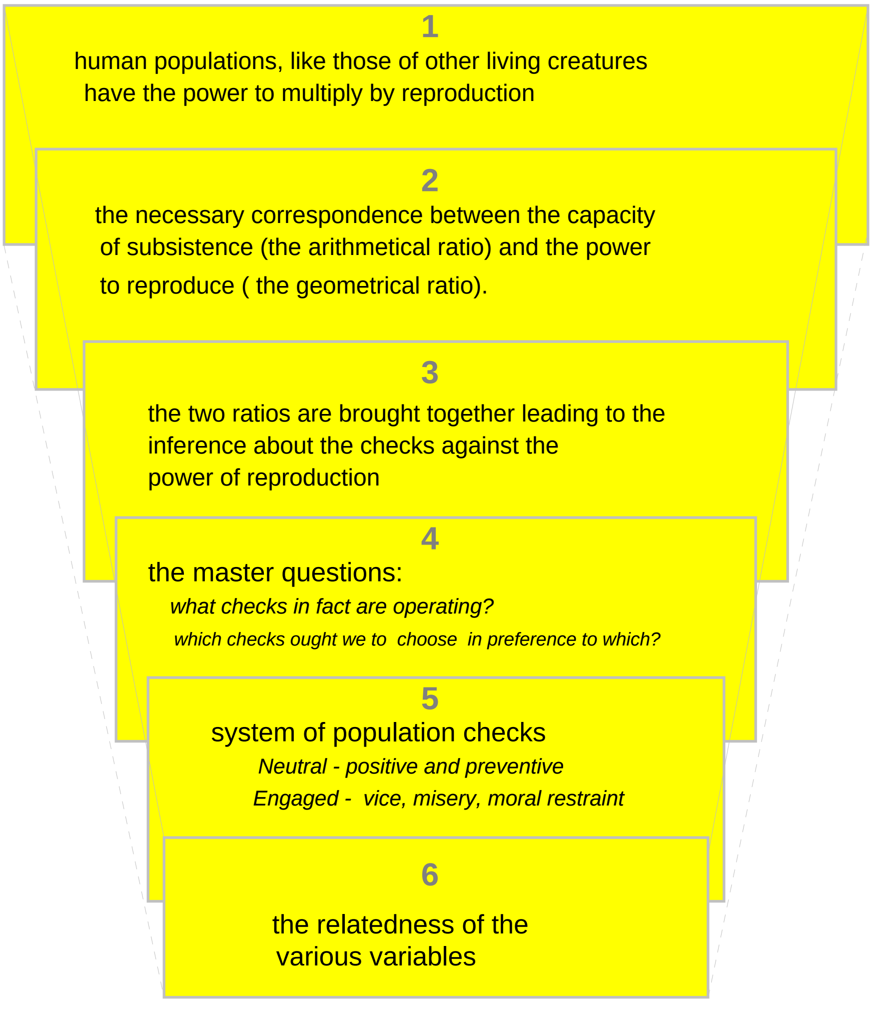
> The idea that an algorithm is a foolproof and somehow 'mechanical' procedure has been present for centuries, but it was the pioneering work of Alan M. Turing, Kurt Gödel, and Alonzo Church in the 1930s that more or less fixed the current understanding of the term ... the algorithms that will concern us have nothing particular to do with the number system or other mathematical objects; they are algorithms for sorting, winnowing, and building things (Dennett, 1995b: 50, 52).

Dennett identifies three key features of algorithms: 1) *Substrative neutrality* - 'The power of the procedure is a result of its logical structure, not the materials that happen to be used in carrying it out'; 2) *Underlying mindlessness* - ' Although the overall design of the procedure may be brilliant ... each constituent step is utterly simple. The recipe requires no wise decisions or delicate judgments on the part of the recipe reader'. 3) *Guaranteed results* - 'Whatever it is an algorithm does, it always does it, provided the algorithm is executed without misstep. An algorithm is a foolproof recipe' (Dennett, 1995a: 36, 1995b: 50-51).

algorithms. As Dennett (1995b: 41) and other authors acknowledge, Darwin drew on Malthus's reasoning on the power of organisms to multiply by reproduction from 'its political birthplace' and elevated it to a more abstract and general perspective. That is, Darwin made the best use of the 'substrate neutrality' (Dennett, 1995b: 50) that he grasped in Malthus's theoretical scheme.

Moreover, Malthus considered that population has its own recipe of mechanisms which establish a certain correspondence between the capacity to survive and reproduce. This happens regardless of whether people are individually aware of such a process. That is, there is an 'underlying mindlessness' (Dennett, 1995b: 51) in demographic phenomena, which is mostly asserted to be found in situations that demographers of today call 'natural fertility' or the absence of deliberate birth control.

Figure 1.5.1 Malthus's demographic theoric construction



Source: Flew, 1970:17-31

Finally, whatever it is that the demographic algorithms do, they always lead to a certain outcome, or 'guaranteed results' (Dennett, 1995b: 51). These results focus around what Flew (1970: 22) called Malthus's master question, which is 'as much theoretical as practical':

> The speculative question is: what checks in fact are operating? The practical question is: what is to be done about the principle of population, and, in particular, which checks ought we to choose in preference to which (Flew, 1970: 22)

This approach should have many and important theoretical and methodological implications. While this thesis cannot explore them further, at least this brief discussion should help to support my argument that conventional demography has yet to explore adequately and fully the scientific implications of Malthus's principle of population for demographic analysis.

‘Elegance and experience’ ... and anything else?

The relationship between deductive and inductive methods is seldom found in mainstream demographic literature. About a decade ago, Wunsch (1984: 1) acknowledged the paucity of essays devoted to this and other aspects of the acquisition of knowledge, from the point of view of either the philosophy or the sociology of science.

Yet the situation will perhaps appear less odd if one considers that discussion of epistemological and methodological issues in demography is often but camouflaged by euphemistic terms. For instance, Wrigley (1986: 46-64) discusses the issue of deduction and induction with regard to Malthus's theory construction through two dummy-concepts: ‘elegance' and 'experience'. In this article, Wrigley seems to be concerned in sketching a catch-all portrait of Malthus's work, one which should please simultaneously both the deductivists and the empiricists. However, while he attributes Malthus's tendency to analyse society through 'the principles of deductive logic' to his mathematical background, his article is clearly more sympathetic to Malthus’s appeal to experience. 'He [Malthus] valued elegance’, writes Wrigley, ‘but gave pride of place to experience'; or, 'it is highly characteristic of Malthus to place an appeal to empirical evidence higher than any other authority in determining an issue' (Wrigley, 1986: 46-47). Wrigley's account is not original, for even Malthus's contemporaries such as Godwin grasped it:

> It has been agreed among the best philosophers in Europe, especially from the time of Lord Bacon to the present day, that the proper basis of all our knowledge respecting man and nature, respecting what has been in times that are past, and what may be expected in time to come, is experience. This standard is peculiarly applicable to the subject of population. Mr. Malthus seems in one respect fully to concur in this way of viewing the subject. There are two methods of approaching the question, the first, by deriving our ideas respecting it from the volumes of sacred writ, and the second, by having recourse to such enumerations, statistical tables, and calculations, as the industry of mere uninspired men has collected; and Mr. Malthus has made his election for the latter. Dr. Robert Wallace ... has taken the opposite road ... Mr. Malthus, on the contrary reposes throughout his Essay on the pure basis of human experience (Godwin, 1820: 22-23).

Yet to insist on a clear-cut dichotomy between the two methods in Malthus's work seem rather inadequate.[8] In reading Malthus's *Principle of Population* the immediate difficulty is to find out where Malthus's deductive reasoning ends and where his inductive approach begins. On the one hand, the theoretical scheme that emerges resembles a classical deductive logic construction such as the following:

1. Postulata or laws	1. That food is necessary to the existence of man; 2. That the passion between the sexes is necessary and will remain nearly in its present state.
2. Initial conditions	1. The power of population is indefinitely greater than the power in the earth to produce subsistence for man; 2. Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio
3. Predictions and explanations	* In the United States of America, where the means of subsistence have been more ample, the manners of the people more pure, and consequently the checks to early marriage fewer, than in any of the modern states of Europe, the population has been found to double itself in twenty-five years.

Malthus started by formulating his principle in the form of classic deductive logic: first, he set what he called postulata or laws; then he identified the specific statements or initial conditions supposed to describe the details of the set-up under investigation; and, at thirdly he derived some explanations and predictions with the objective 'to deduce that therefore checks must already be operating; and to raise questions about their nature and interaction' (Flew, 1970: 31).

On the other hand, Wrigley is also correct in saying that Malthus placed 'an appeal to empirical evidence higher than any other authority'. Perhaps for this reason Chalmers (1988) could charge Malthus with being a ‘naive inductivist’; even when Malthus invites the reader to take his two postulata for granted, he does so on the grounds of experience of life; the first chapter in the first publication closes thus:

> I have thus sketched the general outline of the argument, but I will examine it more particularly, and I think it will be found that experience, the true source and foundation of all knowledge, invariably confirms its truths (Malthus, 1970: 72).

In short, the most accurate picture of the demographic theory outlined in the different editions of his *Principle of Population* is that Malthus never relied on empirical

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[8] In 1955, Davis discussed the theory construction of Malthus in terms of the following four elements:

> 1. A frame of reference. 2. A set of deductive propositions. (These concern the relationships between variables defined in the frame of reference.) 3. A set of empirical propositions verified by disciplined observation. 4. Crude empirical propositions based only on commonsense observation. The third element must be sharply distinguished from the fourth one, the ‘rough empirical generalizations’ based on immediate experience and commonsense ...*The Malthusian Frame of Reference.* The frame of reference is the least understood part of scientific theory (Davis, 1955: 542).

experience without systematic logical demonstration, nor did he deduce his postulata without referring to empirical evidence.

This picture stands on the standard Newtonian model about which Malthus was explicitly enthusiastic (Flew, 1970: 9, 31-32, 39, 278; Malthus, 1970: 126, 213). However, the conventional comparison between rationalist and empiricist methods overlooks the theoretical role of the 'algorithmic process' considered above. This issue cannot be debated in more detail here, but it can be conjectured at least that if Dennett (1995b: 48) is right in asserting that 'Darwin should be seen, rather, as postulating that evolution is an algorithmic process', then the implications of this claim for demographic analysis deserve careful consideration. In particular, if the algorithmic process 'permits us to do justice to the undeniable *a priori* of Darwin's thinking without forcing it into the Procrustean (and obsolete) bed of the nomologico-deductive model' (Dennett, 1995b: 48), this should have significant implications for any review of Malthus's population theory.

Of course, if one had to decide just between the two classical opposites, as a black or white approach, Malthus's methodology would have to be tagged as a mixed or a deductive-inductive method. But this assessment overlooks the paramount role that the 'principle of population' and its specific mechanisms play throughout all stages of his theory construction. In other words, to paraphrase Dennett it can be asserted that the standard deductive and inductive reasoning does no justice to the undeniable *a priori* flavour of Malthus's reasoning standing on his dual algorithmic principle of population. A crucial feature here is the twofold character of the principle of population. As I will show in the next section, the rejection of the unity between the two postulata in Malthus's principle is the same as suggesting that population can survive without reproducing, or perhaps it can reproduce without surviving.

Malthus's withdrawal of the principle of population

The entire Malthusian principle of population has been controversial ever since it was first made public. However, when the two postulata are compared, it becomes immediately apparent that they have received different and unequal attention. The first postulate has been by far the most extensively investigated and has led to diverse areas of research: some purely theoretical, others mainly mathematical, and still others of an empirical nature. In each of these areas countless articles have been written offering proofs, counterproofs, confirmations, and refutations.

In contrast, the second postulate has been met with much contempt. In particular, Malthus's remark that the passion between the sexes is not only necessary but will remain 'in its present state' has provoked irritation among his supporters, and derision among his critics. But before considering such responses Malthus's own attitude towards the postulates needs to be mentioned.

In the second and later editions of *The Principle of Population*, Malthus dropped the two postulata cited above. He never made the reason for this change explicit, though it is likely that it was the same as the one that led him 'to suppose another check to population possible, which does not strictly come under the head either of vice or misery'. On this, Malthus (1989[1803]: 3) explains: 'I have endeavoured to soften some of the harshest conclusions of the first essay'.

Indeed, Malthus not only softened some of the harshest conclusions, more significantly than that he concealed the two postulata, a change that was not missed by his critics:

> But, as Mr. Malthus has retained to the last all the conclusions drawn from these *postulata,* and as his argument respecting the impracticability of a permanent state of equality among human beings, founded upon the parity of these two propositions, stands in the Fifth Edition *verbatim* as it stood in the first, I cannot myself consent to his withdrawing his premises, at the same time that he retains the inferences built upon them (Godwin, 1820: 31-32).

At the end of this same book, Godwin (1820: 525-537) dedicates a chapter to a more detailed discussion 'respecting the nature of man upon which the Essay on Population is constructed'.

> The fundamental error of Mr. Malthus’s system, as far as the constitution and structure of man, independently of the geometrical ratio, is concerned, seems to me to lie in two propositions, which were explicitly stated in the first edition of his book, but which he has since withdrawn ... I would be the last man in the world to deny an author the benefit of his after-thoughts. If Mr. Malthus has since discovered, that food and the passion between the sexes are necessities not exactly alike and of equal force, that were well. But I cannot consent to his withdrawing his premises, while he maintains the conclusions built upon them. This seems to be one of the instances of 'a passage expunged, that the author might not inflict an unnecessary violence on the feelings of his readers' (Godwin, 1820: 526).

Here is a crucial issue to account for. Malthus dropped some of the wording and, specifically, his initial statement containing the two postulata that appeared in the first edition of the *Principle of Population*. But while in his mature demographic work Malthus openly withdrew the wording of his two postulates, it is visible that he seemed more concerned in strengthening the conclusion he built upon than with new data than renounced the content of his principle of population. In Malthus's Preface for the 1803 edition of the *Principle of Population* one can read his own remarks comparing the new with the first edition :

> In its present shape it may be considered as a new work, and I should probably have published it as such, omitting the few parts of the former which I have retained, but I wished it to form a whole of itself, and not to need a continual reference to the other ... I should hope that there are some parts of it, not reprinted in this, which may still have their use; as they were rejected, not because I thought them all of less value than what has been inserted, but because they did not suit the different plan of treating the subject which I had adopted ... Throughout the whole of the present work, I have so far differed in principle from the former, as to suppose another check to population possible, which does not strictly come under the head either of vice or misery; and, in the latter part, I have endeavoured to soften some of the harshest conclusions of the first essay. In doing this, I hope that I have not violated the principles of just reasoning, nor expressed any opinion

> respecting the probable improvements of society in which I am not borne out by the experience of the past. To those who shall still think that any check to population whatever would be worse than the evils which it would relieve, the conclusions of the former essay will remain in full force (Malthus, 1817/1989: 2-3).

In his 1817 preface to the fifth edition of the *Principle of Population* Malthus enumerated 'the principal additions and alterations made in the present edition', and concluded: 'They consist in a considerable degree of the application of the general principles of the Essay to the present state of things' (Malthus, 1817/1989: 7) (see also Boxes 1 and 2). This indicates that Malthus's 'principle of population' was born almost in mature form in 1798, though as any field that is alive has changed and improved. But this seems not enough to conclude that Malthus thoroughly abandoned the twofold character of the principle of population and, thus, 'Population Malthus' can be clearly split in two as Demeny (1995: 8) proposed recently: 'The young Malthus was of course a biological determinist ... But the mature Malthus was a social scientist'.

How some demographers deceive themselves about paradigms

If the majority of the demographic community accepts 1798 as a turning point for demography, it is somewhat surprising that at least those demographers who consider the Kuhnian notions of ‘paradigm’ and 'paradigm shift' useful to evaluate the state of demographic theory continue to dismiss Malthus's principle of population as a fundamental demographic paradigm. Some authors, both demographers and other social scientists, who have explicitly considered the 'ability to induce paradigm shift', to use an expression suggested by Liao (1990), have generally failed to explore adequately to what extent Malthus’s principle of population provoked a paradigm shift in demographic theory.

Over the decade of the 1990s Liao (1990, 1992, 1993) has proposed a 'three-dimensional framework of theory construction' and applied it to demographic theory, namely stable population theory, relative deprivation theory in migration, and fertility theory. According to Liao scientific theory can best be constructed in three dimensions: confirmation or falsification, scope condition, and ability to induce paradigm shift. Liao's framework has already provoked some severe comments from sociologists who questioned its usefulness either to theory construction or the cumulative development of theory in sociology (Harris and Walker, 1992; Willer, 1992).

This is not the appropriate place to discuss in any detail the flaws of Liao's assessment of theory construction in demography. But while several passages in this thesis should expose Liao's misconceptions concerning the history and progress of demographic ideas, at least his attempt to apply the Kuhnian paradigm is of interest for this section.[9]

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[9] I am less certain that reviewers should exercise any right of censorship, or even soften what Harris and Walker (1992: 111) proposed: 'must bear responsibility for the publication of ideas that are embryonic at

Liao indicates no appreciation for Malthus's predecessors, including Graunt's model of demography's whole design, for reasons apparent in the following statement:

> The first systematic principle explaining human population trends was proposed by Malthus (1830), before whom there had been an absence of general fertility theory - the field being in a Kuhnian pre-paradigmatic state. The essence of his principle of population rests on two propositions ... Malthus attributed the tendency for population increase to a constant, 'the passion between the sexes' (Coale 1979). The influence of this Malthusian paradigm was tremendous: It helped withdraw amendments to the Poor Law designed to encourage large families, and ... [led] to the first census of England and Wales (Coale 1979) (Liao, 1990: 93-94).

Moreover, Liao’s rejection of the claim that demographers are atheoretical is condescending and unconvincing: 'Demography, however, is not that atheoretical as it first appears', Liao asserted in a paper published in 1993.

> Many demographers approach their research questions in a sociological, economic, or another social scientific perspective. Often theories from other disciplines are adopted, adapted, or synthesized to explain population-related phenomena. Empirical generalizations may also serve as theoretical guiding principles for later researchers. *Indeed, researchers construct pure demographic theories as well* (Liao, 1990: 389) [emphasis added].

Clearly, this defence of demographic theorizing is far from convincing, mainly because Liao (1993: 389) suggests that at least 'the type of research many demographers conduct - empirical descriptions and generalizations of demographic trends, patterns, and processes rather than theory construction and testing using demographic data' exists independent of any theory. In this context, it is not surprising that Liao even considers Malthus's theory pre-paradigmatic: 'In reference to the theoretical model of stable population theory relating major demographic processes of fertility, mortality, and age distribution, Malthusian theory is itself pre-paradigmatic (Liao, 1993: 398).

Perhaps a more striking debate on paradigms and Malthus appears in a book edited by Coleman and Schofield (1986), *The State of Population Theory: Forward from Malthus*. 'Demography is still far from possessing a central paradigm linking small-scale and large-scale processes', Schofield and Coleman (1986: 11) write in the introduction to this book which is a collection of essays selected from a conference of the British Society for Population Studies, held in 1984, to commemorate the 150th anniversary of Malthus’s death.[10] Of course, the occasion justified and motivated many references to Malthus's theory:

> In the course of these brief remarks on the nature of demography and population theory the name of Robert Malthus has been mentioned several times. Malthus was the first to develop a total population system operating on specified, if elementary, rules relating population behaviour to the social, economic and moral context. Although the idea of such systems, together with the nature of their feedback

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best'. If '[s]cientific knowledge grows by means of the sucessive reformulation of theories that are addressed to the explanation of empirical phenonema', as Harris and Walker (1992: 116) maintained, the idea that embryonic ideas should not be exposed to the wide public is rather interesting.

10 For a review of Coleman and Schofield's book see McNicoll (1988).

> mechanisms and their effect on human society and economic development, was raised so long ago, we are still very far from answering all the questions which Malthus posed (Schofield and Coleman, 1986: 6).

Undoubtedly, this statement is much more accurate than those I have commented on earlier. But contrary to Thomlinson (1976: 11) and Lucas (1994a: 19-21), Schofield and Coleman use expressions such as 'paradigm' and 'central paradigms' which in the philosophy of science have a different and stronger connotation than, for instance, 'turning point'. So, it becomes more striking that they do not recognize Malthus's principle of population as one, if not the most, important paradigm after Graunt's own paradigm; nor do they explore the dimensions of Malthus's principle of population, and in particular its breakthrough from Graunt's *Observations*. On the latter they made just this remark:

> At the beginning of modern population theory, Malthus needed to go some way towards the development of stable-population theory (Coale 1979) in order to advance his argument. The absence of any such interest by Graunt 250 years earlier (Kreager, 1980) may help to explain why his work remained at the level of 'Observations' rather than ascending to the pretension of a 'Principle' (Schofield and Coleman, 1986: 6).

However, this statement is inaccurate. Malthus's interest in developing certain methods, such as those that anticipated Lotka's stable-population theory, was determined by his 'Principle' not the other way around. Likewise, the fact that Graunt's study of population remained at the level of 'observations' can be better understood by the absence of a 'Principle' than any mysterious interest which has not ascend 'to the pretension of a 'Principle''.

Schofield and Coleman's use of the term 'pretension' should also not pass without notice. Some paragraphs before, Schofield and Coleman (1986: 2) cast strong doubts on the possibility of demography possessing a central paradigm:

> Universal theory or generalizations which link small-scale with large -scale processes, such as 'grand unified theories' in physics or evolutionary theory in biology, may not be possible in demography. Especially in the social sciences, such generalizations may not even be falsifiable (Schofield and Coleman, 1986: 4).

Remarkably, given the occasion, the authors proceed as if the celebration had nothing to do with Malthus. Following the assertion just cited, Schofield and Coleman turn their attention to Sauvy's *General Theory of Population* (1969), as 'one of the few attempts to bring together ideas in demography'. They pass over Malthus's principle of population, and assert that Sauvy's project was 'an adventurous compilation of generalizations on a broad front, but there is no central paradigm which links all the ideas together'. This did not admit the possibility that Sauvy could have been less adventurous if his so-called 'compilation of generalizations' had been consistently placed and bound by Malthus's principle of population.

A major definitional problem impedes any meaningful usage of the term paradigm in demography. What do demographers really mean by a paradigm in their field? Schofield and Coleman's discussion arguably reduces the term paradigm to emptiness. They speak of a 'central paradigm' as if it was an empty concept that awaits for someone to fill it with a relevant demographic content.

But these authors are not alone; in fact, they have basically echoed the view expressed by Wunsch in 1984. In his paper, Wunsch (1984) is openly critical of any pretension to 'universal theories' in demography. 'Indeed, one should even be suspicious', Wunsch writes, 'of universal theories in the social sciences, as they are unspecific and probably not falsifiable' (Wunsch, 1984).[11]

Schofield and Coleman (1986: 7) appear not so convinced and, in fact, they hesitate between Wunsch's advice and their own anxiety for a 'central paradigm'. As they put it, 'Through its combination of "laws" and "initial conditions" Malthus's theoretical formulation accords well with the canons of scientific inquiry'. Does this mean that Malthus's principle of population can be considered falsifiable? Or is it only posing as a scientific hypothesis and, because it is not truly falsifiable, should it be rejected as a scientific theory?[12] What is really missing in Malthus's principle of population, beside the word itself, to be called a demographic paradigm?

Schofield and Coleman say little to help their readers find some answer to this sort of question. After high expectations are raised with the use of terms such as 'central paradigms' and 'falsifiable', readers are left to make their own guesses, including upon what the authors actually think about the falsifiability of Malthus's theory. Beyond that, it seems clear that Schofield and Coleman do not see Malthus's principle of population as a central paradigm in demography. This may help to explain their failure to clarify what they mean by 'central paradigm'.

This is surprising, the more so if one considers that Darwin found inspiration from Malthus's Principle. Demographers, it seems, turn their attention elsewhere, looking to other fields for some intellectual inspiration. As Schofield and Coleman (1986: 4) point out, 'In the search for a central paradigm general biological models have sometimes proved popular in demography'. Or yet, Schofield and Coleman suggest that demographers have no better alternative but to rely on the so-called integration of 'the new insights of "external" theory with the "internal" theory of mathematical demography; this is how 'population theory today can develop into a more rigorous and realistic discipline. This was Malthus's method, and his agenda: the way ahead lies "Forward from Malthus".' (Schofield and Coleman, 1986: 11).

How can one expect to find an adequate 'central paradigm linking small-scale and large-scale processes' without starting by acknowledging those which have so far shaped demographic theory, such as the Grauntian model of demography's whole design, the Malthusian principle of population? The demographic paradigms set out in Graunt's

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11 Wunsch dismisses Popper's curious assertion about the advantage of social sciences over the natural sciences because 'people's rationality makes it easier for us to comprehend human behaviour than to understand why e.g. atoms and particles react the way they do'. However, according to Wunsch, 'People can alter their behaviour in a way atoms and particles cannot, if the situation they are in changes'.

12 On falsificationism see Chalmers's (1988) book, *What Is This Thing Called Science?*

Observations and Malthus's *Principle of Population* are, perhaps, the two most important in demography. Are there others? Should demographers search for better ones? Is there any possibility to devise a 'non-Newtoian-demographic paradigm? To answer these questions demographers must first recognize the role and limitations of these paradigms that have driven the scientific study of population.[13]

The passion between the sexes: Malthus versus his critics

The developments that have emerged from Malthus's theory and gained recognition since 1798 can easily be identified by reviewing the existing literature. What is more difficult, though, is to anticipate developments that may still emerge from it, particularly from elements which have remained dormant because they were not ripe, or not timely for exploration. Yet, if the concept of 'passion between the sexes' was for Malthus more than a simple constant in a mathematical equation it must be possible to show it.

Commenting further on Malthus's second postulate, it is useful to consider his own statements on the passion between the sexes. Box 1.5.1 provides almost all the important statements on this that can be found in the first edition of the *Principle of Population*. Box 1.5.2 does the same for later editions of Malthus's book, based on James's (1989) edition of the 1803 version, with the variora of 1806, 1807, 1817, and 1826. The fact that these two Boxes distinguish the first from other editions should reinforce my argument above that despite Malthus's withdrawal of the initial statement of his two postulata he never abandoned their content throughout the years.

To set against Malthus's views, Box 1.5.3 gathers statements from eight critics, ranging from Godwin to some prominent names in contemporary demography. Not all the comments included in Box 1.5.3 are negative; but the positive are in the minority and definitely the least influential among the current assessments of Malthus's second postulate. Besides, Box 1.5.3 does not do justice to the great majority of authors who express their thoughts by abstention and silence. For instance, Coleman and Schofield's book (1986) is notably in this respect; in more than 300 pages, only eight discuss explicitly Malthus's second postulate: five in Wrigley's (1986: 46, 48, 53-55) essay, three in von Tunzelmann's (1986: 72, 86, 89).[14] The silence of other authors provides a good

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[13] In 1959 Vance discussed the 'status of demography in the United States' and made this interesting remark:

> Demography developed out of two different lines of approach: (1) the problem approach of Thomas Robert Malthus (1776-1834) and (2) the statistical analysis of population aggregates represented in the work of John Graunt (1627-74) and Adolphe Quételet (1796-1874). Failure to fuse these two approaches helps to explain present scientific dilemmas in the discipline (Vance, 1959: 292).

[14] Von Tunzelmann's (1986) paper attempts to pack Malthus's population principle in the Volterra-Lotka model of predator-prey interaction, but fails to explore the features that would have justified bringing the two frameworks into a unified abstract system. In 1976 Berlinski (1976: 81-84) identifed two interesting lessons afforded by Volterra-Lotka predator-prey equations. First, while these equations are simple, 'the

illustration of my assertion that contemporary demographers still proceed as if 1798 had never happened; their silence says little for Malthus's principle but much for the state of population theory today.

Box 1.5.1 Malthus on the passion between the sexes- *Principle of Population* [1798/1970]

1. the passion between the sexes is necessary and will remain nearly in its present state (p. 70)

2. Godwin has conjectured that the passion between the sexes may in time be extinguished ... But towards the extinction of the passion between the sexes, no progress whatever has hitherto been made. It appears to exist in as much force at present as it did two thousand or four thousand years ago. There are individual exceptions now as there always have been. But, as these exceptions do not appear to increase in number, it would surely be a very unphilosophical mode of arguing to infer, merely from the existence of an exception, that the exception would, in time, become the rule, and the rule the exception (p. 71).

3. I think it will be allowed, that no state has hitherto existed (at least that we have any account of) where the manners were so pure and simple, and the means of subsistence so abundant, that no check whatever has existed to early marriages, among the lower classes, from a fear of not providing well for their families, or among the higher classes, from a fear of lowering their conditions in life. Consequently in no state that we have yet known has the power of population been left to exert itself with perfect freedom (p. 73).

4. It is said that the passion between the sexes is less ardent among the North American Indians than among any other race of men. Yet, notwithstanding this apathy, the effort towards population, even in this people, seems to be always greater than the means to support it ... The same observation has been made with regard to the Hottentots near the Cape. These facts prove the superior power of population to the means of subsistence in nations of hunters, and that this power always shews itself the moment it is left to act with freedom (p. 81).

5. the number of unmarried persons in proportion to the whole number, existing at different periods, in the same or different states will enable us to judge whether population at these periods was increasing, stationary, or decreasing, but will form no criterion by which we can determine the actual population (p. 88)

6. The passion between the sexes has appeared in every age to be so nearly the same that it may always be considered, in algebraic language, as a given quantity. The great law of necessity which prevents population from increasing in any country beyond the food which it can either produce or acquire, is a law so open to our view, so obvious and evident to our understandings, and so completely confirmed by the experience of every age, that we cannot for a moment doubt it. The different modes which nature takes to prevent or repress a redundant population do not appear, indeed, to us so certain and regular, but though we cannot always predict the mode we may with certainty predict the fact (pp. 114-115).

7. Mr Godwin considers marriage as a fraud and a monopoly. Let us suppose the commerce of the sexes established upon principles of the most perfect freedom. Mr Godwin does not think himself

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assumptions that govern them are ruthless and their analysis subtle. This suggests a maxim for the mathematical modeler: start simply and use to the fullest the resources of theory'; it is this prescription but in reverse that characterizes von Tunzelmann's paper: 'pile up an imposingly complex system of equations and then subject them to an analysis of ineffable innocence' (Berlinski, 1976: 83). Berlinski identified a second lesson afforded by the Volterra-Lotka predator-prey equations, which Tunzelmann tries to explore but without being aware of it:

> it is not always necessary to subject an analytically intractable system to simulation in order to understand it qualitatively; correspondingly, qualitative insights are at greater depth than partially quantitative results. The moral: look to systems for which a qualitative analysis is possible. Although nothing is known of explicit solutions to the predator-prey equations, except for the trivial case, everything relevant is known about the *general class* of solutions: they are all periodic about a stable center (Berlinski, 1976: 83-84).

that this freedom would lead to a promiscuous intercourse, and in this I perfectly agree with him. The love of variety is a vicious, corrupt, and unnatural taste and could not prevail in any great degree in a simple and virtuous state of society. Each man would probably select himself a partner, to whom he would adhere as long as that adherence continued to be the choice of both parties. It would be of little consequence, according to Mr Godwin, how many children a woman had or to whom they belonged. Provisions and assistance would spontaneously flow from the quarter in which they abounded to the quarter that was deficient ... And every man would be ready to furnish instruction to the rising generation according to his capacity (pp. 135-136).

8. When these two fundamental laws of society, the security of property, and the institution of marriage, were once established, inequality of conditions must necessarily follow. Those who were born after the division of property would come into a world already possessed. If their parents, from having too large a family, could not give them sufficient for their support, what are they to do in a world where everything is appropriated? (p. 143)

9. No move towards the extinction of the passion between the sexes has taken place in the five or six thousand years that the world has existed. Men in the decline of life have in all ages declaimed against a passion which they have ceased to feel, but with as little reason as success. Those who from coldness of constitutional temperament have never felt what love is, will surely be allowed to be very incompetent judges with regard to the power of this passion to contribute to the sum of pleasurable sensations in life ... Perhaps there is scarcely a man who has once experienced the genuine delight of virtuous love, however great his intellectual pleasure may have been, that does not look back to the period as the sunny spot in his whole life, where his imagination loves to bask, which he recollects and contemplates with the fondest regrets, and which he would most wish to live over again. (p. 146).

10. Mr Godwin says, in order to shew the evident inferiority of the pleasures of sense, ‘Strip the commerce of the sexes of all its attendant circumstances, and it would be generally despised’ ... To strip sensual pleasures of all their adjuncts, in order to prove their inferiority, is to deprive a magnet of some of its most essential causes of attraction, and then to say that it is weak and inefficient (pp. 147-148).

Box 1.5.2 Malthus on the principle of population and the passion between the sexes:
***Principle of Population* [1803 and others/1989]**

1. The main principle advanced is so incontrovertible that, if I had confined myself merely to general views, I could have entrenched myself in an impregnable fortress; and the work, in this form, would probably have had a much more masterly air. But such general views, though they may advance the cause of abstract truth, rarely tend to promote any practical good ... if I refused to consider any of the consequences which appeared necessarily to flow from it, whatever these consequences might be (Vol I: 2-3).

2. In plants and animals the view of the subject is simple. They are all impelled by a powerful instinct to the increase of their species; and this instinct is interrupted by no reasoning or doubts about providing for their offspring. Whenever, therefore, there is liberty, the power of increase is exerted; and the superabundant effects are repressed afterwards by want of room and nourishment, which is common to plants and animals; and among animals, by their becoming the prey of each other. The effect of this check on man are more complicated. Impelled to the increase of his species by an equally powerful instinct, reason interrupts his career, and asks him whether he may not bring beings into the world for whom he cannot provide the means of support (Vol I: 10).

3. in no state that we have yet known has the power of population been left to exert itself with perfect freedom. Whether the law of marriage be instituted or not, the dictate of nature and virtue seems to be an early attachment to one woman; and where there were no impediments of any kind in the way of an union to which such an attachment would lead, and no causes of depopulation afterwards, the increase of the human species would be evidently much greater than any increase which has been hitherto known (Vol. I: 11).

4. In those countries of America where, from peculiar situation or further advantages in improvement, the hardships of savage life are less severely felt, the passion between the sexes becomes more ardent (Vol. I: 31).

5. The effect of polygamy in increasing the number of married women and preventing celibacy is beyond dispute; but how far this may tend to increase the actual population is a very different consideration (Vol. I: 92)

6. The principle in Egypt, at present, does all that is possible for it to do. It keeps the population fully up to the level of the means of subsistence; and, were its power ten times greater than it really is, it could do no more (Vol. I: 96).

7. He [Plato] next proceeds to consider the proper age for marriage, and determines it to be twenty for the women, and thirty for the men. Beginning at twenty, the woman is to bear children for the state till she is forty, and the man is to fulfil his duty in this respect from thirty to thirty-five. If a man produce a child into public either before or after this period, the action is to be considered in the same criminal and profane light as if he had produced one without the nuptial ceremonies, and instigated solely by incontinence ... When both sexes have passed the age assigned for presenting children to the state, Plato allows a great latitude of intercourse, but no child is to be brought to light ... From these passage it is evident that Plato fully saw the tendency of population to increase beyond the means of subsistence (Vol. I: 136-137).

8. Aristotle appears to have seen this necessity still more clearly. He fixes the proper age of marriage at thirty-seven for the men, and eighteen for the women; which must of course condemn a great number of women to celibacy, as there never can be so many men of thirty-seven as there are women of eighteen. Yet though he has fixed the age of marriage for the men at so late a period, he still thinks that there may be too many children, and proposes that the number allowed to each marriage should be regulated; and if any woman be pregnant after she has produced the prescribed number, that an abortion should be procured before the foetus has life ... When both sexes have passed the prescribed age, they are allowed to continue a connexion; but, as in Plato's republic, no child which may be result is to brought to light ... From a remark which he afterwards makes respecting Sparta, it appears still more clear that he fully understood the principle of population (Vol. I: 137-138).

9. to prevent the population of a country from regularly decreasing, it is absolutely necessary that each marriage, on an average, should yield a marriage; that is yield two children who live to be married. If the result fall short of this, the number of marriages must be gradually diminishing, and the number of children to each marriage remaining the same, the population, of course, will continue decreasing. If each marriage yield accurately two marrying children, the number of marriages and the number of children being the same in every generation, the population can be neither retrograde nor progressive, but must remain exactly stationary (V. I: 181).

10. The passion between the sexes has appeared in every age to be so nearly the same that it may always be considered, in algebraic language, as a given quantity (Vol. I: 301).

11. The system of equality which Mr. Godwin proposes is, on a first view, the most beautiful and engaging of any that has yet appeared ... But alas! ... The whole is little better than a dream - a phantom of the imagination ... Mr. Godwin, at the conclusion of the third chapter of his eighth book, speaking of population says: 'There is a principle in human society, by which population is perpetually kept down to the level of the means of subsistence. Thus, among the wandering tribes of America and Asia, we never find, through the lapse of ages, that population has so increased as to render necessary the cultivation of the earth. This principle, which Mr. Godwin thus mentions as some mysterious and occult cause, and which he does not attempt to investigate, has appeared to be the grinding law of necessity - misery, and the fear of misery (Vol. I: 316-317).

12. It is a perfectly just observation of Mr. Godwin, that 'there is a principle in human society by which population is perpetually kept down to the level of the means of subsistence.' The sole question is, what is this principle? (Vol I: 322).

13. After the desire of food, the most powerful and general of our desires is the passion between the sexes, taken in an enlarged sense. Of the happiness spread over human life by this passion, very few are unconscious ... It is a very great mistake to suppose that the passion between the sexes only operates and influences human conduct when the immediate gratification of it is in contemplation. The formation and steady pursuit of some particular plan of life has been justly considered as one of the most permanent sources of happiness ... The evening meal, the warm house, and the comfortable fireside, would lose half of their interest, if we were to exclude the idea of some object of affection

with whom they were to be shared ... the passion between the sexes has the most powerful tendency to soften and meliorate the human character and keep it more alive to all the kindlier emotions of benevolence and pity ... If, indeed, this bond of conjugal affection were considerably weakened, it seems probable, either that the man would make use of his superior physical strength, and turn his wife into a slave, as among the generality of savages; or at best, that every little inequality of temper, which must necessarily occur between two persons, would produce a total alienation of affection; and this could hardly take place without a diminution of parental fondness and care, which would have the most fatal effect on the happiness of society (Vol. II: 90-91).

14. Considering then the passion between the sexes in all its bearings and relations, and including the endearing engagement of parent and child resulting from it, few will be disposed to deny that it is one of the principal ingredients of human happiness. Yet experience teaches us that much evil flows from the irregular gratification of it (Vol II: 92).

15. The fecundity of the human species is, in some respects, a distinct consideration from the passion between the sexes, as it evidently depends more upon the power of women in bearing children, than upon the strength or weakness of this passion. It is, however, a law, exactly similar in its great features to all the other laws of nature. It is strong and general, and apparently would not admit of any very considerable diminution, without being inadequate to its object; the evils arising from it are incidental to these necessary qualities of strength and generality; and these evils are capable of being very greatly mitigated and rendered comparatively hight by human energy and virtue (Vol. II: 93).

16. It is evidently, therefore, regulation and direction which are required with regard to the principle of population, not diminution or alteration. And if moral restraint be the only virtuous mode of avoiding the incidental evils arising from this principle, our obligation to practice it will evidently rest exactly upon the same foundation, as our obligation to practise any of the other virtues, the foundation of utility (Vol. II: 94).

17. The interval between the age of puberty and the period at which each individual might venture on marriage must, according to the supposition, be passed in strict chastity; because the law of chastity cannot be violated without producing evil. The effect of anything like a promiscuous intercourse, which prevents the birth of children, is evidently to weaken the best affections of the heart, and in a very marked manner to degrade the female character. And any other intercourse would, without improper arts, bring as many children into the society as marriage, with a much greater probability of their becoming a burden to it (Vol II: 97).

18. The apparent object of the passion between the sexes is the continuation of the species, and the formation of such an intimate union of views and interests between two persons as will best promote their happiness, and at the same time secure the proper degree of attention to the helplessness of infancy and the education of the rising generation; but if every man were to obey at all times the impulses of nature in the gratification of this passion, without regard to consequences, the principal part of these important objects would not be attained, and even the continuation of the species might be defeated by a promiscuous intercourse (Vol. II: 156).

19. Neither theory nor experience will justify us in believing, either that the passion between the sexes, or the natural prolificness of women, diminishes in the progress of society (Vol. II, p. 239).

Box 1.5.3 The passion between the sexes - Malthus's second postulate as seen by its critics

1. **Godwin:** I would ask, What is its present state? The want of a precise explanation under this head, is a deficiency that goes to the heart of the system ... This member of Mr. Malthus's proposition, if explicitly unfolded, must mean, that ' the passion between the sexes' always exists and acts, in all persons, in all countries, and in all ages of the world, under all institutions, prejudices, superstitions, and systems of thinking, in the same manner ... Will it be affirmed, that the most decent single women, in those countries of Europe, where morality most steadily maintains its empire, are as prone to violations of chastity, as the most licentious men, or as the women of Cafraria or Otaheite? Are the Fakirs, who voluntarily exercise on their bodies the most tremendous severities, at the same time immersed in the most shameless voluptuousness? Have the most reverend bishops, in times when celibacy was ranked among the first of virtues and the most indispensable, led exactly the same lives, as a Mohammedan sultan in his seraglio, as Tiberius or Sardanapalus? Many satirical and cutting things have been invented against monks and nuns and hermits: but are we really to believe that all such societies, without exception, have been sinks of debauchery, and all such persons the most audacious and consummate hypocrites that ever existed? ... It cannot therefore be supposed that there is any thing in woman, that should make her by nature more capable of abstinence and rigorous self-government than our own sex. Nor on the other hand will any impartial enquires affirm, that the passion of the male sex are stronger than those of the female, so as by that means, though we have more power to control our appetites, yet having a more forceful antagonist to contend with, we should for that reason be oftener subdued ... but how is it in reality as to the male? Away with the licentious and unprincipled doctrines, that we are not in many cases as pure and beyond suspicion in these respects as the females! (Godwin , 1820: 530-535).

2. **Engels**: The main reproach levelled against Darwin is that he transferred the Malthusian population theory from economics into natural science, that he never got beyond the ideas of an animal breeder, and that in his theory of the struggle for existence he pursued unscientific semi-poetry, and the whole of Darwinism, after deducting what had been borrowed from Lamarck, is a piece of brutality directed against humanity (Engels, 1953/1878: 180-181).

3. **Davis:** we should expect a careful definition of what is meant by the capacity for population growth and a set of inferences from this definition as to the factors which, if they were present, would maximize it. We should expect the same to be done with the checks. Malthus does do something like this for the checks, but with respect to the growth capacity he is content to attribute it to an instinct which man shares with the animals. If this instinct alone were operating, without any checks 'the increase of the human species would be evidently much greater than any increase which has been hitherto known'. But he does not clarify the concept of instinct; he does not analyze the mechanisms of reproduction; and, as a consequence, he overlooks some implications that bear on the other side of his equation, the checks. The human and animal motivation, on the instinctual level, is primarily for sexual intercourse. Reproduction comes as a mechanical aftermath which, in the case of human beings, can be avoided. By saying that man is 'impelled to the increase of his species' by a 'powerful instinct', Malthus overlooks an opportunity to clarify his concept of growth capacity in such a way as to draw implications for his conception of the checks (Davis, 1955: 543).

4. **Ryder:** The model of demographic dynamics which Malthus introduced, and which still serves as a focus for many discussions of theory and policy, was undoubtedly a stimulant to the increasing scope and quality of population data; but the form was not conducive to fertility research because Malthus regarded fertility more as a parameter than a variable. The 'passion between the sexes' was considered constant, and the relationship between progress and population growth was established through the agency of mortality variation. During the nineteenth century two distinct sets of ideas were developed in opposition to the view of constancy of fertility. The first was that the transformation in man's ways of life, associated with the idea of progress, weakened his generative faculties through some unspecified physiological nexus and thus caused fertility decline. The second was man's decision to control his output of children, to extend the sphere of rationality into the area of reproduction, and to bring into the world only those who could be raised in that standard of life he deemed essential or desirable (Ryder, 1959: 401).

5. **Flew:** Malthus always and explicitly makes the reasonable but by no means unquestionable assumption that sexual desire and the capacities to fertilize and conceive are constants ... Thus in the first *Essay* he denies that there has been ' a decay of the passion between the sexes. We have sufficient reason to think that this natural propensity exists still in undiminished vigour' ... Over a century later the *Report* of the British Royal Commission on Population recorded its verdict in the same sense: 'It is just possible that there has been some decline in reproductive capacity, though there is no positive evidence to this effect; and indeed so far as we know reproductive capacity may have risen.' Maybe some day such positive evidence will appear. But already we can be certain that the time scale of any such natural adjustment as may in act be occurring is that of biological evolution rather than of practical politics; and, therefore, that it is not safe just to leave it complacently to 'the wisdom of nature' to dispose of this vast power of multiplication (Flew, 1970: 33)

6. **Demeny**: Some two decades after *The Wealth of Nations* appeared, the young Malthus pronounced his 'second postulatum' of the principle of population ... This was a romantic but thoroughly erroneous theory: it implied, for instance, as Kingsley Davis has observed, that if sex and procreation could be separated - as they can be today with ease - fertility would fall to zero. Malthus, however, soon had better ideas about fertility change. (Demeny, 1986: 478).

7. **Demeny**: The young Malthus was of course a biological determinist; in his view, fertility, governed by the 'passion between the sexes', was not under effective social control. More sophisticated variants of this formulation- -derived from Malthus, vintage 1798 - continue to this day to dominate the views held by most biologists and ecologists on fertility behavior. But the mature Malthus was a social scientist, developing ideas about adjustment mechanisms for population growth fundamentally different from those governing the growth of animal species (Demeny, 1995: 8).

8. **Wrigley**: It is convenient to begin with his two basic postulata. Both might be described as biological rather than sociological or historical in character. Malthus hoped that they would be recognized as invariant features of all societies. In this he may have been too sanguine, at least in regard to the second of the two, since patterns of sexual behaviour vary substantially between different societies, though not perhaps in ways that would seriously affect the arguments he advance... With the second postulate, as with the first, if there is a difficulty it lies less with the postulate itself than with the assumptions which Malthus made in parallel with it. If the passion between the sexes might be taken as a constant, Malthus thought it safe to assume that population would display a tendency to grow exponentially. In his first statement of the case, and in the flush of exuberant youth, he emphasized the near-identity of human and all other forms of life in this respect ... Later his treatment of the issue was very much refined from his initial formulation, but he never abandoned one of the inferences which flows directly from this postulate. Any increase in the supply of food, disregarding the random fluctuations imposed by the fortunes of the harvest, must produce a proportionate increase in the population (Wrigley, 1986: 48, 53).

9. **Lesthaeghe:** As the preventive checks of reproductive regimes are located at the level of 'intermediate fertility variables', it is essential to understand why some societies rely predominantly on controls on the starting pattern of fertility (postponement of first sexual unions, celibacy), while others locate them within the spacing or stopping patterns (such as, through long postpartum nonsusceptible periods, reduced remarriage, or early 'terminal' abstinence). An exploration of such differentiation leads us to a study of patterns of kinship organization and to an examination of systems of production and control over resources. At this point, various links can be made between aspects of 'production' and features of 'reproduction' ... So far we have established two basic facts and their raison d'être: the reproductive regime of most of sub-Saharan Africa has a major preventive check operating through marked child-spacing, but this operates within the system that makes nearly complete use of the entire reproductive age span ... More elaborate views, adopting a systems analysis approach, which is essentially derived from Malthus's philosophy, consider reproductive regimes in a *dual* context: fertility levels not only need to be high enough to offset the force of mortality, they also need to be low enough to prevent population growth rates from threatening long-term subsistence means. This view stresses the importance of preventive checks on fertility and sets them in an environmental, economic, social, and cultural context (Lesthaeghe, 1989:15, 23, 52).

The passion between the sexes and sexual reproduction

After letting Malthus and his critics speak for themselves, it should now be easier to comment on the passion between the sexes and its significance to demographers. In Box 1.5.3 Godwin is the only contemporary of Malthus, and thus also a predecessor of Darwin, a feature that seems crucial to understand better Malthus's second postulate.

I consider Godwin's complaint the only one that is fair in its criticism. It charges Malthus with lack of clarity about his second postulate. Seen from today, one should be in a position to appreciate Godwin's difficulty in coming to terms with Malthus's intriguing remarks on the demographic role of the sexual instinct. Of course, Godwin did not criticize Malthus for mixing 'moralistic and scientific aims' (Davis, 1955: 543); besides the fact that this was not a valid issue in their time, as far as moralism is concerned Malthus was rather modest and liberal when compared with Godwin. Among other things, Godwin accused Malthus of irreverence: ‘He had made no allusion to Adam and Eve, and has written just as any speculator in political economy might have done, to whom the records of the Bible were unknown’ (Godwin, 1820: 23).

Engels's statement included in Box 1.5.3 reflects one of his different reactions towards Darwin, which he shared with his closest friend Marx. In some cases, both authors were exultant in their welcome of Darwin's theory of evolution, while in others they reacted as apoplectically as they generally did to Malthus:

> As regards Darwin, whom I have looked at again, it amuses me that he says he applies the 'Malthusian' theory *also* to plants and animals, as if Malthus's whole point did not consist in the fact that his theory is applied *not* to plants and animals, but only to humans beings - in geometrical progression - as opposed to plants and animals. It is remarkable that Darwin recognises among brutes and plants his English society with its division of labour, competition, opening up of new markets, 'inventions' and Malthusian 'struggle for existence'. It is Hobbes's *bellum omnium contra omnes*, and it is reminiscent of Hegel in the *Phenomenology*, where bourgeois society figures as 'spiritual animal kingdom', while with Darwin the animal kingdom figures as bourgeois society (Marx's letter to Engels, 1862, in Meek, 1953: 173)

> all that has to be done is to translate every concrete struggle into the phrase, 'struggle for existence', and this phrase itself into the Malthusian population fantasy. One must admit that this is a very impressive method - for swaggering, sham-scientific, bombastic ignorance and intellectual laziness (Marx's letter to Kugelmann, 1870, in Meek, 1953: 174).

Contemporary demographers sometimes mention Marx because of his well known remark 'that population models are "culture-specific", denying the validity of any general theoretical statements about population processes' (Schofield and Coleman, 1986: 2). Whatever this statement really means, Marx was never interested in demography.[15]

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[15] This is certainly not the view of, for instance, Valentei, who is not short in words as to what he considers that Marx, Engels and Lenin demonstrated about population growth in different societies:

> In defiance of convictions widely held among bourgeois demographers and sociologists, Marx and Engels demonstrated that population growth is not determined by biological factors quite independent of society; this growth is accelerated or slowed down depending on the nature of the social system and the level of its development. Thus historical materialism, while

Marx singled out Malthus from other 'bourgeois economists', some of whom he treated with great esteem, more for political and ideological reasons than anything else (Petersen, 1979: 74-81).[16] Beyond that, if one tries to remove the commonsense blinkers that ideology creates it may be possible to accept that Engels's famous book, *The Origin of the Family, Private Property and the State*, first published in 1884, stands on two propositions which can be seen as the Marxist reply to Malthus's principle of population.

> According to the materialistic conception, the determining factor in history is, in the last resort, the production and reproduction of immediate life. But this itself is of a twofold character. On one the hand, the production of the means of subsistence, of food, clothing and shelter and the tools requisite therefore; on the other, the production of human beings themselves, the propagation of the species (Engels, 1988[1884]: 707).

Of course, beneath this statement is a philosophical approach that is radically different from the one with which Malthus associated his theory. But not less important, Engels wrote his book about 25 years after Darwin's *Origin*; with this, among other scientific developments and his own historical materialist philosophy, Engels was in a better position than Malthus to search for explanations by questioning nature rather than appealing to the authority of a Being.

For these, among other reasons, the Marxist principle of population summarized by Engels’s statement cited above has been remarkably rewarded with a variety of stimulating theoretical developments that should be the envy of any author who, for whatever reason, is more sympathetic to Malthus than to Engels and Marx. While Malthus's pioneering discussion of the determinant factor in the history of demographic change continues to trouble mainstream students of population, it is ironic that anthropologists and demographers have met at the edge of their own fields in recent decades stimulated not by Malthus's principle of population but by its Marxist replica.[17]

> acknowledging that biological factors have a certain role to play, nevertheless regards the socio-economic factors shaping processes of a population's natural reproduction as possessed of decisive importance (Valentei, 1977: 18)

[16] As Petersen put it, 'to preserve the dogma that socialism is man's inevitable future, Marx had to discard Malthus's principle of population'. Whether Marx did have to discard Malthus's principle is debatable, but it is a fact Marx did it as the following shows:

> If Malthus's theory of population is correct, then I can *not* abolish this [iron law of wages] even if I abolish wage labor a hundred times, because this law is not only paramount over the system of wage labor but also over *every* social system. Stepping straight from this, the Economists proved fifty years ago or more that socialism cannot abolish poverty, which is based on nature, but only *communalize* it, distributing it equally over the whole surface of society (Marx, cited by Petersen, 1979: 75-76).

[17] To mention just some of the many theoretical developments: Meillassoux (1981: xi) disputed Engels’s suggestion that the 'propagation of the species' plays a role as important as 'the production of the means of subsistence’, and then reduced the social formation to a single mode of production (Terray, 1972: 95-186). Bodemann (1980: 75-83) challenged the assertion that Engels’s attribution of independent role to production and reproduction was an isolated error; for this author Engels was at odds with Marx and even Morgan because he really attributed independent causality to the reproduction of life. Edholm, Harris and Young (1979: 101-130) reviewed Meillassoux's (1981) work and O’Laughlin’s (1977) critique of it, and concluded that the conflation of human reproduction with both social reproduction and the reproduction of labour force is far from adequate. Ekholm and Friedman (1985) rejected the usefulness of the concept of

The success of Engels’s demographic propositions and some of Marx's concepts could also be explained, as in the case of Malthus and Darwin, by the power of their own algorithms. This is not the place to do it, though it can be conjectured that from a demographic point of view Malthus set out a good starting for an investigation on the relationship between survivorship and reproduction. In contrast to Engels and Marx, Malthus did not just formulate general 'laws'; he outlined specific hypotheses about some expected relationships between the variables of production and reproduction: 'Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio' (Malthus, 1970: 71).

Whatever the result of empirical research on these hypotheses, Malthus's theoretical scheme guarantees that there is always a result. In other words, with regard to the three features of an algorithmic process mentioned above, Malthus was far more specific than Engels in providing a theoretical basis for any empirical investigation of demographic relations. Malthus's second postulate is especially concerned with the determinants of 'reproduction of immediate life'; he considered that humans share with other species which reproduce sexually a similar instinctive design, but among the former the passion between the sexes sets the grounds for the social mechanisms which check and determine demographic reproduction. In turn, Engels's discussion of the relationships between the sexes highlighted the struggle for power and dominance, and considered that such a struggle is generally subordinated to class antagonisms.

It is interesting that three decades ago Davis (1955) charged Malthus with not clarifying 'the concept of instinct'. 'By saying that man is "impelled to the increase of his species",’ Davis (1955: 543) writes, 'Malthus overlooks an opportunity to clarify his concept of growth capacity in such a way as to draw implications for his conception of the checks'. This is more or less saying that Ptolemy overlooked an opportunity to anticipate the Copernican concept of the Universe. In particular, Davis’s use of the verb 'overlook' is puzzling; this verb implies that Malthus should have anticipated the understanding about sexual instinct that Darwin, Wallace and Mendel outlined. Malthus did ‘not analyze the mechanisms of reproduction' (Davis, 1955: 543) simply because most

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mode of production and, in its place, proposed the notion of ‘reproductive process’. Seccombe (1986: 28-30) attempted to rework the mode of production concept to integrate with the demographic reproduction of the labour force. Caldwell (1978: 573 used Marx's concept of modes of production and relations of production as the stage of his analysis of family systems and intergenerational wealth flows (Caldwell and Caldwell, 1992: 46), though he departs from Marx and Engels's orthodoxy to argue ‘ that there have been only two models of production and that these determine the two very different types of society that exist. The first is familial production ...The second is labour-market production’ (Caldwell and Caldwell, 1992: 46). In recent decades, feminists of several currents have visited and revisited Engels, either to praise him or to excoriate his views on women's roles in society (Millett, 1971: 120-127; Rubin, 1975: 157-210; Sacks, 1975: 211-234; O'Brien, 1981: 22, 83, 85, 220, 224-225; Moen, 1987: 277-287; Sayers, Evans and Redclift, 1987; Folbre and Hartmann, 1988: 190, 194; Folbre, 1993: 94, 101, 107; Humm, 1990: 62; Waring, 1989: 5-6). The list is already long, and not even the fall of the Berlin Wall and its aftermath show any indication that interest in Marx and Engels is fading away.

of his intuition about the passion between the sexes can only be fully grasped with the insights of the theory of natural selection and genetics. As Dennett remarks:

> It has taken a century of further work to replace Darwin's brilliant but inconclusive musings on the mechanism of speciation with accounts that are to some degree demonstrable. Controversy about the mechanisms and principles of speciation still persists, so in one sense neither Darwin nor any subsequent Darwinian has explained the origin of the species . As the geneticist Steve Jones (1993) has remarked ... 'Darwin knew nothing about genetics. Now we know a great deal, and although the way in which species begin is still a mystery, it is one with the details filled' (Dennett, 1995b: 44)

In his 1955 paper, Davis highlighted several deficiencies in Malthus's framework, especially in its deductive system. But Davis's assessment did not aim to propose ways to overcome 'Malthus's great empirical mistakes' resulting from the 'weakness of his conceptual framework' and 'the confusion of moral evaluation with scientific analysis'. Because of that, Davis's judgement is generally unfair, indeed even intellectually arrogant; his assessment of Malthus's theory is guilty of the anachronism that Kreager (1991: 207) pointed out in those authors who 'lump together all writings before 1800 as "pre-Malthusian" and consider them chiefly for their anticipation of Malthus'. To paraphrase Kreager (1991: 207), Davis judged Malthus in terms of later scientific developments of which he could not have been aware.

I turn next to the passage by Ryder in Box 1.5.3. Ryder concluded that Malthus's model 'was not conducive to fertility research because Malthus regarded fertility more as a parameter than a variable'. I do not believe that Boxes 1 and 2 offer any reasonable basis for Ryder's statement. In reviewing Malthus's statements, it may help to replace mentally the term 'passion between the sexes' by 'sexual reproduction'; this emphasises Malthus's intuition that there is an evolutionary drive in individuals and population in general to reproduce sexually, and clarifies his claim that: 'It appears to exist in as much force at present as it did two thousand or four thousand years ago'. In paragraph 3 and 4 of Box 1.5.1, Malthus refers to the aftermath of the passion between the sexes, namely the mechanisms of sexual reproduction, which differ regionally.

In paragraph 6 of Box 1.5.1 and paragraph 10 of Box 1.5.2, Malthus insists on his notion of constancy: 'The passion between the sexes has appeared in every age to be so nearly the same that it may always be considered, in algebraic language, as a given quantity'. In other words, basically Malthus implies in this statement: 'let us take sexual reproduction for granted'. But it is totally wrong to conclude from this that Malthus considered fertility constant.

Ryder's interpretation is wrong for two very simple reasons. First, 'The fecundity of the human species is,' as Malthus writes clearly (see paragraph 15 in Box 1.5.2), 'in some respects, a distinct consideration from the passion between the sexes'. As the next chapter will clarify further, in 1798 the concepts of fecundity and fertility were still used in demography interchangeably, even though Malthus shows explicitly he is aware that fertility 'evidently depends more upon the power of women in bearing children, than upon the strength or weakness of this passion'.

The second reason why Ryder’s interpretation is wrong is in part explained by Flew’s statement in paragraph 5 of Box 1.5.3. In 1970, Flew expressed what may be considered one of the most accurate interpretations of Malthus's second postulate ever proposed in the past two centuries. Flew started by dismissing a remark from Kenneth Smith (1951) in his book *The Malthusian Controversy* ‘that Francis Place's "advocacy of birth control was the beginning of a movement which can completely nullify the geometrical or any other ratio”’(Flew, 1970: 32). To this Flew responds:

> But, on the contrary, it is precisely, and only in order to put a check on, this formidable power to be fruitful and multiply that contraception is and has to be employed. There would be no scope for birth-control movement if there were not a Malthusian power for it to control (Flew, 1970: 33)

One would assume that this interpretation should be obvious and straightforward, particularly for authors such as Davis, Ryder and Demeny. If Malthus did in fact believe that marital fertility is an invariant parameter of what use would the 'preventive checks' and 'moral restraint' be? Even if one admits that Malthus's statements on the passion between sexes stirred up confusion, how can one imagine that an intellectual of his stature would dare to propose checks for something that is invariable?

Moreover, as can be seen in Box 1.5.3 Demeny and Wrigley attribute Malthus' second postulate to the exuberance of his youth. Again, this interpretation can only be possible if one refuses to see that Malthus never abandoned 'the flush of exuberant youth' found in his 'initial formulation' of the second postulate. It is true that 'Later his treatment of the issue was very much refined', as Wrigley (1986: 53) remarks, but as the statements in Box 1.5.2 show it is wrong to infer from this any substantial inconsistency between the young and the old Malthus.

With regard to Demeny, in his 1986 paper he appreciates the aesthetic beauty of the classical metaphor of the 'invisible hand' from the classical economist Adam Smith. Would it be meaningful to imagine the 'passion between the sexes' as a sort of ‘invisible hand’ or an auto-mechanism of self-regulation which spontaneously orders and determines the demographic change of population? Demeny's recent reference to Malthus's second postulate did not discuss this issue. But again Demeny considers the 'passion between the sexes' as expression from the 'young Malthus', the 'biological determinist', and more sophisticated variants 'derived from Malthus, vintage 1798 - continue to this day to dominate the views held by most biologists and ecologists on fertility behavior'. Yet, 'the mature Malthus was a social scientist', and thus Demeny finds no place for the 'passion between the sexes': 'developing ideas about adjustment mechanisms for population growth fundamentally different from those governing the growth of animal species' (Demeny, 1995: 8). This leaves little space to search for similarity, if not even links, between the 'invisible hand' in the economic market and the 'passion between the sexes' in the 'marriage market'.

Wrigley's paper seems also afflicted by the opposition between 'biological' factors, on the one hand, and 'sociological' and 'historical', on the other. In this case, the following remark from Ridley seems appropriate here:

> I have gradually come to realize that almost all of social science proceeds as if 1859, the year of the publication of *The Origin of Species*, has never happened; it does so quite deliberately, for it insists that man's nature is a product of his own free will and invention. Society is not the product of human physiology, it asserts, but vice versa (Ridley, 1993: 6-7).

In a period in which even time and the whole universe is said to have a history, the assertion that anything that is biological is at odds with the social and historic seems rather brave. In the present context, such an assertion is particularly detrimental to any adequate understanding on the nature of the relationship between sex and gender, a point to which I will return later; for now it should be enough to recall Udry's (1994: 363) remarks made at the same forum where, about ten years ago, Demeny spoke of 'population and the invisible hand':

> There are important reasons why social scientists do not consider the biological gender theory. First, most do not know about it. Second, it has no place in our disciplinary paradigm. Third, it is politically incorrect; some call it 'sexist' and 'ideological' (Longino, 1990, ch. 6). We believe that accepting a biological foundation for gender logically implies the support of current gender arrangements in society and undercuts motivation for change in gender structure. The idea that *any* behavior has a biological foundation is considered politically conservative. Social scientists imagine that if a behavior is under biological influence, there is nothing we can do about it; this naive notion is held only by social scientists (Udry, 1994: 563)

In summing up, it can be asserted that Malthus's postulate on the passion between the sexes was one, if not the first, most important theoretical statement in demography concerning the role of sexuality in demographic change. From reviewing Malthus's remarks specifically on the passion between the sexes it is clear that he did not consider sexuality as the product of free will only, and spoke about sex in its multiple dimensions: a designed drive and individual instinct; an organizing mechanism of reproduction which sets the basis for social relations between males and females; the basis of virtuous love and sensual pleasure; a powerful device to soften and meliorate the human character, though also a dangerous source of vice and alienation of affection if the bonds of conjugal affection are weak; and the primary determinant of the fertility of the human species, which in the end depends more upon the power of women in bearing children than upon the strength or weakness of this passion. After all, if one tries to grasp Malthus's passion between the sexes without prejudice his vision can be summed more or less like this. Apart from being 'the most fun I had without laughing' (Woody Allen cited by Coveney and Highfield, 1991: 257), the passion between the sexes exists in human population not only for fun.

6.

Three scientific breakthroughs in leaps of one hundred years: a new concept of fertility

> Historiography is a dialogue between an interrogating present and an interrogated past. Separated forever from the living past, the interrogating historian in following his proper art can reconstruct only from what he sees and understands ... We are alerted to the past by experience of the present (Crombie, 1994: 8)

> A theory can be tested by experiences, but there is no way from experience to the setting up of a theory (Einstein, 1949:89)

Three demographic concepts that commonsense treats as one

At the core of contemporary demographic analysis lies the description, measurement and analysis of fertility. This chapter provides the historical information necessary for readers to accept the following two statements as not simply rhetoric: first, that the demographic concept of fertility is probably the most original and fascinating theoretical contribution provided by demography in social sciences. Secondly, that the demographic concept of fertility has been as much invented as discovered and, beneath it lie the two concepts discussed in the previous four chapters: the sex ratio and the passion between the sexes or sexual reproduction.

While in commonsense language the concept of fertility is customarily used interchangeably with the term fecundity, in demography the concept of fertility already has a relatively long story. General dictionaries reflect commonsense language; it is therefore easy to pick up one of such dictionaries and confirm that they still explain fecundity and fertility as synonyms. For instance, the *Macquarie Dictionary* (1985: 643, 649) defines fecundity as 'the capacity, especially in female animals, of producing young in great numbers ... fruitfulness or fertility'; at the same time, it defines fertility as the 'ability to produce offspring ... power of reproduction'.[1]

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[1] The Dictionary in the Winword 2.0c computer program used to write this thesis defines fecundity as synonym of fertility, fruitfulness, productivity and virility. Even more specialized dictionaries such as the

Curiously, while in commonsense language the similarity between fertility and fecundity is still taken for granted, within the scope of studies of population the distinction between these two concepts is soon treated as given. The tyro in demography soon learns that the term fecundity should be reserved to mean only the potential or physiological capability of an individual to conceive offspring, whether or not such capacity is actually exercised. Biologists have a similar working concept; as Lawrence (1993: 182) put it, fecundity is 'the capacity of an individual or a species to multiply rapidly; in a stricter sense the number of eggs produced by an individual'. In turn, demographers and biologists have come to an agreement that by fertility they mean the actual live births a woman has at a certain point in time; or as Lawrence (1993: 183) put it, 'the reproductive performance of an individual or population, measured as the number of viable offspring produced per unit time'.

From this distinction an important feature seems apparent. It is part of commonsense that two different and fecund sexes, that is the sperm from the male and the egg from the woman, are needed for a conception to occur and woman's pregnancy to begin. However, only one sex, the female, has the physiological capacity to conceive and develop an offspring and produce children. In other words, in demographic usage both males and females can be either fecund or sterile, but only women can be either fertile or childless. In other words, both sexes can determine and contribute to the magnitude and changes in the direction of fertility but only females produce its output.

This does not mean that the demographic concept of fertility cannot be used to measure the number of children that men actually ‘fathered’. What it means is that while women can give birth to children by themselves, men can only father them through women. Moreover, and probably even more important as far as the description and measurement of fertility are concerned, is the way fertility is usually calculated: only one sex at a time needs to be considered and, within a given sex only the individuals who are actually exposed to the event of birth should be actually taken into consideration in the denominator. So, the situation seems rather obscure as to how, when, and why should both males and females matter to understand demographic change. Contrary to what may be the expectations of commonsense, the standard knowledge of reproduction in conventional demography suggests that only one sex is needed for demographic analysis. Indeed, mothers and fathers do not really have to meet and interact at the level of measurement of fertility output.

Dictionary of Feminist Theory (Humm, 1990: 76) show little awareness of the distinction that demographers and biologists draw between fecundity and fertility. In this latter case Humm defined fertility as 'Women's *ability* to produce children' and added: 'Feminist theory argues that fertility is not simply a biological or private concern' (italic added). There is an ironic paradox in this statement. On the one hand, Humm's position is an understandable reaction to the limited analysis that she has found in social sciences, including demography, concerning the relationship between the biological and social determinants of fertility. But on the other hand, it is unfortunate that an author who seems anxious for a broader understanding of fertility is not aware that demographers have long been creating a conceptual basis for that to be possible.

Conventional demographic theory is far from clear and explicit as to the reasons why most of its theories draw on one-sex approaches. This contrasts, for instance, with the fact that demographers have long been rather convincing about the need to distinguish natality from fertility. The former concept is sometimes used interchangeably with fertility, but in fact it is meant to relate births with the total of the population as a whole.[2]

It is widely accepted that measures such as crude birth rate can lead to misleading inferences because they do not take into consideration the effect of population structure. In fact, this is a sign of a more general problem associated with concepts and theoretical explanations which rely on the conflation of individuals of both sexes and all ages and thus are unable to account for the different and distinct roles of males and females in reproduction.

In any case, the fact that demographers have for long found it necessary to distinguish natality from fertility means that demography has already evolved a step further from the simple distinction between fecundity and fertility. Yet, even though commonsense is often a poor guide to an adequate understanding of the real world, one cannot dismiss the fact that commonsense works well when it is congruent with reality. The view that both sexes matter to reproduction is consistent with people's experience of life; but while commonsense shies away from any further explanation on this, there is also little support for this in the scientific studies of population. In particular, demography has no explanation for questions such as: when should both sexes be taken into consideration? How do males and females actually share and determine the process of fertility?

These kind of questions approach the core of this thesis, and I return to them in Part II. The reason they are raised at this stage is that the following chapters need the historical background provided in this chapter. In particular, the acceptance of this thesis by readers will depend a great deal on what I consider to have been the threefold bifurcation in the development of the concept of fertility. The concept of fertility in demography is substantially different from the way it is understood by commonsense and this has been the result of an historical metamorphosis which has led to three main bifurcations. The historical background of this process is provided in this chapter; since in the remaining chapters specific aspects considered here historically are addressed at some length, I find it important to give now the basic outline of perhaps the most significant issues tackled by this thesis.

An informal sketch of the metamorphoses of fertility in demography

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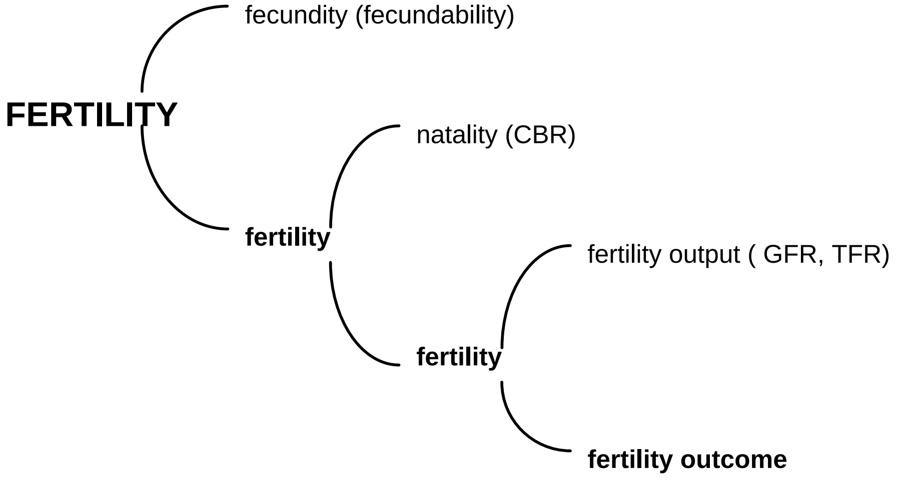
[2] I will return to the definition of natality in Chaper 9, in the section called 'From natality to fertility through nuptiality' which shows how Knibbs (1917) conceptualized the overall subject of reproduction in demographic terms.

Since Graunt, both the process of elaboration of operational definitions to analyse fertility and the identification of the population-base to measure it have been at the centre of the evolution of demographic theory of fertility. This evolution is much more interesting than demographic textbooks let students believe; it was never an easy process, and it is far from being concluded and fully established.

Figure 1.6.1 sketches the metamorphoses of the demographic concept of fertility as a threefold bifurcation. The first bifurcation corresponds to the split and distinction between fertility and fecundity. The second bifurcation refers to the split and distinction between natality concepts and specific operational definitions or ways to measuring it, i.e. crude birth rate, on the one hand, and fertility and reproductive indicators, i.e. general fertility rate, total fertility rate, parity progression ratios and net reproduction rate, on the other.

Figure 1.6.1 Three bifurcations in the conceptualization of fertility



Yet, strictly speaking, what is currently understood as fertility, in broad terms, means and should simply be called 'fertility output'. This is for two reasons. First, the concept of output refers to what is actually produced. That is, with regard to population in general the number of births produced by the population and expressed by the concept of population size; in the particular case of fertility, the output refers to the number of births related to the producing sex or the part of population which is exposed and has the ability to produce children. Implicit to what is produced is certainly a set of acts or a process over time which directly determine the overall population structure and the childbearing process in demographic reproduction. Secondly, the population directly relevant to such a study is the producing sex, the females; for this reason the study of demographic output has generally relied on a one-sex methodology.

Throughout the twentieth century, one-sex demography has become remarkably successful precisely because it has developed clearly designated and reliable measures around the concept of demographic output in general, and fertility output, in particular. The concepts and variables defining demographic output are indispensable to any description of what actually happened in a given population. This is where one-sex demography has shown itself to be reliable, valid and handy. As Chapter 10 demonstrates the power of one-sex demography derives from the match that it has achieved between the conceptualization of important demographic phenomena and the one-sex methodological approach. In other words, there is an adequate consistency between the epistemological setting and the methodological rules certain demographic issues are formulated and studied. On the one hand, the epistemological setting establishes that in its fundamental way the producing sex is the one that matters whenever the objective is to describe population change from the point of view of demographic output. On the other hand, the methodological rules allow to develop the necessary operational definitions and tools, including measures, variables, and models that relate specific abstract concepts to observable events.

As demography's achievements standards at the descriptive level have become more sophisticated a new challenge has arisen from the need to move on to the explanatory level. Over the past decades demographers have become generally confident as to the descriptive aspect of their discipline but extremely insecure with regard to its explanatory ability. Increasingly, demographers have started to face the new challenge, going beyond the descriptive level of population dynamics. They are increasingly accepting that demography is not just about what happened but also about why and how a certain event and change has occurred. An indication of this is the development in the past couple of decades of a vast literature on the so-called determinants of fertility.

However, for this task the one-sex approach is seen to be rather inadequate and weak. Although demographers have hoped to benefit from the success they enjoy whenever the one-sex approach is used to describe and measure demographic magnitudes and trends, they have generally failed to get adequate explanatory results.

It is not accidental that during the second half of the twentieth century a two-sex approach has been attempted in several areas of demography, particularly the formal and the empirical. These efforts suggest that there is no lack of an adequate and reliable descriptive basis for an explanatory endeavour. What is needed now are reliable ways to measure and explain the causal relationships and the changes in the relevant mechanisms that actually determine changes governing the magnitude and direction of demographic output.

The distinction I have been discussing between description and explanation can be regarded as the driving force leading to the third bifurcation of the concept of fertility. It seems inevitable that demography will have to acknowledge that 'fertility output' should be distinguished from what I shall call 'fertility outcome'. The former refers to the quantity or number of live children produced by females, that is by the producing sex, in a given time. The latter is defined as the cluster of practices, attitudes and knowledge of both sexes that explain the whys and wherefores of fertility output.

An adequate explanation of the magnitude and direction of fertility obtained by the one-sex demography has two main aspects. One is conceptual and is concerned with the identification of the total set of gender and generational contingencies on which the relationships between both sexes and a single joint fertility output depend. Just as in the case of fertility output, demographers will create - and to some extent are already creating - concepts which embody the content of demographic phenomena seen as outcome. Several concepts have already been developed, such as: the sex ratio, marriage function, value of children, demand for children, ideal family size, desired number of children, and intergenational wealth flows.

The second aspect is of a methodological nature. The concepts just mentioned remain crude and inadequate as indicators of demographic outcomes because they have been generally sketched within one-sex frameworks. However, while the producing sex is the one that matters when studying demographic outputs, with regard to demographic outcomes no sex can *a priori* be assumed more relevant than the other. Thus, males do not immediately produce demographic and fertility outputs, but for explanatory purposes their practices, attitudes and knowledge are potentially relevant as those of females in affecting those outputs.

So, the conceptual and the methodological aspects of the notion of demographic outcome are just different faces of the same issue. They are both of crucial importance for the development of demographic theory in contemporary times because demographic explanation entails understanding how and why population changes. This requires the discovery and specification of the total set of mechanisms, determinants and contingencies bearing on the causal relationships and variability of demographic outputs. For this sort of analysis, a necessary, though not sufficient, condition is to accept that both sexes matter, not one sex at a time. The reason this is not sufficient is because two-sex models and methods, just as in the case of the one-sex demography, should exist to give form to specific relationships between abstract concepts and observable events of a two-sex nature. That is, a two-sex approach should not be accepted as necessary for its own sake but to produce indicators and results that cannot be obtained on the basis of a one-sex methodology.

In short, as in the previous two bifurcations this third one can be seen as a breakthrough in the development of the concept of fertility. First, it entails creating abstract concepts consistent with the nature of two-sex demographic phenomena. And then the two-sex model and methods become indispensable tools in order to be able to transform the abstract concepts into variables or operational definitions. In this context, recognition of the notion of fertility outcome as a cluster of gender and generational on which the relationships between both sexes and a single joint fertility output depend is arguably part of a major step forward in the development of demographic theory.

Even though this rather brief introduction to the two new concepts proposed here may be far from clear, as in the previous chapters it seems better not to reduce such important concepts to a logical and terminological debate. So, before more systematic definitions are proposed for the new terms introduced above it seems better to establish the need for them; hence before setting out logical argumentation, the subject is discussed in an historical perspective. To some extent, this is consistent with the genetic approach to fertility illustrated by Figure 1.6.1; the difference is that the remainder of this chapter deals with the same basic argument in historical rather than logical terms. I trace briefly the origin, modes of formation and structure of fertility analysis in the development of demographic theory. This consideration of some of the significant historical events in the theoretical development of demography should contribute to an understanding of the logical result sketched in Figure 1.6.1.

The contemporary assessment of the history of fertility studies

In conventional demography it is widely believed that the study of fertility, as compared with the study of mortality, has captured the interest of demographers only in the twentieth century. A few examples of this view may be useful to situate the dominant argument.

Behar (1985: 173), in a paper dedicated to the measurement of fertility among earlier demographers, starts by highlighting the sharp contrast between the interest in studying fecundity nowadays and in the eighteenth century. The same did not happen to mortality, Behar asserts, a field that two centuries ago was already attractive, rich in approaches, and uniform in its analytical techniques.[3] Although as a field of observation and analysis fecundity was completely marginal, so Behar insists, in the case of mortality names like Graunt immediately come to mind. Still Behar in a more recent paper, 'Malthus and the development of demographic analysis', writes:

> Fertility was far from being a popular subject with demographers during the eighteenth - and even the nineteenth - centuries. This was due partly to lack of data and partly because fertility itself was regarded as given. There was, therefore, no real technical or analytical progress in the study of fertility, in contrast to the situation relating to mortality (Behar, 1987: 276).

A similar picture about fertility in the nineteenth century was also drawn by Lorimer more than three decades ago:

> Interest in fertility during most of the nineteenth century was largely incidental to other concerns and was rather sporadic. There were two reasons for this: there was

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[3] Behar detailed his view on earlier mortality achievements in a paper published in 1977. Although Diamond and McDonald's (1994: 29) assessment is far less supported by evidence, their view of the process is somewhat at odds with Behar's. 'Graunt's study was followed by other isolated studies based on church records in various parts of Europe but these studies remained fragmentary until the 19th century' (Diamond and McDonald, 1994: 29). In the same textbook from which Diamond and McDonald’s chapter comes, the chapters on fertility (Lucas, 1994:44-55; Lucas and Meyer, 1994: 56-68) make no comment about the history of the development of fertility studies.

> no obvious reason for any widespread concern at this time about variations or trends in fertility in the Western world from an economic, social, or political point of view; theoretical interest in the processes and structure of population - the mainstream of 'pure demography' - was eclipsed by more 'practical interests' during the decades that intervened between the decline of political arithmetic and the rise of modern demography (Lorimer, 1959: 142).

But current textbooks are definitely more accessible to the average reader, and they reproduce the same portrait; for instance Newell writes:

> It was only in the early part of the twentieth century that fertility began to be studied to any great extent, but the history of mortality analysis goes back as far as the work of Graunt in the late seventeenth century, and even to Roman times (1988: 63).

As an old proverb says, against facts there are no arguments. If the above descriptions reflect the historical facts, is there any chance of arguing against the conventional picture, and above all its interpretation of the history of fertility in demography?

Fertility as much invented as discovered: the remarkable step forward in 1775

The conventional picture provided above about the earlier history of fertility in demography is not totally false on its surface, but it is very misleading in its deeper content. Perhaps a slight caricature may help to pinpoint the main problem. For instance, let us replace three words in Behar's sentence cited above: in place of fertility put relativity, in place of demographers put physicists, and in place of mortality put astronomy. So, Behar's sentence turns into the following:

> Relativity was far from being a popular subject with physicists during the eighteenth - and even the nineteenth - centuries. This was due partly to lack of data and partly because relativity itself was regarded as given. There was, therefore, no real technical or analytical progress in the study of relativity, in contrast to the situation of astronomy.

Even demographers should have little difficulty in noting that this caricature refers to Einstein's theory of relativity. At first glance the parallelism seems exaggerated and meaningless; but, in fact, some of the crucial events in demography and physics almost coincided even in years. Bortkiewicz (1911) and Lotka published their seminal papers in mathematical demography (Lotka, 1907, 1922; Sharpe and Lotka, 1911) almost in the same years that Einstein published his famous papers on relativity theory in 1905 and 1915.

On whether or not Einstein was anticipated by any proto-concept of relativity of time demographers have certainly little to say. Demographers know very well that Lotka did not invent the concept of fertility, though the timing they consider decisive to the big-bang of fertility analysis is centred around Lotka's stable population theory. There is some truth in that view, but this is because the creation of several fertility measures occurred at the turn of the nineteenth to the twentieth centuries. In particular, the emergence of Lotka's theory was the culmination of a long and slow process of convergence of the development that occurred in fertility and mortality for about two centuries and half. Yet, this interpretation of stable population theory as the culmination of a convergence rather than the beginning of a new field is substantially different from the perspective about the development of fertility and mortality reviewed above.

It remains to be seen whether historians will be able one day to single out and credit any particular demographer as the one who first tied down the components of the demographic concept of fertility. The specific details of who or whether someone in particular sketched the demographic concept of fertility is not of major interest for the discussion here. This would hardly be the case if this work had the pretension to be a rigorous history of demographic theory. Yet, without attempting to pursue the subject in great detail, I will nevertheless make some conjectures useful for the core argument of this thesis.

Just as physicists had to abandon the idea of a universal time and, more recently, accept even counterintuitive working concepts such as 'imaginary time', demographers will have to abandon their ahistorical view of fertility in demography. This is consistent with the recognition that fertility could never be analysed scientifically if demographers did not move away from the common sense, in which fertility is synonym of a universal human capacity to reproduce. The problem, though, is that even when historians of the discipline admit that the concept of fertility has a history of its own, the way they portray such a history depends very much on how they distil the origin and evolution of this working concept from the history of demographic research. For instance, Behar (1987: 274) asserts that Jean Louis Muret 'was the first to calculate crude birth, marriage and death rates, though he did not call them that'; he explains further that Muret

> also invented the multiple decrement table as a by-product of his study of female mortality by marital status. He was also the first to calculate a life table for the first year of life by months, with the first month being sub-divided into weeks. His analysis of nuptiality and mortality anticipates later developments (Behar, 1987: 274).

In another passage Behar states that the Dutch demographer Willem Kersseboom 'invented' the 'most elaborate index of fertility which was used until well into the nineteenth century ... the ratio of annual births to annual marriages (N_t / M_t)' (Behar, 1987: 276). In short, Behar finds it acceptable to speak of invention with regard to specific measures and techniques in fertility analysis, but not when he refers to the fundamental demographic concept that lies behind them.[4]

In part, this can be understood on the grounds that the term fertility, like fecundity, is drawn from notions that exist in commonsense language and their origin is not easily identifiable.[5] However, in demography the concept of fertility is not the same

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[4] According to Dupâquier and Dupâquier (1985: 369) W. Farr invented the natality rate. In 1870, Farr had the idea of relating the number of births to the whole population, in percentage, and in place of the average number of inhabitants by birth. But Farr basically followed Muret's initiative a century earlier.

[5] Knibbs, in 1917, traced the etymologic origin of these two terms:

as the term used in common sense. Instead, it is the result of successive syntheses of ideas throughout a long process leading to the formulation of a scientific category previously unknown. For this reason one can speak of fertility in demography as being as much invented as discovered, though not by a single author but by a vast community of demographers.

Often the term invention is associated more with technical discoveries. So, in the case of demography, 'invention' would be reserved for situations in which a certain concept has been transformed into specific technical procedures, such as formulas, equations, or models of specific demographic phenomena. Yet the literature in this chapter seems enough for one to maintain that fertility has been as much invented as discovered.

I refer particularly to the papers from Westergaard (1932: 87), Kuczynski (1935: 115-116),) Lorimer (1959: 133, 143), Behar (1985: 179), and Dupâquier and Dupâquier (1985: 98, 282, 349); they all agree on at least one important historical event: it was Swedish demographers and statisticians, or more precisely someone between Per Wargentin (1717-1783), the head of the Swedish Statistical Commission, Peter Elvius (1710-1749) and Henrik Nicander (1744-1845), who first set up a system of data collection, in 1775, which included ‘particulars concerning the age of *mothers bearing children* (in five-years groups)’ (Westergaard, 1932: 87).

Certainly the Swedish demographers rather than being intellectually isolated were in contact with some of the most influential demographers of their time. Hecht (1987: 36), in a paper about Süssmilch, reports that Wargentin was his principal and most famous Swedish correspondent. Wargentin ‘repeatedly referred to Süssmilch writings in his own publications on demography, in 1754-5’, says Hecht; ‘though he did not always agree with Süssmilch, he adopted some of his methods and results’ (Hecht, 1987: 36). This is an interesting clue to the intellectual networking that preceded the Swedish decision in 1775. Moreover, Kuczynski's (1935) book, *The Measurement of Population Growth*, provides one of the most detailed and didactic reviews of earlier demographers’ attempts to transform the concept of fertility into adequate operational measures. Kuczynski, who first computed the total fertility rate remarks: ‘The first who computed the ratio of births to the women of child-bearing age was Nicolas Struyck (1753)’. In another passage Kuczynski informs the reader about who first calculated age specific fertility rates:

The terms 'fertility' and 'fecundity' though ordinarily sensibly identical in meaning, have sometimes been assigned different meanings by statisticians, one being employed to signify the qualitative, and the other the quantitative, aspect of reproductivity ... In Latin, although 'fertilitas' and 'fecunditas' have no marked difference of meaning, the latter word seems to be the preferable one for denoting frequency of bearing offspring. The root of fecundus is 'feo' (obsolete), or FE = Greek ϕv; c.f. Sanskrit bhu; Zend bû; see ϕύω Liddell and Scott's Greek-English Lexicon, 8 Edit., p. 1703. The root of 'fertilis' is 'fero' = Greek ϕερ: c.f., Sanskrit 'bhar'; Zend 'bar'; A.S., 'bear-n'; the radical meaning being to bear or carry. See Liddell and Scott op. cit., p. 1662. In regard to 'sterilitas', c.f., Sanskrit 'starî' (vacca sterilis) (Knibbs, 1917: 234).

> The first to realize that in order to measure fertility accurately it is necessary to compute fertility rates for the individual age groups of mothers apparently was the Swedish astronomer Per Wargentin. At the time when he was Secretary of the Swedish Academy of Science and the moving spirit of the Swedish Statistical Commission, the Swedish statistical records began (1775) to show the mothers bearing children by quinquennial age groups. On the basis of these records, the Secretary of the Statistical Commission, H. Nicander, computed specific fertility rates according to age by relating the average number of deliveries in 1780-1795 for each quinquennial age group from 15 to 55 years to the mean number of living females. He published the results in the Transactions of the Swedish Academy (1800), and they were made known to a larger public in 1815 by Joshua Milne (Kuczynski, 1935: 115-116).[6]

The sources I am using here provide no detailed information on the theoretical background of the 1775 event. But at least they inform contemporary demographers that in 1753 the Dutch mathematician Nicolas Struyck (1687-1769), and in the 1770s Wargentin and Nicander had a very close intuition of the concept of fertility as it is understood nowadays in conventional demography; an intuition similar to the one that has led contemporary demographers to credit Graunt's hypothetical distribution of mortality into age groups as the pioneer of the life table, even though it was Halley who in fact sketched its model. Thus, if the historical evidence used here is correct, particularly the Swedish demographers should be remembered as the first to set a system of data explicitly relevant to fertility analysis. This led Westergaard (1932: 87) to consider the event ‘a most remarkable step forward'; although he did not explain the meaning of his metaphor ‘step’, it can probably be perceived as a synonym of ‘invention’ or ‘discovery’.

The major objection to the idea that fertility as it is perceived by demographers today had to be 'invented' may come from those who like to entertain the thought that their simplistic comparisons with what happened to mortality are meaningful. In particular, these demographers tend to rely on the view that in mortality analysis demographers had adequate data much earlier than in fertility: they underestimate and overlook the important theoretical process of formulation of abstract concepts and their transformation into specific technical procedures or measures.

Wunsch, in his 1984 paper already mentioned in previous chapters, quotes Einstein's famous sentence: 'Theory cannot be fabricated out of the results of observations; it can only be invented'. Nevertheless, Wunsch is not very clear on the theoretical process that transforms concepts into specific indicators. Otherwise, he would have made it clear that what he considered to be the 'crucial step' of transforming the theoretical concepts into observational indicators or auxiliary theory is preceded by a no less important scientific process: one that involves the formulation of the scientific concepts themselves, either as a new synthesis of previous ideas or as a departure and

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[6] James (1989) provides some information about Nicander’s biography and contacts with Malthus:

> His [Malthus] first reference to Süssmilch, in his chapter on Sweden, is to the final complete edition of the *Göttliche Ordnung*, published in three volumes in 1798. This may provide a clue. Malthus met Professor Nicander (q.v.) in Stockholm in 1799, and he possibly supplied his visitor with some extracts from this edition, perhaps translated into Latin ... Nicander at this time was 55, and he might have seemed an old man to Malthus at 33 (James, 1989: 312, 342).

development from commonsense views. In this respect, the process should not be very different in the natural sciences from the social sciences. Just as in physics terms such as ‘relativity’ and ‘imaginary’ time are expanding commonsense and even Newton's view of time, in demography the concept of fertility has evolved and increasingly expanded beyond the horizon of everyday language, including Graunt's view of fertility. The recognition of this theoretical evolution in fertility theorization is very important for one to be able to come to terms with current need of further theoretical development in this field.

From natality to fertility: the problem of simplistic comparisons

This debate should not get tangled up in historical details. It seems understandable that textbooks summarize the history of the discipline as briefly as possible, but when such a summary becomes distorted the only way to correct the inaccuracies is to turn to the original events. This is what I have done with regard to Graunt's *Observations* (Francisco, 1995). Here, I will do it again, though not extensively, with respect to the evolution and development of one of the most important concepts in demography.

Demographers usually speak about fertility in terms of its measure, that is the number of births relative to the number of women of reproductive age. Conventional textbooks generally caution readers against the linguistic confusion in the use of this term in English as compared to the Latin languages such as French, Italian and Portuguese (see Newell, 1988: 35; Lucas, 1994: 44). The fact that fertility is measured for women only is usually treated as given, and only few authors find it necessary to provide any explanation for that: '*Fertility* refers to the number of live births, and is more easily measured for women because they, and not men, actually give birth to babies' (Lucas, 1994: 44). Some scholars, such as Brouard (1977: 1124-1157) and Paget and Timaeus (1994: 333-340) have calculated male fertility; but in general demographers have never found valid reasons to believe that their understanding would improve substantially if male rates were used extensively in parallel to those based on females. This is because male TFRs are calculated in the same way and with the same objective as female TFRs. To insist on that is more or less, as Udry (1994: 562) put it, to try to invent a theory or a measure when a good one already exists.

The tendency to reduce fertility to its technical and measurable procedures has often lead students of population to miss the methodological implications of its conceptual subtleties. The same happens when the application of fertility measurements to women only is justified, if at all, in terms of common sense. An awareness of these positions should help to clarify not only why conventional literature insists so much on the simplistic comparison between the development of fertility and mortality, but also to understand the implications of emasculating the topic of fertility of any relevant theoretical principle.

The most important aspect in this comparison refers to the way age is used in mortality and fertility. A quick way to stress the principal difference between the use of age in the two cases is to imagine the paradoxical situation in which someone would attempt to create a mortality table relating all deaths in a given population with the ‘age of their mothers’. No demographer analyses deaths in that way, simply because the relationships that need to be explained in mortality are expressed is a very different way from fertility; the relationship between the events of death and birth are related with the timing of their occurrence in substantially different form and this entails significantly different conceptual and methodological problems.

The comparison between the development of fertility and mortality can be useful not so much to demonstrate that fertility came of age in demography later than mortality, but to highlight the conceptual and methodological differences between them. This is so because it is not correct to pretend that the concept of fertility in demography is as obvious to everyday language as the concept of mortality.

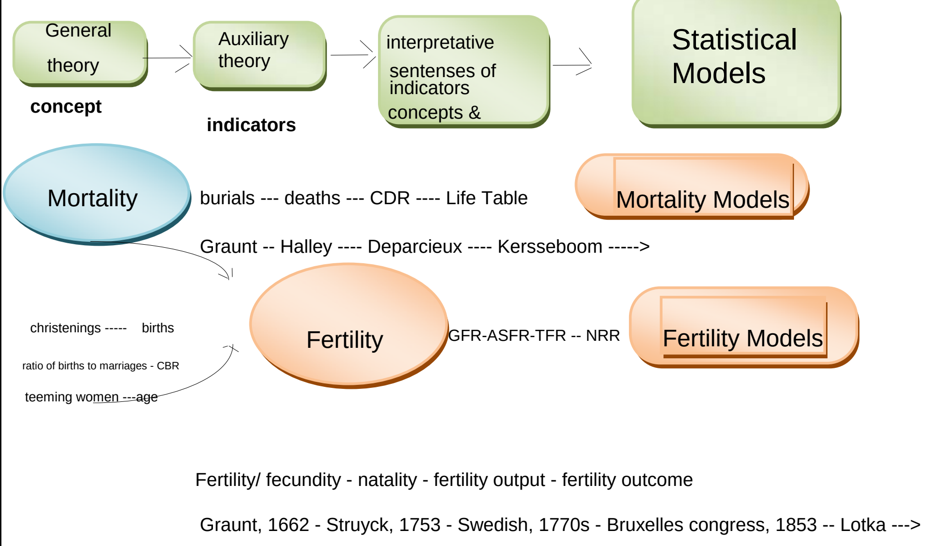
Returning to Wunsch again, his 1984 paper is useful to clarify some important features in the logic of demographic discovery, in this case features concerning the conceptual and methodological differences between mortality and fertility. Wunsch’s (1984: 2) assertion that ‘The crucial step is transforming the theoretical concept into observational indicators’ may be immediately applicable to mortality but not to fertility. The usage of the term mortality in commonsense language is not significantly different from its scientific conceptualization in demography. Likewise, the record of the time of the event concerning the death of individuals is more or less straightforward in the case of mortality.

This is apparent in Graunt's *Observations* and explains why this book has been credited as the inception of mortality analysis immediately after being published in 1662. Graunt easily identified the deficiencies in the data on mortality, particularly the fact that they referred to burials and thus did not cover deaths in general. This was possible because the statistical data concerning deaths are as given as the concept beneath them. What was lacking for the debut of the field of mortality was, in part, the social motivation to take interest in the records available. The Great Plague created such a motivation, and Graunt added to it his own scientific curiosity; that is, rather than just looking 'at the foot, how the burials increased or decreased’ he organized the data ‘for other and greater uses’ (Graunt, 1662: 14).

Graunt himself transformed burials into surrogates of deaths. This is the reason the concept of mortality appears in Figure 1.6.2 at the first stage of the logic of demographic discovery; burials are followed immediately by deaths and then the specific measures and models. So, in this context Graunt emerges as the pioneer of the crucial step towards the transformation of the abstract concept of mortality into specific observational indicators. Whether one follows Behar's idea that the development of mortality studies was somewhat uniform and smooth, or Diamond and McDonald's (1994: 29) more critical remarks about relative fragmentation in the study of mortality until the nineteenth century, in at least one aspect these demographers agree that the systematic analysis of mortality started not in Roman times, as Newell (1988: 63) put it, but with Graunt's *Observations*.

In short, each individual produces his or her own death. So, in mortality analysis the event of death is related to the individual's characteristic of growing older or ageing over time. In quantitative terms, this can be represented by the variable age as starting always at birth with zero, and increasing linearly with time; mathematically, this can be expressed by the first-order differential equation $\frac{da}{dt} = 1$.

Figure 1.6.2: Comparison of the logic of the discovery of mortality and



Source: Wunsch (1984), Dupâquiers (1985)

In turn, the history of fertility has been very different from the beginning. Just as statisticians and demographers are not supposed to tabulate the number of deaths by age of their deceased's mothers, they are not expected to aggregate the number of births by age simply because all birth have age zero. Besides the fact that the ability of an individual to be fecund depends on age, the birth of a child involves the emergence of a completely new entity or individual. This is clear to commonsense, but beyond that people not familiarized with demography make no explicit association between the attributes of the two separate individuals dealt with by the demographic concept of fertility: mothers classified by age at confinement and the child's birth. That such an association is not obvious to commonsense is apparent when the first anniversary of a child is commemorated in certain societies; the motive of the celebration is the baby, and only very few people still remember to give a flower to its mother.

With regard to *The Observations* it is apparent that fertility as it is perceived in demography was not obvious to Graunt's commonsense. Just as deaths were recorded in terms of burials, the data referring to births recorded the event of baptism. Graunt commented on the limitations of the data on births and discussed the reasons why christenings were neglected more often than burials, but in his remarks he did not go beyond the level of commonsense. Even from the point of view of natality, in which births are related to the whole population, Graunt's remarks need to be considered very primitive and crude. This does not mean that Graunt's discussion of the data on christenings was irrelevant from the point of view of fertility analysis. The fact that Graunt sketched the first attempt at systematic analysis of demographic change is in itself of merit and he went as far as his own background and knowledge allowed him to go. Graunt did not find in his data set the necessary information required for a fertility analysis in the modern sense; but to see this as a limitation is as valid as charging him with the fact that his arithmetic calculations show some mistakes which he could have avoided if he had used a calculator, or even better a Microsoft Excel Spreadsheet. In other words, this is a bad way of dismissing earlier theoretical and scientific progress in which the overall demographic concept of fertility needs to be understood as a process of creation and invention.

To understand adequately the lack of fertility data demographers should pay attention to the conceptual foundations beneath the motivations and the systems of data gathering. Surely, Graunt did not undertake a fertility analysis in the way it is understood in current times because he had no adequate data, but data do not exist in the field until researchers harvest them. If Graunt had imagined the concept that is embodied in fertility data, without doubt his interrogating and insightful mind would probably have attempted the sort of estimations that, for instance, the United Nations Population Division provides for those countries which today still lack adequate fertility data. Despite the lack of data the UNDP can now provide relatively good estimates of fertility for countries with inadequate data for the simple reason that demography now has the conceptual and methodological machinery to do it. Of course, this was not the case in the time of Graunt and for many years after he first organized the crude data on baptisms that were available.

In any case, regardless of the deficiencies in the data created by earlier demographers it would be totally misleading to dismiss what they achieved with the few tools that they had. About sixty years ago Kuczynski (1938) published a paper that thoroughly illustrates this point; although the paper deals with British demographers, its excellent review of the literature is certainly enough to counter the more judgemental overviews given by contemporary authors. So instead of trying to outline something similar here, it seems more appropriate to highlight Kuczynski's own paper. Box 1.6.1 provides a summary of Kuczynski's perusal of the British demographic literature between 1660 and 1760, that is 'during the century preceding the Industrial Revolution' (Kuczynski, 1938: 325); the titles in bold and the subtitles in italic correspond to the actual text of Kuczynski's paper.

Kuczynski's paper published in 1938 differs from more contemporary reviews, including those from Behar (1985) and Dupâquier and Dupâquier (1985) because he allowed earlier authors to speak for themselves through appropriate quotations; his review covers a wide range of fertility conceptualization, from the difference between fecundity and fertility and the causes of reducing fertility, to differential fertility and the means of promoting fertility.

Kuczynski's paper summarized in Box 1.6.1 offers to the reader a close contact with the ideas of earlier demographers. Perhaps the statements from this or that author can be used to give authority to several current interpretations, but at least the paper leaves room for more than one point of view. More recent papers, some of them reviewed in this chapter, contain also useful information about the changes over time in the technical understanding of fertility. However, whether or not contemporary authors deal with demographic techniques as part of a broader theoretical development is something that cannot be determined by earlier demographers.

Box 1.6.1 British Demographers' opinions on Fertility, 1660 to 1760 (Kuczynski (1938: 283- 327)

Fecundity and fertility Hume Graunt, 1662 Petty, 1671 Halley, 1693 Short, 1750 Graunt, 1662 Petty, 1687 Halley, 1693	There was a consensus of opinion among British demographers in the century preceding the Industrial Revolution that fertility, i.e. the actual production of children, lagged considerably behind fecundity, i.e. the child-bearing capacity. Some writers of the period thought that this had always been so. Thus David Hume (p. 159) states (1752) that 'there is in all men, both male and female, a desire and power of generation more active than is ever universally exerted'. Other writers rather emphasize the big gap between fecundity and fertility in the England of their time as compared with former periods or with the American colonies. Most demographers contented themselves with pointing to the fact, but some made attempts actually to measure fecundity. *Reproductive age* - Graunt estimated the childbearing period at 25 years ('between 16 and 40, or between 20 and 44'), and the reproductive period of men at 40 years; - Petty counts 25 years for females and 40 years for males; in the same year he figures the childbearing period at 27 years (between 18 and 44 years); in 1682 he assumes 30 years (15 to 44 years) for women and 42 years for men (18 to 59 years); in 1687 he speaks of the 'teeming women of between 16 and 45 years old'. - Halley considers 29 years as the childbearing period (above 16 and under 45); - Short states that 'Women are generally sooner marriageable than Men by four or five Years'. On the other hand, men 'are longer capable of procreation ... till 80 or 90; Women seldom beyond 45, but very rarely above 50'. He reckons the childbearing period at 30 years (between 15 years of age and 45). *Reproductive capacity* - For Graunt 'a man be *Prolifique* forty years, and a woman but five and twenty, which makes the males to be as 560 to 325 females'. He adds, however, that the excess of males over females at the reproductive age is smaller because 'more men die violent deaths then women ...'. As to married women at child-bearing age, he estimates them at one-sixteenth of the total population. He assumed for London proper 384,000 inhabitants, 24,000 married women of childbearing age, and 12,000 yearly births. - Petty follows rather closely Graunt's argument, though he add a tendency to estimate fecundity lower. He seems inclined to estimate the maximum possible fertility rate at 400 or 417 per 1,000 women at childbearing age. He implied that fertility rate of women lagged behind fecundity rate. -Halley thinks that if all women at childbearing age were married a general fertility rate of 667 'would not appear strange or unlikely'.
Causes reducing fertility Graunt Short, 1750 Morris 1751 Arbuthnot 1710 Short, 1750 Wallace, 1753 D'Avenant, 1699 Tucker, 1755 King, 1696	The demographers of the period give manifold reasons why fertility is lower than fecundity. *Diseases* Graunt, Short, Wallace; *Birth control* - Graunt refers to 'unlawful copulation' which 'beget conceptions but to frustrate them by procured Abortion; Short complains of the 'Destruction of real beings in the Womb'. *Abstention of wives* Graunt and Morris refer to the reduction of fertility caused by the temporary abstinence of country women when husbands are absent. *Suckling of children* Petty suggests that 'long suckling of children' be a 'hindrance to the speedier propagation of mankind'. *Polygamy* Graunt, Arbuthnot, Short, and Wallace are of the opinion that polygamy reduces fertility. *Promiscuous sexual intercourse* Graunt, King, D'Avenant, Morris, Wallace - most demographers of the period are convinced that sexual intercourse with another man than the husband and promiscuous sexual intercourse in general seldom are followed by conception. *Intemperance* Short though that polygamy reduced fertility because 'it requires too frequent gratification of the amorous passion'; Graunt states that 'intemperance in feeding' in London certainly does 'hinder breeding'. *Obstacles to divorce* Wallace and Tucker though that the difficulty of obtaining divorces slightly reduced fertility.

	Age at marriage King considers 'that a just equality, or too great an inequality of age, in marriages, are prejudicial to the increase of mankind; and that the early or late marriages, in men and women, do tend little to the propagation of the human race'; Tucker emphasizes the infertility of late marriages. *Celibacy* All demographers of the period agreed that the obstacles or the disinclination to marriage were a most important factor in reducing fertility: 1) Monks and Nuns, 2) Servants, 3) Apprentices, 4) Soldiers, 5) The Poor, 6) The Wealthy, 7) Political and economic system, 8) Public opinion, 9) Religious prohibition of marriages, 10) Care for illegitimate children.
Differential fertility	*Unmarried and married women* Starting from the assumption that unmarried women, having sexual intercourse, promiscuously admit numerous men, the demographers of the period agree that the sexual intercourse of unmarried women is less apt to lead to conceptions than that of wives with their husbands.
Graunt, 1662	*Urban and rural dwellers* Most demographers emphasize the smaller fertility of married couples in the cities than in the country.
Short, 1750	*The Poor and the Wealthy* Short states that 'the most laborius Part of Mankind are also the most fruitful in proportion to their Numbers; and the most voluptuous, idle, effeminate and luxurius are the barrenest ... hard Labour makes the Poor more fruitful'.
Means of promoting fertility Graunt, 1662	- Graunt recommended 'encouraging Marriage, and hindering Licentiousness'.
Petty, 1927	- Petty: 'The first command of God was to increase and multiply. Wherefore the law for marriages is that which will cause the most increase of people'; he proposed several legal measures and urged a complete organization of the institution of marriage in order to promote fertility specifically in Ireland.
Halley, 1693	- Halley recommends discouraging celibacy through special taxes and military service, and to encourage marriage through privileges for families with numerous children, and through better care for the poor.
D'Avenant, 1699	- D'Avenant: 'our Polity is some way or other Defective, or the Marriages would bear a nearer proportion with the gross Number of our People ... a large Proportion of the Females remain unmarried ...'. To promote marriages, he recommends 1) 'securing the Liberties of a Nation, 2) abolishing the taxes on marriages and births, 3) granting privileges and tax exemptions to families with numerous children, and excluding bachelors from certain offices, 4) compelling the father of a illegitimate child to marry its mother.
Short, 1750	- Short is of the opinion that since the poor constitute the bulk of 'all ... a Nation' and since they 'are generally the most prolific', all efforts to promote fertility should focus on encouraging the poor to marry.
Morris 1751	- Morris demands a tax on keeping numerous unmarried servants and further proposes that only married people be permitted to start a retail business.
Wallace, 1753	- Wallace is rather shy in making concrete proposals, but proposes a particular scheme for encouraging marriage.
Tucker, 1755	- Tucker: 'the married State is the only efficacious Method of increasing the Numbers of Mankind, and rendering a Country truly populous'. He recommends several methods for encouraging matrimony aiming at both sexes.
Bell, 1756	- Bell proposes four methods and concludes: 'those arts, by which we are supplied with food and all other necessaries, tend directly to promote the populousness of a nation' through each of those four methods, while 'commerce and the arts of elegance and refinement ... are far less adapted to promote the increase of a people', he recommends the utmost promotion of 'agriculture and the more necessary employment', and with this object in view demands 'an equal division of lands', supported by suitable laws of succession 'in a well-constituted republick'.

Conclusion

A perusal of the British demographic literature during the century preceding the Industrial Revolution affords very scanty testimony on the practice of birth-control. There were, to be sure, writers who, in discussing population growth, pointed to practices preventing contraception and procuring abortion, and also to differential fertility between urban and rural dwellers, between the well-to-do and the poor. They referred, however, merely to birth control by unmarried women and they did not intimate that differential fertility of married women was due to any deliberate action but rather to physical disability. To encourage matrimony and to hinder intemperance and licentiousness seemed to them the best and practically the only means of promoting fertility.

The Aristotelian subtleties of procreation and fertility determinants

Although women have always been the only producers of children, the recognition of their biological procreative role as compared with that of men was not always as obvious as it certainly appears for many lay people and scientists of today. This is what I call the Aristotelian subtleties of procreation and fertility determinants to stress that an attempt to grasp the evolution of the concept of fertility in demography should not be isolated from the dominant broad-ranging system of philosophical and scientific ideas.

The recognition that man's semen is not, as had been thought since the days of Aristotle, the only crucial element in procreation was settled scientifically only in the seventeenth century (Tannahill, 1980: 249). Before that the religious and philosophical representations had long replaced female deities with male representations of sources of life, such as God and Adam.

As Cadden (1993) explained in a recent investigation, the answers to questions such as ‘Did women as well as men produce procreative seed? What are the male and female roles in reproduction?’ started to create a network of flexible concepts in the Middle Ages. By this time issues surrounding reproduction and sexuality became increasingly important in writings about gynaecology, the human constitution, foetal development, or the naturalistic dimensions of divine Creation. However, as Cadden (1993) showed, even though such concepts did affect views, for instance, of the health consequences of sexual abstinence for women and men, and the allocation of responsibility for infertility, they did not endorse a single model of male-female relations.

Thomlinson's (1976) remarks on the long-lasting conceptual difficulty of assigning responsibility for sterility and fertility deserve to be quoted here at some length:

> Opinion has shifted regarding the relative importance of the father and the mother in the physiology of reproduction, but Western culture has traditionally assigned responsibility for sterility to the female. Aristotle felt that the man's semen was the source of life, and that the female merely nourished the fetus. Descartes accepted the theory that both sexes emitted semen in coitus and compared the chemistry of reproduction to that of brewing: 'The semina of the two sexes mingle and act as yeast, each on the other.' The biseminal theory was attacked in 1651 by William Harvey, the discoverer of the circulation of the blood, who founded *ovism*, the doctrine that the female element is decisive in procreation. Carolus Linnaeus summed up Harvey's thesis in an epigram: 'Vivum omne ex ovo' (everything living comes from the egg). The Dutch lens-grinder Anton van Leeuwenhoek put semen under his invention, the microscope, and became the first man to see the small swimming creatures which he called spermatozoa - a blow from which the ovists never recovered. Yet it was over two centuries before biologists were able to produce microscopic evidence of the fertilization process in human beings. Today the essentiality of both the male sperm and the female egg are well recognized (Thomlinson, 1976:167-168).

Indeed, if nature and the physiological traits of individuals were once good allies of androcentric explanations of human reproduction that men are said to have invented, their scientific basis was extremely weak; surely, as weak and distorted by empirical observations as common sense or the Ptolemaic picture of the Universe.

So, one cannot pretend that authors such as Graunt, Malthus and Darwin discovered nothing that Aristotles already knew. In a cultural environment in which men were perceived as the source of life, while women were little more than incubators, the calculation of fertility rates in terms of women could hardly be regarded as obvious. It is hard to imagine that before the seventeenth century the conceptual link between the age of woman at confinement and the determinants of her child's conception and birth could be considered 'natural'.[7]

As Tannahill (1980: 249) put it, the implication of the seventeenth century scientific breakthrough which has clarified the biological role of each sex in reproduction was that 'woman became not an incubator, but a mother'. Seen from the viewpoint of demography, perhaps even the history of perceptions of motherhood seems to have evolved in parallel, if not in close association, with the increasing recognition that females matter as much as males in the conception of the offspring.

Fertility in demography: concepts versus data

The evolution of the conceptual and technical understanding of fertility in demography has been interpreted in two main ways: most authors attribute the weakness of fertility measurement in earlier demography to the inadequacy of data, while a few stress its conceptual reasons.

Behar's paper published in 1985 provides an example of the first interpretation. Behar starts by pointing out that the age of females at confinement is the most important factor to be taken into consideration in the study of variations of fertility. This understanding is not found among the demographers of the seventeenth and eighteenth centuries, says Behar, for they make almost no mention of differential fertility; nor was the analysis of fertility as a demographic phenomenon a subject on its own account and separate from other demographic indicators. 'The absence of adequate data is one reason', Behar concludes without even attempt to offer any further evidence for his assertion.

Dupâquier and Dupâquier (1985: 354-365) take the same position in their monumental *Historie de la Démographie*. In a chapter called 'The tools of demography', the Dupâquiers explain that before the eighteenth century demographers gave to

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[7] The Scientific Revolution that is said to have began with Copernicus was a revolution because a powerful views of the world, such as those of Ptolemy, Aristotle and the Bible that had been accepted dogma for more than a thousand years (Capra, 1982: 38). But just as after Copernicus the earth was no longer the centre of the universe, after the essentiality of both the male sperm and the female egg become recognized male could no longer remain the center of procreation that Aristotle imagined. According to Lerner (1986: 206), Aristotle considered that life was created by the meeting of sperm and what he called catamenia as 'semen' or 'seed':

> If , then, the male stands for the effective and active, and the female, considered as female, for the passive, it follows that what the female would contribute to the semen of the male would not be semen but material for the semen to work upon. This is just what we find to be the case, for the catamenia have in their nature an affinity to the primitive matter (Aristotle, in Lerner, 1986: 206).

demographic analysis two remarkable tools: the table of mortality and the concept of stable population. Starting with Graunt's table of proportional distribution of deaths, passing through the Huygen brothers' distinction between life expectancy and probability of surviving, Halley's concept of closed population, King's radix, Deparcieux's stability and Süssmilch's quotient, Euler brought all these concepts and methods together and laid the foundations for the theory of stable population.

'On the contrary', the Dupâquiers (1985: 334) continue, 'fertility was ignored or badly measured, and so it remained during the major part of the nineteenth century'. Here these historians of demography seem to have lost track of the main point. Above all, the assertion that 'fertility was ignored or badly measured' shows little sense of historical perspective. Dupâquier and Dupâquier do not acknowledge that the demographic concept of fertility was in the womb of creation and thus in process of gestation; perhaps for exactly the same reason they do not explain why Euler conceived the concept of stable population but could not go far without an adequate concept of fertility.

In their description, the Dupâquiers refer to an author who can be associated with the second interpretation pointed out above. In 1981 Le Bras wrote a paper called 'Sur les outils de la démographie' in which he comments:

> Throughout the XIX century, fertility was measured casually. Is it the male? Is it the female? Is concerns both. Is it the age? Is it the duration or the process of marriage? Perhaps neither of them. That is it (Le Bras, 1981: 76-101, cited by Dupâquier and Dupâquier, 1985: 354).

The authors of the *Historie* consider this description a harsh judgement; they add that Le Bras attributed the weakness of fertility analysis to 'the mystery, the scandal and the embarrassment that surrounded contraception, particularly in countries such as France' (Dupâquier and Dupâquier, 1985: 355). It is not clear from this assessment whether Le Bras really considered earlier stages of fertility analysis a 'weakness' (*faiblesse*) rather than a process of growth. In any case, at least the quotation indicates that Le Bras valued the development of the demographic concept of fertility as a theoretical debate, as well as a dynamic and necessary process in scientific investigation; he conjectured about what might have occurred before demographers reached an agreement about the main operational definitions in fertility analysis.

The Dupâquiers countered Le Bras's explanation and asserted that most probably the weakness of fertility analysis reflected the inadequacies of the descriptive statistics: 'no country, except Sweden, registered the age of mother at confinement, thus demographic analysis lacked the materials to be exercised' (Dupâquier and Dupâquier, 1985: 355). And because this situation did not improve for fertility, the Dupâquiers concluded, the progress continued to be restricted almost exclusively to the area of mortality. 'It is only by the end of the 19th century that fertility analysis was able to develop', so the Dupâquiers write. 'Only then was demography able to develop in new directions: population dynamics and the construction of models' (Dupâquier and Dupâquier, 1985: 355).

This review seems enough to make the main point that needs to be understand clear. While the interpretations of, for instance, Behar and the Dupâquiers are not false, they are clearly partial and misleading because they overlook the substantial theoretical differences encountered by earlier demographers when they started to study mortality and fertility. This suggests that the innocent description of records of population theories, data gathering, earlier systems of data registration is not enough for one to come to terms with the dominant view in earlier development of demography.

Perhaps, the strategies applied by other social scientists in their studies of earlier societies could be of some value to understand better the development of demographic tools since their earlier times. Contemporary hunter-gatherer communities have in recent decades been of much use to understand the relationships between demographic events and economic, technological or other phenomena in earlier societies (Boserup, 1976; Howell, 1986; Davis, 1986). Likewise, the current primitive systems of demographic data collection in developing countries could also be used to understand earlier developments in demographic theory. Many developing countries still lack adequate systems of demographic information, though in the past few decades there has been an enormous international effort aiming to improve the statistical capacity and systems of data collection all over the world. However, creating an adequate statistical systems concerns as much technical, institutional and financial foundations, as well as a conceptual capacity or even a scientific culture need to even decide what data quality means.

On this the following two examples may help to illustrate the point. One is from Kuczynski's book published in 1935:

> The fact that nowadays fertility is the decisive factor in determining population growth has had a marked influence in shaping the statistical work in newer countries, but some of the older countries still cling to the antiquated methods of measuring fertility, and do not even provide the basic birth data for an accurate measurement of fertility (births by age of mother) ... This lack of adequate data certainly fosters the use of inadequate methods for measuring fertility. But even where adequate birth data are available, the best use is not always made of them because the analysis of birth statistics, unlike the analysis of death statistics, is not yet considered professional duty involving great responsibilities (Kuczynski, 1935: 5).[8]

[8] In this book, Kuczynski discussed not so much the broader aspects associated with the concept of fertility but what he regarded as the fallacy of the methodological attempt to ascertain fertility by relating births to marriages:

> Short, like all his predecessors, John Graunt, William Petty, Gregory King, William Derham, was not aware that the births for other, more important, reasons cannot be safely related to the marriages of the same year. The first who had some misgivings in this direction was Süssmilch (1761), when he realized that the extraordinarily high ratio of births to marriages in Prussia in 1756 was caused by the sudden drop of marriages due to the outbreak of the war in that year.
>
> While Süssmilch noticed the effect of a sudden change in the number of marriages upon the ratio of births to contemporaneous marriages (3b), he did not see that the population trend itself is a disturbing factor (3a). This was first pointed out by Richard Price (1769) (Kuczynski, 1935: 34-35).

Kuczynski then traced the development of specific fertility methods and mentioned their application by authors such as Malthus's (1807) use of Price's method, Sadler (1830), Bernoulli (1840) and Farr (1842) (Kuczynski, 1935: 33-65).

Another example came to my knowledge in the process of elaboration of this chapter. Dr Neupert called my attention to a current example drawn from his own experience of work for some years with the Statistical Office of Mongolia (SOM), which illustrates the emphasis placed in this chapter on the increasing improvement in the demographic concept of fertility and its importance for the quality of data gathering and their analysis. Until late in 1980s, the amount of population data collected by the Mongolian statisticians and demographers was quite impressive. However, the data were usually tabulated in a *ad hoc* manner and with little awareness even for fundamental characteristics such as age and sex. Analytical demographic measures were seldom computed and presented in reports; many tables contained only absolute values and no substantial interpretations, analyses and evaluations. Curiously, in a report that Dr Neupert kindly allowed me to read, Mongolian researchers are said to be reluctant 'to absorb the technical support provided by the international technical project of support'; or, in another passage, the 'SOM officials usually showed a distrust and dogmatic attitude towards using more modern and up-to-date procedures and techniques of population data collection, tabulation, evaluation and analyses' (Neupert, 1992). But in our personal conversations Dr Neupert admitted that the Mongolian demographers usually did not question the need to determine accurately the population size and some vital rates. The fact that they have tabulated the number of women who had children by age groups but did not classify the births according to the age of mothers can be seen as lack of understanding of the concept of fertility (output). Therefore, to charge the Mongolian demographers with unwillingness to abandon their outdated techniques would make sense only if they really had shown any different way of measuring fertility from the conventional in demography. However, it seems that until recently Mongolian demographers had an effective institutional system of data collection, but lacked an adequate conceptual understanding of the demographic concept of fertility.

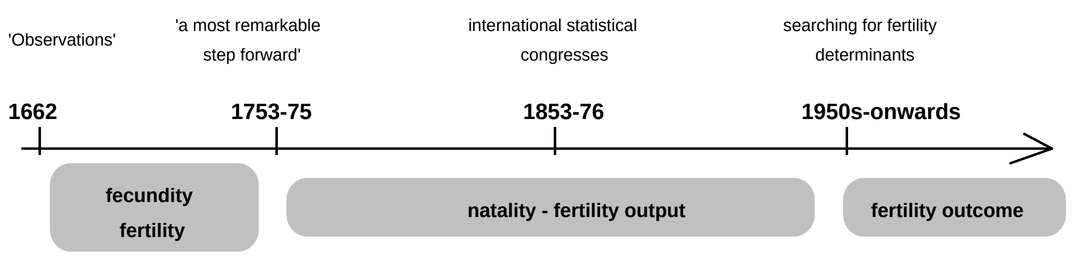
In short, it seems that only an historical interpretation of earlier developments such as the one proposed here can allow demographers of today to acknowledge that the concept of fertility in demography has evolved and such an evolution should not be considered finished. The papers used above, though open to criticism, contain valuable historical evidence to help us understand that fertility was not in fact ignored by earlier demographers. Instead, it required from earlier demographers both theoretical growth and a progressive process of moving away from the limited understanding they often found in terms drawn from commonsense language. This alternative historical perspective is necessary not just to overcome misconceptions about the earlier history of demography but to set an adequate basis to comprehend what lies ahead. In the following section the information provided by Dupâquiers' *Histoire* and other authors mentioned above, especially Westergaard (1932), Lorimer (1959) and Behar (1985, 1987), is used to sketch an alternative periodization of the metamorphoses of fertility in demography from Graunt to the present.

The evolution of fertility: three breakthroughs in leaps of one hundred years

Figure 1.6.1 offers a sketch of the demographic concept of fertility in its logical evolution in three bifurcations and stripped of any historical detail. This section demonstrates that the historic development of fertility is to its logical evolution what the fertility process is to its results measured in terms of levels and trends. The three bifurcations depicted by Figure 1.6.1 can be seen as the synthesis of three important processes in the development of demographic ideas. Curiously, the culmination of each of the three bifurcations are roughly separated from one another by a range of about one hundred years.

Figure 1.6.3 depicts on a time-line what Figure 1.6.1 sketched logically. The first bifurcation in the demographic concept of fertility can be traced to the period between 1753, when Struyck first computed the general fertility rate (Kuczynski, 1935: 106) and 1775, the date when the Swedish demographers explicitly decided to gather data concerning the age of mothers bearing children in five-year age groups (Behar, 1987: 179; Dupâquier and Dupâquier, 1985: 365; Lorimer, 1959: 143-144).[9] This event can be seen as the culmination of a process that lasted about one hundred years, since Graunt attempted his first systematic analysis of the subject in 1662. In this period demographers' analysis was set mainly by drawing on the commonsense understanding of fertility and fecundity.

Figure 1.6.3 The avatar of fertility: three breakthroughs in leaps of one hundred years



As Box 1.6.1 shows, Graunt and other early demographers expressed a clear perception of the gap between fecundity (childbearing capacity) and fertility (the actual production of children). However, from a more strictly theoretical perspective these two

9 According to Lorimer (1959: 144):

The Swedish Statistical Commission, under Per Wargentin's leadership, began in 1775 to collect information on confinements by women in quinquennial age-classes. H. Nicander computed age-specific confinement rates for the period 1780-95 which were published in the *Transactions of the Swedish Academy* (1800). These were reviewed by Milne in his *Treatise on the Valuation of Annuities* (1815) (Lorimer, 1959: 143-144).

terms can be said to have remained conflated until the Swedish demographers set the basis for the development of concept of fertility output. So the first one hundred years correspond to the period in which demography had to distance itself from the commonsense view of fertility. As Westergaard correctly put it, the event of 1775 was ' a most remarkable step forward'.

The significance of this step is not so much for the reasons Wunsch (1984) considered the 'crucial step' as the transformation of an abstract concept into observational indicators. Moreover, it should be noticed that more or less at the same time when in the area of fertility the first bifurcation was about to take place, in mortality the development from Graunt to Süssmilch also culminated in an important synthesis:

> The construction of life tables naturally led to the theory of stable populations, a concept invented by the great Swiss mathematician Leonhard Euler and generalized by another great mathematician, Pierre-Simon Laplace (Behar, 1987: 270).

However, this statement from Behar does not make it clear that stable population theory does not refer to mortality only. It did not occur to Behar because the demographic concept of fertility was in the process of gestation; thus it should also be 'natural' to conclude that by the time Euler invented the concept of stable population, he could simply anticipate the stable population theory that Lotka would much later outline. Euler related a constant mortality with births increasing exponentially over time; but as Keyfitz made it clear in his brief introductory note to Euler's (1760) paper:

> Euler uses 'hommes' for the population and 'enfants' for the births, which could be interpreted as meaning that the sexes are combined in his model. As he nowhere deals with age at childbearing, his argument would apply to the two sexes together, with a consolidated life table. Essentially a one-sex model seems to be intended, with males as the illustration (Keyfitz, 1977a: 85).

Euler's intention had to wait about 150 years before turning into reality. Two thirds of this waiting time corresponds exactly to the period in Figure 1.6.3 between the 1770s and 1853-76. This period refers to the time that led to the second bifurcation between natality and fertility output.

Following 1775, the process leading to the explicit demographic distinction between fecundity and fertility was still not easy, though in this period demographers were already dealing with the matter in more theoretical terms. In 1815, Joshua Milnet defended the distinction between what he called 'potential fertility' and 'effective fertility' (Behar, 1985: 181). Still in 1835, Behar points out, Adolphe Quételet, like Süssmilch, wrote about 'population fertility' in reference to the annual births related to the total population, and about 'marital fertility' in reference to births related to the number of marriages occurring within the same calendar year. According to Dupâquier and Dupâquier (1985: 370-372), in 1866 the work of Matthews Duncan, *Fecundity, Fertility, Sterility and Allied Topics* originated a great, and perhaps the crucial, debate around the notions of fecundity and fertility.

The sources I have been using in this discussion provide enough evidence to conclude that demography needed another hundred years to establish a demographic concept of fertility clearly distinct not only from fecundity but also from natality. As compared with the first bifurcation, between 1770s and 1853-76 the debates are more scientific; demographers were more concerned to define clearly what the term fertility should mean within the scope of their study of population than to insist on what everybody already agreed: that in demography there should be a clear-cut distinction between the concepts of fertility and fecundity.

The period of the international congresses of statistics held between 1853 and 1869 can be regarded as decisive in bringing demographers to a general agreement on the notion of fertility output. The First International Statistical Congress, held in Brussels in 1853, recommended that births should be recorded internationally according to the age of mother at confinement. This seems to have been the second 'most remarkable step forward' since 1775.[10]

Of course, the 1853 international statistical congress was preceded and followed by important debates and extensive search for adequate measures of fertility output. By the end of the eighteenth century, two main crude indexes of fertility were used: (1) the estimate of the number of inhabitants for each baptism, that is the inverse of the natality rate; (2) the account of numbers of baptisms in relation to marriages.

Demographers were aware of the limitations of these indexes and, thus, they did not stop searching for ways to improve them. In 1798 Moheau (1745-1794) extolled the calculation of fertility on the basis of the number of women only and dealt specifically with the fertility of marriages in a long chapter entitled 'De la Fécondité des Femmes' (Behar, 1985: 187).[11]

Moreover, as Dupâquier and Dupâquier (1985: 365-366) noticed, Malthus introduced the notion of longitudinal analysis in his second edition of the *Principle of Population* published in 1803. But they also make it clear that Malthus never attempted to calculate 'natural' fertility; he only measured the productivity of marriages and, 'without clearly formulating the concept, he attempted to define the crude reproduction rate, or more exactly, an indicator of descent' (Dupâquier and Dupâquier, 1985: 366). The Dupâquiers are not wrong in asserting that Malthus opened a new direction in demographic research, which would not be explored until the end of the nineteenth century and beginning of the twentieth century by Lotka and Kuczynski. But even more significant is the fact that Malthus, like Graunt and Euler, still did not have a clear understanding of the demographic concept of fertility output that was about to be formulated.

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[10] The First International Statistical Congress was held in Brussels in 1853; Linder (1959: 331-332) provides an extract of the recommended resolutions, but only concerns the part relating to population census methodology. Eight more congresses followed: the second in Paris in 1855; the third in Vienna in 1857; the fourth in London in 1860; the fifth in Berlin in 1863; the sixth in Florence in 1867; the seventh in The Hague in 1869; the eighth in Saint-Petersburg in 1872; the nineth in Budapest in 1876 (Dupâquier, 1985: 304; Linder, 1959: 332-333).

[11] Moheau's book, *Recherches et Considérations sur la Population de la France*, is seen by some authors as the first true treatise on scientific demography (Nazareth, 1988: 23).

The fact that Malthus focused mainly on the productivity of marriages, instead of fertility output, shows the weakness of the Behar's periodization in his 1985 paper: 'that is roughly between Graunt and Malthus' (Behar, 1985: 174). Indeed, a careful look at the Behar's periodization provides the explanation for its weakness:

> Malthus's argument about the fertility index is terribly simplistic: marriages result from births. Therefore, in a closed population, the ratio of marriages to births will indicate the proportion of those born who have survived to marry. The argument is not more elaborate than that (Behar, 1987: 277).

Since the 1770s, demographers become increasingly concerned not only in setting up a clear-cut distinction between fecundity and fertility, but also establishing also a clear operational definition of fertility in the context of natality in broad terms. Several authors, including Milne, Moheau, Muret, Quételet, Price, Sadler and Süssmilch, seem to have contributed more to this specific issue than Malthus. Clearly, as Dupâquier and Dupâquier (1985: 367) mentioned, the debate around Malthus's theory since the first edition of his *Principle of Population* also contributed to the development of fertility analysis. For instance, in 1830, Sadler, one of Malthus's opponents, tried to demonstrate that the growth of a population is an inverse function of density; in so doing he provoked the interest in fertility analysis by age. 'Sadler used extensively the relationship N/M', the Dupâquiers (1985: 368) write,' but he also tries to improve fertility analysis by using the relation *number of children/number of women*'.

In this context the nine international statistical congresses held between 1853 and 1876 can be seen as the events which brought together a wide agreement among demographers as to the concept of fertility in the sense ever since used by conventional demography. They provided the opportunity for the exchange of ideas and technical knowledge among those who were directly or indirect involved in the subject. In these meetings some scholars were particularly influential, such as Quételet who also played a distinctive role in the organization of the congresses themselves (Dupâquier and Dupâquier, 1985: 304).

After the collapse of the system of international statistical congresses, other international meetings emerged. In particular, the first International Congress of Demography was held in Paris in 1878; this congress was organized by a new generation of demographers anxious to assert their personality and independence from the long-lasting subordination to statistics (Dupâquier, 1985: 312-314). Among its recommendations, the first congress of demography established new forms for data on marriages, deaths, and births. With regard to births the same congress recommended that information should include information on aspects such as: date and hour of birth, sex, ranking in the order of birth in the family, duration of gestation, marital status (legitimate, illegitimate), date and place of birth of father and mother; profession, status, and religion; place of birth, usual place of mother's residence and her name (Dupâquier and Dupâquier, 1985: 314-315).

Throughout the past century the definitions of natality, fecundity and fertility have developed into very detailed operational definitions, but time and again some authors have hesitated on their content and designation. For instance, several decades after Duncan proposed to use the term 'fecundity' to mean the quality of producing, and 'fertility or productiveness' to mean 'the amount of births as distinguished from the capacity to bear', Knibbs argued otherwise:: 'The matter seems of sufficient importance to abandon Duncan's usage' (Knibbs, 1917: 234):

> Owing to their phonic resemblance the words 'sterility' and 'fertility' are the more appropriate to employ in order to denote the difference between producing or non-producing; while 'fecundity', which biologically is used without qualificative to imply *producing in great numbers* (a meaning which requires the qualification 'great' when fertility is used), is obviously the more appropriate word to denote 'multiple fertility' (Knibbs, 1917: 234).

Whatever the reasons, it is obvious that the English demographers have widely agreed to follow Duncan rather than Knibbs. Whether or not they did it only to demarcate themselves from the Latin demographers, who actually use the terms ‘fecundity’ and ‘fertility’ in the way proposed by Knibbs, it may be admitted that the English usage is an elegant way of drawing a clear-cut distinction between the demographic concept of fertility and use of the same term in commonsense language.

In short, I have so far considered only two of the three bifurcations in the history of the demographic concept of fertility. The first bifurcation involved the Swedes even in the 1770s, ‘a most remarkable step forward’ for its qualitative leap in the consideration of fertility by earlier demographers. The second bifurcation can be associated with Wunsch's 'crucial step' from abstract theoretical concepts to observational indicators, for it has led to the clear-cut definition of the demographic concept of fertility as an output and natality. Following the second bifurcation, around the mid-nineteenth century, it did not take long to transform the concept of fertility into statistical models and more specific concepts. Curiously, it took as much time as in mortality after Graunt set the basis for its analysis and about four decades later Halley sketched the first mathematical model life table. Likewise, specific measures on fertility and reproductivity have emerged in the sequence of the establishment of the concept of fertility output. This included the invention of the net reproduction rate in 1884 by Richard Böckh, the director of the statistical office of the city of Berlin (Kuczynski, 1932: 15, 1935: 207; Lorimer, 1959: 154-155); the creation of the total fertility rate in 1907 by Kuczynski (1932: 7; 1935: 117). Then Bortkiewicz (1911) and, 14 years later, Lotka outlined the theoretical model of stable population (Kuczynski, 1935: 223-228). Thus, these authors separately took the concept of stable population from the lethargy in which it had remained since Euler; in particular Lotka, together with authors like Sharpe and Dublin, from 1907 to 1925 systematized the core theoretical body of the one-sex classical stable population theory.

The development of one-sex demography around the concept of the concept demographic output, such as the conventional definition of fertility and net reproduction rate, is treated at length in Part II. Yet the overview provided in this chapter should already have given a relatively good idea of the earlier stages and origin of the power of the one-sex methodological approach developed to deal with demographic outputs. Of course, an important issue that needs to be addressed is whether or not the fact that one-sex demography has increasingly become a female-oriented body of theory is just a matter of convenience. But before turning to this question, it is important to recall two historical anticipations of the third bifurcation in the demographic concept of fertility, both happening in the second part of the nineteenth century: the vision of Quételet and its application in 1895 by Hungarian demography, Joseph de Körösi.

Quételet on fertility: perhaps too idealistic, but not absurd

An interesting event that occurred in 1869 indicates that by then demographers had adopted the concept of fertility output. The Dupâquiers' *Histoire* leaves no doubt that its authors regard Quételet as among the great demographers of the nineteenth century. They show that Quételet contributed decisively to clarifying and developing the new directions of research on fertility set by Sadler. In particular, they mention the four factors which Quételet regarded as determinants of fertility: physiological age, the age combination of the spouses (*l'age croisé des époux*), mortality, and the attitude towards procreation: 'those who get married young are most likely to have a rather numerous family' (Quételet, cited by Dupâquier and Dupâquier, 1985: 369).

Yet the Dupâquiers did not consider all Quételet's lines of thought useful, perhaps because they considered that Quételet was more interested in creating social physics than demography. As they point out, the example of Sweden took about one hundred years to generalize and, here is 'the impulse given by Quételet, thanks to the international congresses' (Dupâquier and Dupâquier, 1985: 350).

> Unfortunately, that impulse had its good direction as well as limitations. As is known, Quételet tried to do social physics rather than demography. This is the reason that motivated him to ask for fertility statistics according to the father's age. Yet, demographers, who had been persuaded, as Moheau wrote, 'for the reproduction of the species, the female sex is the one to which the State has its most obligations because it is the one that produces', did not take long to find that such data have no major practical interest; little by little they stopped appearing in tables (Dupâquier and Dupâquier, 1985: 350).

This description refers specifically to the Hague congress, in 1869, which recommended that data on births should be collected not by age of mother only, as had been recommended sixteen years earlier in Brussels, but also according to the age of father. By then Quételet was very influential as a statistician and the organizer of the international congresses of statistics and certainly he played a decisive role in the recommendations of the Hague congress. Besides, he was not alone. ‘William Farr,’ Kuczynski described,

> before that Congress, had repeatedly deplored the lack of such information in his annual reports: ‘The English schedule is defective, as it does not show the age of the father and mother at the birth of the child’ ... But only once more, in 1875, Farr again drew attention to this lack of information in England: ‘All that is further wanted now in the English Birth Schedule to clear up this vital question

> conclusively is the entry of the ages of the mother and father at the birth of their children, and the order of the births' (Kuczynski, 1935: 130-131)

The Dupâquiers criticised Quételet for giving a privileged place to 'l'âge croisé des époux', and for this reason taking research in secondary directions. There are a few important points to notice in this criticism of Quételet's so-called secondary directions. First, it is clear that by the 1860s the analysis of fertility as a demographic output had come of age. This explains, at least in part, why the authors of the *Histoire* dismiss any attempt to go beyond the newly achieved understanding of fertility. Secondly, it is surprising that the Dupâquiers did not try to benefit from the distance in time and appreciate Quételet with an open-mind. They considered that demographers did not take long to learn that data on fertility by age of father 'have no major practical interest' (Dupâquier and Dupâquier, 1985: 350). Thirdly, the Dupâquiers' criticism of Quételet indicates that they considered fertility in demography as conceptually constant and static rather than a variable concept that has evolved and still needs to evolve.

Quételet may have been idealistic when he spoke of *physique sociale*, or attempted to prove that the phenomena in moral statistics were regular (Westergaard, 1932: 169). But his concerns with a wide range of statistics on *'homme moyen'* were far from absurd. To some extent the sort of data by age of father, that he urged the participants at the Hague congress to recommend, are currently provided by some few countries in the UN *Demographic Yearbooks* (e.g., 1986). Moreover, many of Quételet's concerns with social demography are now treated by demographers as being part of the content of fertility determinants. Demographers of today deal with issues that some one hundred or so years ago would had been included in the so-called 'moral statistics': ideal family size, desired number of children, and ideational fertility theories.

The fact that Quételet's idea of recording birth data by age of father still do not excite demographers does not necessarily mean that has 'no major practical interest'. In part, it means that demographers had come to term on how to define and measure the sort of relations I designated here 'fertility output'. The past more than one hundred years of research around fertility outputs shows that much work was awaiting to be done, and in most of that could well be done by focusing on the female sex only. Seen from today, it is hard to imagine that any further development in fertility, particular the one which requires an articulation of the two sexes, could ever develop adequately without the clarification of the demographic concept of fertility output.

In any case, Quételet's desire to gather birth data by age of father has generally been overlooked over past years by default rather than an explicit agreement based on persuasive arguments. To corroborate this inference I will pass to describing what may have been the first and most elegant research on the lines envisaged by Quételet. Exactly one hundred years ago Körösi, a Hungarian demographer, published his results of a remarkable investigation called 'estimate of the degree of legitimate natality as shown from observations at Budapest'. As the review that follows illustrates, Quételet's dream was applied some three decades later to a greater bulk of issues than Quételet himself imagined and, perhaps, than contemporary demographers are aware off.

Körösi's approach to fertility: the monogenous versus the bigenous approaches

Körösi (1844-1906), member of the Hungarian Academy of Sciences and the Director of Municipal Statistics, published a very interesting work in the *Philosophical Transactions of the Royal Society of London* (1895).[12] Körösi was probably correct when he claimed that his 'Estimate of the degree of legitimate natality' in Budapest provided the first complete table of natality. The reason he called it 'complete' deserves a detailed explanation because it entails the originality of his research.

Körösi started his paper with some preliminary remarks which placed the study of natality in the historical and scientific context. He maintained that both branches of the science of demography, respectively natality and mortality, originated with Graunt's *Observations*, and were immediately developed by Petty and especially Halley, who set the scientific direction of mortality analysis with the first model of the life-table. While the life table indicated the way to develop the measurement of the probability of death by age and sex, Körösi continued, the other branch of vital statistics had loitered far behind the death statistics:

> More than a hundred years have passed since MALTHUS stated that great demological[13] problem concerning the rapid multiplication of mankind - a problem which, since his time, stands as central argument in the controversy of the social question, and still occupies the attention of thinking men, and which will never more disappear from the horizon. Now, this great problem rests essentially on natal statistics. But in spite of the century which had passed between GRAUNT and MALTHUS, the latter, in his proofs, has still to struggle against the insufficiency of the statistical basis. And even to-day, another century after MALTHUS, we are obliged to admit that this basis is still so defective that it is impossible to attempt, on such a foundation, a solution of MALTHUS's problem. The points of view from which we observe to-day the phenomena of natal statistics are hardly more numerous than a century ago, in the time of MALTHUS and SÜSSMILCH (Körösi, 1895: 783).

Körösi admitted that a table of natality was not of so great scientific importance as the life table, but he sustained that as the problem of overpopulation or depopulation

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[12] According to Dupâquier and Dupâquier, Körösi was the most famous Hungarian demographer of his time. Together with Keleti, the Director of the Royal Office, they were the leading organizers of the last International Congress of Statistics in 1876. Like Böckh in Berlin, and Bertillon in Paris, Körösi was in his time one of the most active demographers in applying the recommendations of the international congresses of statistics. In 1881, Körösi proposed a project for a census of the world population; but in the opinion of Dupâquier and Dupâquier, Körösi was the only statistician of his time who thought deeply about the constitution of demography as a science. The *Histoire de la Démographie* concludes with a discussion of Körösi's reflections about the role and limits of demography, a paper presented in 1882 to the Fourth Congress of Hygiene and Demography (Dupâquier and Dupâquier, 1985: 271, 277, 337, 406-408, 418-421).

[13] The author used the term 'demology' as synonym of 'demography' (see Körösi, 1895: 782).

are an effect of two main forces, physiological and social, it would be worthwhile to study the law of such facts. So, a corresponding table of natality, showing the probability of birth for each of the age-combinations of the parents, became the main goal of his investigation.

Körösi asserted that the term 'natality' was formed on the pattern of 'mortality', was diffused especially by Bertillon and 'means generally the whole subject of natal statistics.' In the paper discussed here, Körösi attributed to the term natality 'a more restricted sense, meaning by it especially the probability of birth for a given age, and relating to the duration of a year' (Körösi, 1895: 783). Yet, Körösi added, 'as expressions like "male probability of births" are not only heavy, but might also be confounded with the term "probability of male births", I often prefer to use the term "natality" ' (Körösi, 1895: 783). In addition to his general remarks on the construction of a table of natality, he drew a parallelism and some notorious differences from mortality or 'life table'. While in a life-table the event of death is 'a natural phenomenon, independent of human will', the author maintained, in the case of fertility at least partly it 'is also influenced by voluntary causes' (Körösi, 1895: 783).

> Another difference is presented by the fact that death is caused by the physical conditions of a single person only, fecundity by those of two. The probability of having a child at a given age varies much according to whether the other partner is young, middle-aged, or old' (Körösi, 1895: 784).

With regard to Körösi's research design two aspects deserve some notice. First, he explained the conceptualization of his analysis on fertility in the following terms:

> we may be allowed to introduce two denominations for the two species of natal probabilities, naming those which regard only one sex as *monogenous*, and those which take into consideration the age-combinations of both parents as *bigenous.* It is clear that only the latter ones deserve the name of a true Table of Natality. Further, let us name those tables which proceed by single years *specified*, and those where the ages are put together by greater (quinquennial, decennial, etc.) groups *cumulative*. By a complete bigenous table of natality we mean now such a one, which shows the probability for each single year of age of father and mother, and that not only separately, but by combining *each* year of age of one parent with each year of the other (Körösi, 1895: 784).

So, with regard to the word 'complete' fertility table, Körösi meant what he said: one that referred to single year of age for both sexes combined rather than one at a time. Of course, such a concept of fertility entailed a third important difference from a mortality table of a methodological nature:

> Whilst the latter, if proceeding by the single years from births till to the highest age, is composed of about a hundred, thus for the two sexes of about two hundred yearly elements, this number rises in the table of natality to 1500-2000, this being the number of combinations for each of the 40-45 productive years of men with each of the 35-40 productive years of women ... In order to ascertain the probability of birth for each combination of ages, we need conditions; (1) that the census should tabulate the age of the married couples by combination of the single years; (2) that at each birth the age of the father and of the mother ought to be reported. If we then proportionate the figures of both sides, we get thus the probability of a birth for each of the combinations of age (Körösi, 1895: 784-785).

Körösi's claim that no single specified bigenous table, nor even a single specified male table of natality had ever before been produced is corroborated by an overview of what was available at the time in terms of fertility analysis.[14] 'What we possess is only one specified table for the female (Berlin), whilst a second one (Sweden) has been finished quite recently (after the publication of this paper)' (Körösi, 1895: 786-787).

With regard to the data, Körösi explained that the data used were based on the schedules he introduced in 1888 for each legitimate birth. This schedule had to be filled in by the midwife, and had to be presented to the registrar of births at the moment of registration. In turn, the registrar had to transmit this bulletin each week to the municipal office of statistics. Thus, the analysis focused on 71,800 married couples given by the census of 1891, conforming to the single year combinations, and 46,926 children born between 1889 and 1892 classified according to the age of their parents. Körösi discussed the possible age-combinations, about 2000 (= 51 x 40), that is 51 for males aged under 20 to 70 years old, and 40 for females aged under 16 to 50 years old. Knowing thus the number of all age-combinations, Körösi organized the 46,926 births amongst couples of those ages for four years, two before and two after the 1891 Census.

Following the preliminary remarks, the paper contains five other sections: (II) Monogenous natality; (III) Bigenous natality; (IV) Isogens; (V) Further uses and remarks; (VI) Appendix: remarks relating to the adjustment of the rough results. The second section presents two general findings:

> 1. That the summit of legitimate fertility is reached very soon, so that the decline begins, in the case of the male, upwards from 25, and in that of the female, upwards from 18 years.
>
> 2. That the legitimate fertility does not remain at the same level for many years together, but that it declines *immediately* after reaching the highest point (Körösi, 1895: 791-792).

'These results do not agree with the view of physiology', Körösi (1895: 792) remarked, 'according to which the generative power should remain for some time at the same height'. The reason for this, he continues, is that fertility is the result of two distinct forces: the physiological power reflecting the instinct of nature urging towards multiplication, and the wish to have offspring The latter force is associated with the concept of moral restraint, 'which since Malthus has so much occupied economists, demographers, and statesmen, but still without their succeeding in finding a statistical

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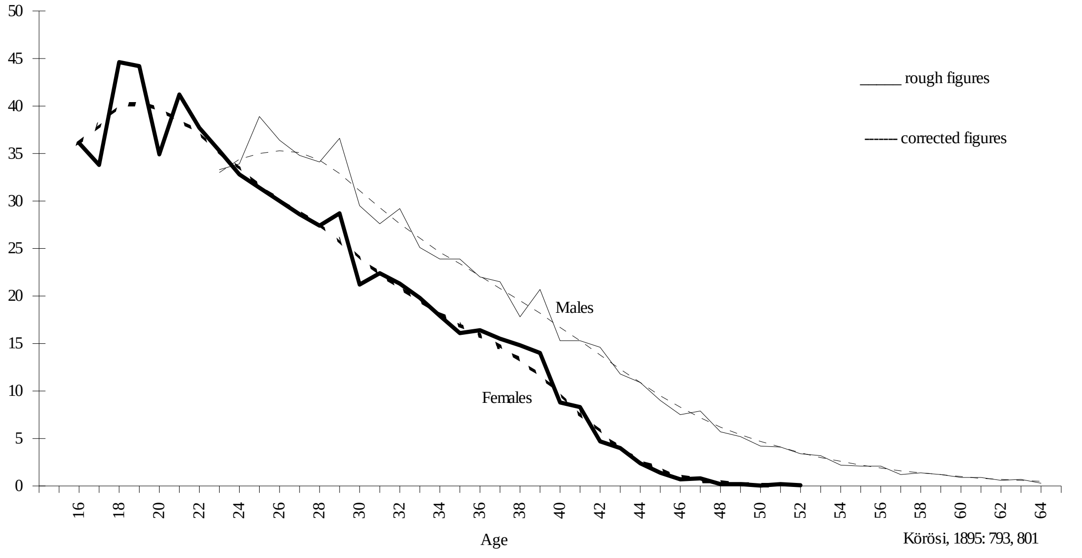
[14] The paper proceeds with some additional and interesting remarks on how to relate births with age combinations of married couples drawn from vital registration and census data; the assumptions that need to be made to minimize the effect of age misreporting in reference to the moment of childbirth and the time of gestation, as well as the age of mother and father at the time of childbirth. Körösi provided a synopsis of the existing data on natal probabilities, including his own computation when the statistical data were not calculated. He started with the respectable statistics of Sweden and then referred to country and regional natality statistics in Finland, Norway, Netherlands, Alsace and Lorraine, Brunswish, Scotland, Berlin, Paris . The review concludes that there existed

> not only no complete - that is, no specified - bigenous table of natality, but even no specified compilation of the paternal monogenous probabilities ... So far as I know, the Budapest table of natality is thus the first complete - that is, specified and bigenous - one (Körösi, 1895: 787-789).

evaluation of this moment' (Körösi, 1895: 808). Figure 1.6.4 illustrates the effect of both factors through the curves of monogenous fertility for females and males.

Figure 1.6.4 Annual natality for 100 wives and husbands, Budapest, 1889-1891



In order to get an insight into the weight of the physiological fertility alone, Körösi looked for couples in whom he expected that the moral restraint should be the weakest or entirely absent. 'To ascertain the isolated effect of the physiological factor', Körösi (1895: 806) wrote,

> we ought to look for such cases where the moral check is nought, or the least possible. No doubt that this check us the strongest where the number of children is greatest, and the weakest where there are no children at all. Thus, we might suppose that it is with the new married couples that the effect of the moral restraint is nearly nought, and where, therefore, the force of the physiological moment could be mostly readily recognized (Körösi, 1895: 806).

In fact, Körösi recognized that the empirical data did not support this supposition, 'not at least the youngest ages'. Despite the fact this could allow the objection against regarding the initial natality of the new-married couples as the right measure of the force of the physiological factor, Körösi argued that 'no other information upon this important point' was available.

From his comparison of the estimated female and male monogenous observations, including both the rough and corrected figures, Körösi concluded that the two curves are very different. On the female monogenous curve he pointed out 'that the legitimate fertility of woman reaches its climax (at Budapest) at the age of 18 and 19 years), and declines above and below this age; further, that it arrives at its null-point at 58'. In turn, on the male monogenous curve, Körösi (1895: 800) remarked 'that the male generative power reaches its climax in married life at the 25th year, that it declines above and below this age, and that it arrives at its null-point at about 70 years' (see Figure 1.6.5).

Still concerning the parallelism and divergence between female and male fertility, Körösi noticed that the former had a higher beginning in the procreative period and earlier finishing.

> In consequence of these conditions - higher beginning and earlier finishing of the female natality - the course of the two curves must be such that they show the greatest divergence in the earliest period of fecundity, the female curve standing there much higher; the two curves subsequently approach more and more, till at a certain point of life they cross one another. After this point of intersection the female curve will be the lower, whilst at the highest ages, where female fecundity has already expired, the weakened male fertility totters lonely towards its term of extinction (Körösi, 1895: 802-803).

In addition, the paper compares the course of both curves showing that the point of intersection occurred at the age of about 45 years of the husband and about 40 of the wife. But, more than speaking of a point Körösi sustained that there was a 'space of intersection, as for a long period of life the natality of the two parents stands so close one to another that it may be regarded as the same' (Körösi, 1895: 803) . And, further, 'for a still longer period of life the male natality reaches the height of the female nearly at the same distance of time, that is about five years later'. However, here Körösi objected

> that this parallelism need not be the necessary effect of a natural law, of later ripening of the male, but it may be caused by the circumstance that fertility is greatest with new married couples, and that the most usual age-distance of wedding couples may be 4-5 years (Körösi, 1895: 804).

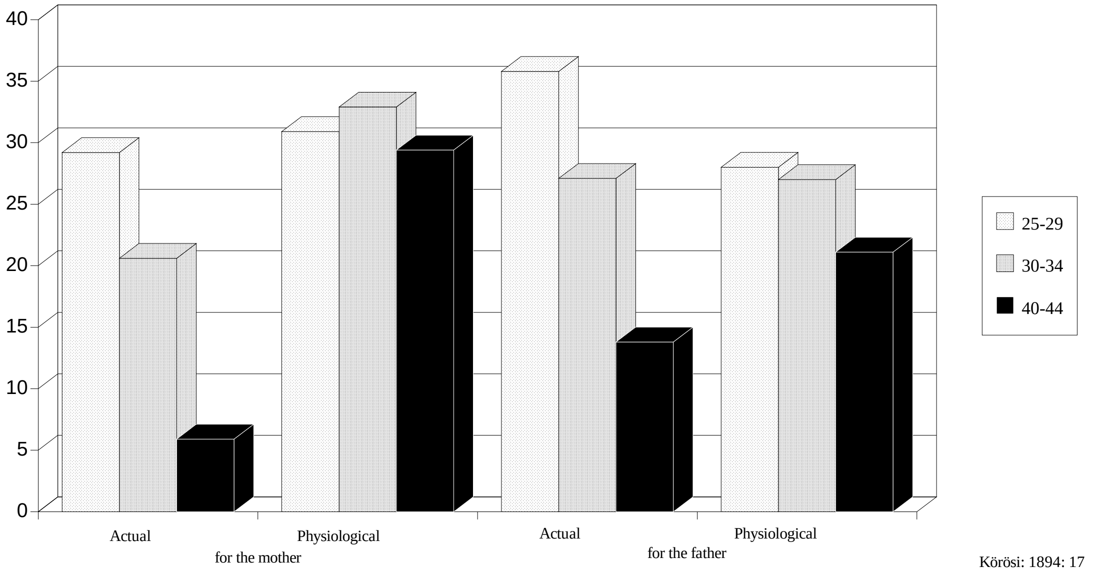
This finding was supported not only by the data for Budapest, but also the only other data available at that time, a bigenous and not specified natality in Norway. Based on these two sources, Körösi concluded that

> the probability is against the supposition of a physiological law, and the fact that at Budapest the males reach the same natality five years later than females may be regarded only as a mechanical effect of the social causes which rule the age combinations of the marrying couples (Körösi, 1895: 806).

The discussion in the paper moves on to the differences between what its author called the 'curves of general (actual) and of initial (physiological) natality'. When these two curves are placed in the same system of co-ordinates two different values appear for each period. 'The difference between these two points ought to give theoretically the measure of the moral restraint', Körösi (1895: 808) explained further. Figure 1.6.5 depicts some figures which Körösi extracted out of the large number of data for an Abstract of his paper published some months earlier.

With the exception of the few cases of 'effects of exhaustion', the difference between the degrees of physiological and that of the actual fertility shows the influence of the moral factor. In the somewhat advanced ages this moral restraint exercises an influence exceeding all expectation. With mothers of 30 to 34 this moral restraint reduces the fertility from 100 to 78 per cent; with those between 35 and 40 years to 45 percent, that is the depression amounted to more than half However, at ages over 40 years, 'the natural fertility of wives is almost suppressed by these factors' (Körösi, 1895: 809). Likewise, 'The moral restraint exercises its influences also on natality of the fathers, though the effect of it is weaker than in the case of mothers', Körösi concluded about the monogenous curves of fertility.

Figure 1.6.5 Difference between physiological and actual fertility, Budapest, 1889-1891



Körösi: 1894: 17

Even more significant for this thesis is the third section of Körösi's work entitled 'bigenous natality'. This section provides a genuine attempt to deal with the interaction between the sexes in empirical but systematic manner. The author's reasoning here is remarkable, for its conceptualization and methodology; it is striking that such a paper has escaped completely the attention of the authors who have dealt, in the past half century, with the so-called 'two-sex problem' and, more general, two-sex models.[15]

> Passing to the enquiry into the natal probability according to the age combination of both parents, we find ourselves face to face with such an overwhelming quantity of facts, that it is impossible to deal with them one by one. Up to this point we had to deal with the course of two curves only, one of the mothers and the other of the fathers. But each single element of these curves is really an average of most different probabilities according to the various ages of the other parent.
>
> If we resolve these average natalities into as many elements as the years of age of the other parent, we ought to divide each figure of female monogenous natality into about 40 constitutive parts (according to the about 40 years included in the period of male fertility), and each figure of the male monogenous natality into about 35 parts (according to the age of the mothers). Thus, instead of the two monogenous curves, we should arrive at about 75 bigenous curves (Körösi, 1895: 809).

The paper then presents Table 1 throughout a total of 25 pages with the primary data on the number of families living in the productive period of life observed in the four years under consideration, and the number of children during the same time. Two additional tables summarize the data and show the rough and the corrected probabilities

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[15] There are at least two exceptions to the neglect of Körösi's paper. Knibbs (1917: 233), to whom I return in the next chapter; Karmel (1984c: 65) also mentioned in a footnote Körösi's paper and mentioned some other works following its approach on the 'bigenous fertility'.

of birth for that part of the age combinations which offered sufficient cases for the calculating of serviceable probabilities.

Moreover, because the possible age combinations lead to 75 bigenous curves, Körösi realized that to interpret each of these curves in the same manner as he did with the two monogenous curves, would form a task too heavy and too tiring both for the author and for the reader'. Besides, he regarded his own observations comprising the births of half a million of inhabitants great enough to show the practicability of his method, but too small to furnish available results for the rarer age combinations.

On this account he restricted himself to pointing out only some of the more remarkable phases in the curves of the bigenous curves. In addition to this Körösi provided a coloured graphical representation, or as he called it, a 'Tabellogram' with a synoptical view of the development and of the mutual interlacement of the 75 curves.[16] The graphical representation contains the annual probability of births for marriages by single years and also five-year groups. Table 1.6.1 reproduces only the quinquennial part of the tabellogram, which depicts the cumulated age of the fathers in combination with the maternal age.[17]

According to which sex is regarded as the determining one and which is regarded as the determined, Körösi explained, two different systems of curves are obtained. That is, according as we wish to learn which change the advancing paternal age produces on the fertility of a given maternal age or *vice versa*, two main systems of curves can be created. One system represent the change of the fertility produced in a given maternal age by the advancement of the paternal age. The other system of curves refers to the change of paternal fertility according to the change of maternal age.

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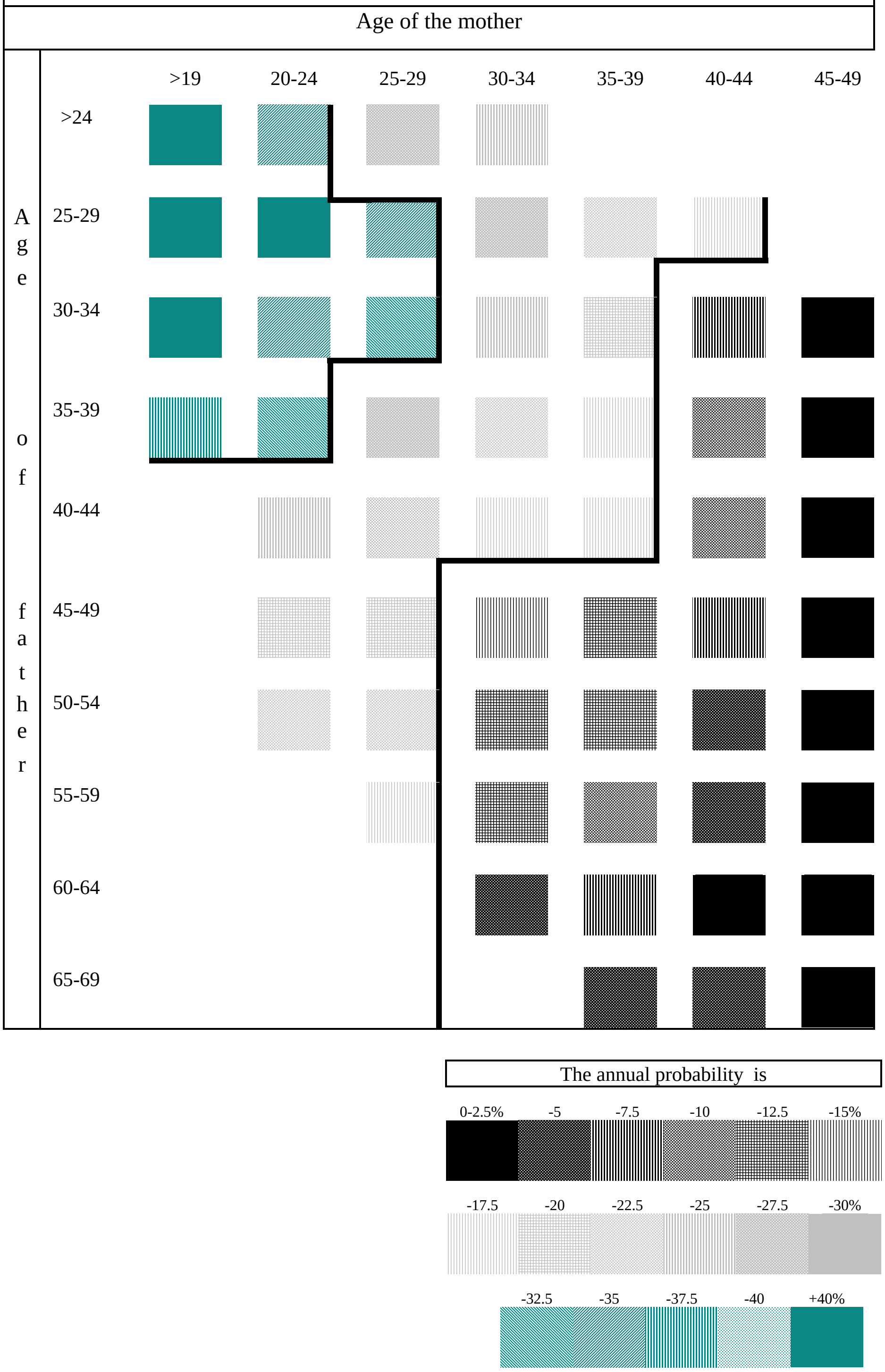
[16] The usual graphical representations in demography give in one of the axes the magnitude of the phenomenon to be observed, for instance the single years of age, and in the second dimension of the drawing, the degree of the scale. In the case of the 'tabellogram' both dimensions are needed: the horizontal axis represents, say, the years of age of the mothers, and the vertical that of the fathers. In order to make sensible the varying level of natality, a third axis is required and thus rather than a single plane a three-dimensional space should be created (Galton, 1894: 18-23; Körösi, 1895: 843-845).

[17] The probabilities described in the paper are based on the rough figures, for reasons explained by the author in an Appendix showing the details of an attempt to adjust the bigenous curves. This exercise was worked out by Blaschke, a mathematician and docent at the Vienna University who occupied himself especially with the question of curve smoothing (Körösi, 1895: 870-875).

Table 1.6.1 Bigenous table of natality, quinquennial recapitulation, Budapest, 1889-1891.[18]



Körösi (1895)

[18] In the original the first group of squares here coloured dark grey is coloured red; the second group here coloured light grey is also coloured red. The third of the squares is coloured black as in the original. The small table with the range of scales refers in the original to the single year table, but it is useful here to allow for the purpose of understanding the meaning of the quinquennial table.

Körösi identified three main groups in his data and separated them with lines of contour. The first group shows that the males under 30 years reach their relative summit of natality, the highest degree which is accessible to their age, with wives under 20 years; those between 35 and 45 ought to choose wives between 20 and 25, and those of the age of 45-50 years, wives between 25 and 30. However, if instead of the highest relative natality one looks for the highest absolute natality, with the exception of the lowest ages this is reached when both parents are young, that is, when women under 25 years are married to husbands under 35. For these cases the natality does not sink below about 35 per cent, but frequently surpasses 40 per cent. If one of the parents is five years older, the natality declines to 32-37 per cent.

The second group of natality coloured light grey lies between the two limits, presenting the stratum of middle natality, between 15 and 27 per cent. The third group, here coloured in black, refers to the end of the reproductive period and contains the weakest fertility levels. If by weak fertility is meant lower than 15 per cent, so Körösi explained, nearly all the families where the mother is above 40 years belong to this class; or yet, also where the mother is between 30 and 40, but the husband is above 45. From the analysis of the difference between the absolute and relative maximum of bigenous natality Körösi deduced that

> in order to secure the possible greatest fecundity, *females ought to select in their younger years older husbands, and in advanced ages younger ones, but that the males ought to select always younger wives;* further, that the husband may be even seventeen years older than the wife, but the age of the latter can surpass the age of the husband only by five years, and also that only if she is above 30 years (Körösi, 1895: 837).

On the best age distance and on the best marriage age, the analysis of the data led its author to conclude 'that the best chances of prolification are offered by the couples where the woman is of 18-20 years of age and the husband of 24-26 years’ (Körösi, 1895: 841).

The fourth section is dedicated to the study of 'Isogens', that is the two-sex age combinations showing the same levels of fertility: the use and the construction of isogens, isogens and age combinations, isogens and age distances, returning isogens points, and crossing points of the isogens. This section was inspired by Francis Galton's (1894: 18-23) paper explaining the application of methods of contours or isogens to the natality table of Körösi. The last section, before the Appendix, provides a review on fertility concepts and measures. It is important to recall that Körösi set his investigation around the goal to establish a model of natality table comparable to the one existing for mortality. 'The probabilities of death were soon utilised for practical purposes, that is for the establishment of life-insurance', so he wrote at the beginning of the last section.[19] But in

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[19] He continued on this, saying:

> It is not impossible that the probabilities of birth may also lead to a new species of insurance, in order to cover the costs of child-bed or even the education of the child. In this case, as the chances which exist against the birth of a child are in favour of the insurer, the premiums could

his paper, Körösi was even more concerned in clarifying some theoretical points concerning the definition of fertility. Overall, his review of the concepts and measures of fertility covered the comprehensively those that were used in his time, though distinctions made among fecundity, fertility, and natality are not conceptually clear. However, it is at the methodological level that the originality of Körösi's analytical framework remains remarkable.

Perhaps the most important feature is the way Körösi balanced his conceptualization of fertility with the demands of empirical research

> Especially with the legitimate fecundity, we have to provide for the fact that the number of children depends not only on the number of husbands or wives at the procreative age, but also on the condition that *both* partners of the couple be still prolific. The birth-rates mentioned before are all monogenous ones; it needs no proof that the bigenous birth-rates furnish a more reliable measure of legitimate fecundity (Körösi, 1895: 866).

Despite this remark on an ideal two-sex measure of fertility, Körösi (1895: 866) immediately acknowledged

> that the only right measure of fecundity is obtained by investigating not the fecundity of the whole productive population, but for a single age element of it. We arrive then at those statistical values which we have treated in the present paper, and which we named Natalities, attributing to this term the same quality of probability as in the life-table to the term Mortality. When these probabilities referred to single years of age, we called them Specified, if to groups of age, Cumulative (Körösi, 1895: 866).

The expression 'the only right measure of fecundity' may be interpreted as referring to what here I am calling 'fertility output' relating births with the female-sex, or as Körösi put it, the female monogenous population. Over the twentieth century the conceptualization of fertility measures has followed this direction. This may explain the neglect of the bigenous or two-sex approach sketched followed the basic principle, as Udry (1994: 562) put it recently, never to invent a theory, or in this case a measure, when a good one exists. However, this explanation does not fully dismiss the potential of Körösi's bigenous approach for purposes in which the interaction between the sexes should not be overlooked. Körösi finished his paper with some remarks on 'measures of the richness of marriages (expectation of children), but about this his remarks were more in line with the one-sex approaches on marriage widely used today in demography. He certainly did not attempt the sort of reasoning that two decades later led Knibbs to conceive the notion of 'conjugal potential' or 'marriage function'.

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be lower than those for a child already existing. If the probabilities of birth should once be used also as nett premium tables, as was the case with the probabilities of death, in order to make such an institution possible, one ought to provide against this insurance becoming a premium on procreation. The insured sum ought to be therefore in proportion to the revenue of the family, and the question might arise whether it would be advisable not to begin the insurance before the second or third child (Körösi, 1895: 864).

7.

Knibbs: a pilgrim of a new world in demographic theory

> An ideal theory of population is one which would enable the statistician not only to determine definitely the influences thereupon of the various elements of human development, and of the phenomena of Nature, but also to examine all facts of interest to mankind, as they stand in relation to population. And however hopeless may be the expectations of establishing such a theory with meticulous precision and all detail, it nevertheless remains true that fluctuations of population can often be adequately understood only when they are analysed by means of definite mathematical conceptions (Knibbs, 1917: 3).

The cradle of the two-sex demography: why Australia?

In conventional demographic analyses most of the role played by the principle of complementarity between the sexes is generally taken for granted, or even deliberately ignored. This is not because demographers are unaware of complementarity in demographic phenomena; a great deal of demography stands on principles which are exactly the opposite of complementarity. For some purposes, for instance, a neuter approach which abstracts even from the standard variables age and sex may be enough; for others there is a need to strip off a layer of appearances which are misleading and which impede the study of demographic reality. As Keyfitz pointed out, in a paper published in 1980, there are several cases in which one can easily draw 'a wrong conclusion from exact statistical data and even when they are known to be quite accurate' (Keyfitz, 1980: 48). A large array of concepts, measures and models which have led to the one-sex demography have been developed with the objective of digging into the depths of demographic relations which strongly influence what appears on the surface. 'Indeed', Keyfitz (1980: 63) remarked further, 'perhaps the biggest difference between professional demographers and others who deal with population is that the professionals know just enough to realise that the surface phenomena are influenced by these deeper ones'.

This explains, at least in part, why demographers of the twentieth century have been much more concerned with the separation than the complementarity between the sexes. However, the separation of the sexes and the method of controlling or stripping off the effect of, say, population structure, have been very effective for some purposes but not for others. Some aspects of the deeper layers of demographic phenomena need to be studied through methods which combine and integrate the role of both sexes. Demographers are aware of this, including those who have simply attempted to adapt mechanically their one-sex methods to the demands imposed by phenomena which can only be understood through the mechanisms of complementarity between the sexes; because they have tried to modify the one-sex methods in an *ad hoc* manner, in general their theoretical frameworks have become creaking and ugly edifices.

In the context of the alternatives between one-sex and two-sex approaches the sex ratio plays a sort of arbitration role that has proved to be paramount for the consistency of any demographic model. Although, if not because, the sex ratio is two-sex by its nature and the most simple composition measure available in demography, during the twentieth century it has already been much more used in one-sex than two-sex demographic approaches.

Historically, the use of the sex ratio in the twentieth-century demography can be traced to two important directions, both sketched and made public in the 1910s. These two directions were developed independently of one another, and while one applied the sex ratio in the construction of a one-sex approach the other applied it in the construction of a two-sex approach. The former emerged in Europe and the United States, when Böckh created the net reproduction rate and more fully when Lotka and his co-authors developed the mathematical model of classical stable population theory (see Part II).

On the other hand, in Australia Knibbs seems to have been totally busy for a great part of the 1910s with his massive work, *The Mathematical Theory of Population, of Its Character and Fluctuations and of the Factors which Influence Them.* This work, first published in 1917, was written as an Appendix to the 1911 Australian Census, though its sophistication and depth surpassed any expectation for an appendix, to the extent that even contemporary authors still refer to it as a 'highly unusual document' (Gray, 1988: 5).

The reason Knibbs's *Mathematical Theory of Population* has been considered an unusual document is because of its comprehensiveness and, perhaps, even more relevant, because of its emergence in parallel and independently of Lotka's stable population theory.

> *The Mathematical Theory* was a highly unusual document. It was certainly ahead of its time, as Wilson contends, but this was in part because it was hardly possible to attempt an undertaking of its type in the second decade of the twentieth century, before the development of stable population theory. The theoretical emphasis of the work is the search for immutable mathematical laws which describe the components of population structure and growth, laws which ultimately could not be justified. On the other hand, the book contains a large number of ideas for statistical methods and

> measures, especially in fertility and mortality. Some of these ideas have become standard methods of demographic analysis in the second half of the twentieth century, one suspects in most cases after rediscovery. Other ideas remain to be recycled into use (Gray, 1988: 5-6). [1]

Also unusual is the slight attention given to Knibbs's *Mathematical Theory of Population* in conventional demography, even in situations where this work should be a compulsory reference. For instance, the huge and comprehensive ‘inventory and appraisal’ edited by Hauser and Duncan in 1959 made no single reference to Knibbs’s *Mathematical Theory of Population*; even Lorimer (1959), in his otherwise very interesting overview of the development of demography, found no reason to mention Knibbs’s original demographic work. This neglect has not been overcome in the last three decades or so; for this reason, statements such as the following from Caldwell are not just very rare but probably dismissed as exaggeration: ‘The modern attempt to examine global population and rates of change originates in Australia with George Knibbs’s (1917) remarkable Appendix to the 1911 Census’ (Caldwell, 1985a: 23).

If Caldwell's remark about Knibbs's contribution to modern demography is not an exaggeration, it is reason to wonder: 'How can it be explained that a central figure in twentieth century demography has been so excluded from contemporary reviews of the history of modern demography?'. Two cases are particularly striking, one related to fertility and the other to nuptiality.

There is a reason why I have singled out the name of Lorimer from many other authors who have dealt with the history of modern demography. Although in his 1959 review Lorimer failed to acknowledge the originality of Knibbs's demographic work, he provided an interesting distinction as to the tradition of fertility conceptualization in Europe and United States:

> Fertility is traditionally conceptualized in Europe as ‘fertility of marriages’ and in the United States as ‘fertility of persons’ (by sex and age) - marital status being treated merely as one of the conditions influencing reproductive behavior. This difference is probably due in large part to differences in types of available data. It may also be due in part to the influence of scientists with biological orientation, notably Pearl and Lotka, on American demography in the 1920’s. But the difference in approach also reflects differences in real situations ... In any case, European demographers have tended to place greater emphasis than their American colleagues on the differentiation between the formation of conjugal unions and nuptial fertility as major components in total fertility (Lorimer, 1959: 143).

Knibbs’s conceptualization of fertility could not be accommodated in Lorimer's characterization of the existing traditions, not because Knibbs was neither European nor American, but because his *Mathematical Theory of Population* conceptualized fertility as

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[1] Gray wrote this paper to question Wilson’s claim in 1986 that Knibbs was not the real author of the work *The Mathematical Theory of Population*. In an address to mark the fiftieth anniversary of his appointment as Commonwealth Statistician, Sir Roland Wilson referred to the first two Commonwealth Statisticians Sir George Knibbs and Mr Charles Henry Wickens and maintained that the latter was the one who wrote that book (Gray, 1988).

‘fertility of marriages’ as much ‘fertility of persons’. On these grounds, Knibbs's approach was at odds with tradition and, perhaps, too much ahead of its time to even deserve a reference.

But the neglect of Knibbs's work does not stop here. Another field in which Knibbs has generally been missed out is in the so-called 'two-sex problem' and, in particular, the consideration of nuptiality from a two-sex point of view. In conventional literature, the formal treatment of the 'problem of the sexes' is generally traced to the work of the French demographer Vincent (1946), and that of the two Australians, Karmel (1947, 1948a, b, c) and Pollard (1948). A few notorious examples where Knibbs's treatment of the problem of the sexes has been completely ignored are the works of Pollard (1973), Pollak (1990), Schoen (1988), and Smith and Keyfitz (1977).[2]

With regard to the conceptualization of fertility, Knibbs's *Mathematical Theory of Population* has been ignored perhaps because it was on the margin of the two main traditions of conceptualizing demographic reproduction; a similar explanation can be found for the nuptiality and the 'two-sex problem'. Nuptiality has been conceptualized during the twentieth century in association with the female population and only seldom in terms of combination and interaction of both sexes. Moreover, with regard to the interaction between the sexes Knibbs saw the subject not just as a formal or mathematical issue but as an empirical problem treated on an equal footing. In contrast, in the 'two-sex problem' the subject has been treated predominantly as a mathematical problem and as a reaction to the one-sex nature of Lotka's stable population theory; in this case, the empirical has been used almost exclusively as an illustration and subsidiary of the formal models, but not a matter valid on its own.

In 1947 and 1948, Karmel and Pollard proposed the first two mathematical models intended to reconcile the male and female net reproduction rates in stable and non-stable populations. Ever since, the aspiration to replace the one-sex nature of stable population theory has provided motivation for unprecedented growth of the research on the two-sex problem in formal demography. However, the interaction between the sexes cannot be ascribed to the stable population theory, nor even to a mathematical difficulty only. This has been demonstrated 1980s and 1990s by some of the treatment of the interaction between the sexes from a two-sex perspective in broader terms (Schoen, 1988, 1993; Pollard and Höhn, 1994). The question of the sexes related with stable population theory is just one part of the broad scope of the relevance of a two-sex approach in studies of fertility and nuptiality. This is corroborated, for instance, by Schoen's (1988: 121) definition of the 'two-sex problem' as ‘... the inability of conventional population models to capture the

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[2] Feeney has been one of the few authors, if not the only one, among those interested in the ‘two-sex problem’ to acknowledge Knibbs's authorship of the concept of ‘marriage function’. Feeney did this in his 1972 Ph.D Dissertation ‘Marriage rates and population growth: the two-sex problem in demography’, though he only paid attention to Knibbs’s definition of the concept of 'marriage function' on page 214 of *The Mathematical Theory of Population*. Feeney’s main goal in his dissertation was ‘to systematically explore the structure of the class of all mathematical functions which may express the dependence of numbers of marriages in a population on the numbers of males and females available for marriage’ (Feeney, 1972: 15).

changes in nuptiality and fertility rates that are produced by changes in population composition', even though Schoen still remained prisoner of the misconception which traces the origin of two-sex demographic research to the work of Karmel (1948c) and A. Pollard (1948) only.

This chapter is particularly concerned with creating the basis to relieve the debate on the interaction between the sexes, an important aspect of the principle of demographic complementarity, from the two misconceptions identified above. There are two advantages in doing this while reviewing Knibbs's work. On the one hand, this review is intended to challenge the neglect of Knibbs's work in the development of demographic theory during the twentieth century. On the other hand, the ‘fertility of persons’ and ‘fertility of marriages’ become parts of the issue from a two-sex point of view and Knibbs’s book illustrates this rather convincingly. After all, from my review of the anticipations of the two-sex demography, I have found no other work like Knibbs's *Mathematical Theory of Population* which provides a balanced, though brief, a framework for a two-sex approach in the theoretical, formal and empirical areas of demography.

Knibbs's attempt to bring together the two most important traditions in the conceptualization of fertility, those identified by Lorimer in 1959, lay dormant for about three decades. There is no doubt that Karmel and Pollard were the authors who set the new research agenda in the late 1940s, but they did it more by resuming and placing the problems in investigation in a new context than starting from scratch. Karmel, in his PhD thesis, showed that he was well aware of Knibbs’s and Körösi’s work. The works of Karmel and Pollard, both Australian authors, have led to a new field in formal demography; this seems enough to point to Australia as the cradle of two-sex demography. This does not mean that the contributions of authors from elsewhere are of less importance, but if one can already speak of a certain tradition in the demographic conceptualization of fertility and nuptiality from a two-sex point of view, Australia has certainly been the source of the first and most important initiatives.

On the question, 'why Australia?', perhaps Lorimer's own classification provides a plausible explanation. In a way, while Australian demographers seem to have often hesitated between the two main demographic traditions developed in Europe and United States, Knibbs can be credited as the pioneer of truly new world in demographic theory. Quételet had perhaps envisaged it, and Körösi made an original and elegant research in term of a one-sex and two-sex approaches of fertility. But Knibbs treated the subject in a comprehensive way, linking the theoretical and formal, both mathematical and geometrical, as well as the empirical. Moreover, the fact that later the precedent created by Knibbs was followed by another two Australians may reflect differences in real situations from those that Lorimer found in Europe and United States. After all, the twentieth century European and American demographers have grown within their own strong traditions. The Australian demographers, in turn, had to approach both traditions from elsewhere, and certainly they found a way to claim a tradition of their own which had never been explored before.

I will review the eve and background of two-sex demography by placing attention on Knibbs's *Mathematical Theory of Population* and other works of his. The objective of this chapter is to demonstrate that it is possible to speak of a two-sex approach tradition set by the attempts to conceptualize fertility not just as fertility of marriages or fertility of persons in disregard of the methodological approach behind them. As Knibbs indicated, the one-sex approach used to deal with fertility and nuptiality needs to be placed in the wider context provided by a two-sex approach.

Population in the aggregate: sex ratio, multiple births and human reproduction

Knibbs's *Mathematical Theory of Population* comprises a total of 466 pages divided into eighteen chapters. The first eight chapters introduce several issues on the theory of population, such as: the nature of demographic problems; the necessity for the mathematical expression of the conditions of demographic problems; various types of population fluctuations; group values, their adjustment and analysis; ways of summation and integration for statistical aggregates; the place of graphics and smoothing in the analysis of population-statistics; and conspectus of population-characters (Knibbs, 1917: 1-107). Chapter 9 focuses on population as an aggregate, including its distribution by sex and age, while Chapter 10 discusses the 'masculinity of population'.

Today Knibbs would most probably not dare to use the terms masculinity and femininity in the dispassionate and technical fashion he did; but in his time demographers could not envisage that some decades later such concepts would be considered guilty of androcentric stereotypes. Without using the term 'sex ratio', Knibbs discussed its content through its two well known surrogates, 'masculinity' and 'femininity', and covered the following issues: the norms and the various definitions of masculinity and femininity; the use of norms for persons and masculinity only; the relation between masculinity at birth and general masculinity of population; masculinity of still and live nuptial and ex-nuptial births; coefficients of ex-nuptial and still-birth masculinity; masculinity of first-born; masculinity of populations according to age, and its secular fluctuations; and theories of masculinity (Knibbs, 1917: 130-141).[3]

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[3] Following his finding that masculinity of still-births was considerably higher than that of live-births, and that masculinity at birth was about 1.05 or 1.06, Knibbs remarked about the various attempts to explain the masculinity at birth:

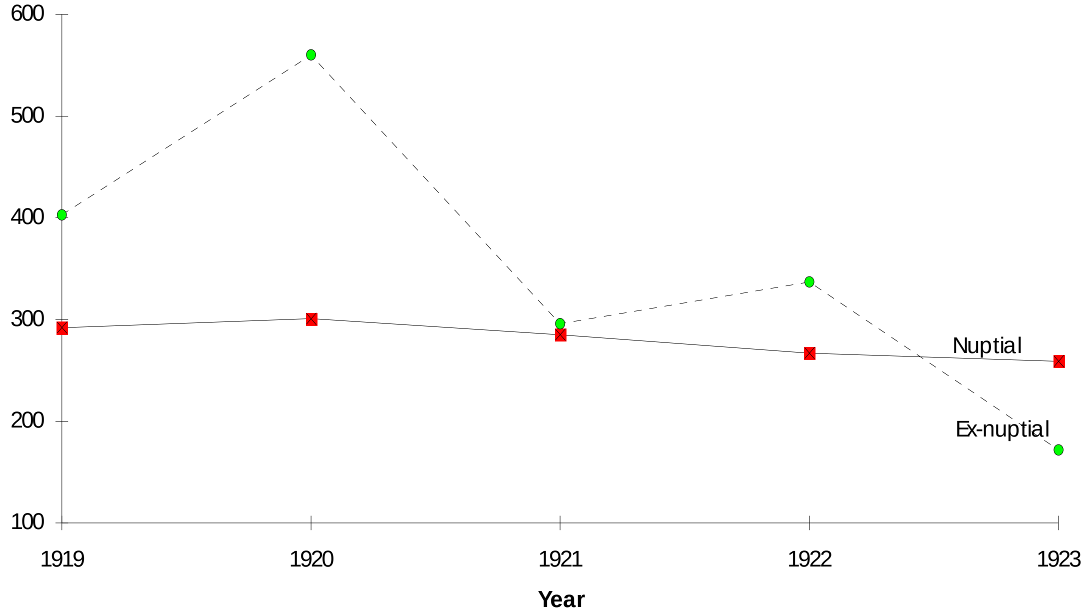
> J. A. Thomson is his 'Heredity' says that, according to Blumenbach, Drelincourt in the 18th century brought together 262 groundless hypotheses as to the determination of sex, and that Blumenbach regarded Drelincourt's theory as being the 263rd. Blumenbach postulated a 'Bildungstrieb' (formative impulse), but this was regarded as equally groundless. It has been suggested that war, cholera, epidemics, famine, etc., are followed by increase in the masculinity. These will have to form the subject of later investigations. At present it would seem that the first necessity is a sufficiently large accumulation of accurate statistics, as a basis for study. The one point which is clear is that death *in utero* (at least in the later stages) is marked by much greater masculinity than

Knibbs returned to the significance of the sex ratio later, in two papers published in 1925:

> The phenomena of the sex-ratios of various forms of life are of the first order of importance, and among them, those which throw light upon the make-up of human population are of special interest (Knibbs, 1925a: 212).

Contrary to 'our predecessors', as Westergaard (1932: 72) put it, Knibbs was not just struck by the regularity of the sex ratio, and paid equal attention to its deviations. ' "Masculinity" may be expressed ...', so Knibbs defined his most used surrogate of the sex ratio, 'by the difference between males and females divided by their sum; that is (M-F)/(M+F)' (Knibbs, 1925a: 213). Figure 1.7.1 depicts graphically the data provided in Knibbs's 1925 articles: 'Per 10,000 nuptial births and per 10,000 ex-nuptial births in Australia from 1919 to 1923 the masculinities were respectively as follow' (Knibbs, 1925a: 213).

Figure 1.7.1 Australian masculinities at bith, 1919-1923



Source: Knibbs, 1925a: 213

'The irregularity of the ex-nuptial case is very striking', Knibbs added about the data and called attention to the 'mean square deviation' in the final column of his table: 15.6 for nuptial and 127.8 for ex-nuptial. Furthermore, he compared the ratio of males to females for still and live-births in several European countries and found that the former was about 1.305, the latter was about 1.070, and the ratio between the two was 1.220. These

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that which characterises live-births. This will be referred to later in dealing with infantile mortality (Knibbs, 1917: 140-141).

findings confirmed those Knibbs had presented in the *Mathematical Theory of Population* for the Western Australian population, and this led him to conclude:

> That this sex-ratio, males to females, is invariably greater for still than for live-births indicates that male lives are in greater jeopardy prior to birth than are female lives (Knibbs, 1925a: 214).

With regard to the effect of multiple births on masculinity Knibbs wrote in the same article:

> I have shown elsewhere that ratio of males to females is reduced by multiple births ... Inasmuch as everywhere the numbers of multiple births are relatively very small (for example, in Australia per ten million confinements there are only 98,020 twins, 829 triples, 15 quadruplets, and perhaps 2 quintuplets, or, roughly, 1 in a hundred confinements for twins, and about 1 in 10,000 for triples), it is evident that these can affect the general masculinity but very slightly ... For this reason the variation of masculinity with size of family, which will be shown hereinafter to occur, must be regarded as a fundamental fact in the phenomena of human reproduction, just as much so as the production of fertile male ova exceeds in number that of fertile female ova, and not regarded merely as a consequence of multiple births (Knibbs, 1925a: 215-216).

By drawing attention to specific empirical data from different sources and some possible mathematical equations Knibbs sought to outline a general law of diminution of masculinity with increase of family . He found no simple curve which could represent exactly the results, but on the grounds of the empirical evidence concluded that: first, on the average large families tended to have more females than small families; this result seemed to be more defined for families of 1 to 6, after which possibly the masculinity was less well marked. Secondly, multiple births markedly confirmed previous findings, but because they were relatively small in numbers, they quantitatively affected the general result but slightly. Thirdly, a more extensive study was needed and should embrace separately the living issue, the deceased issue, and both combined; as yet, he found it important to compute the results from male-parent records and female-parent records separately, as well as in combination. Fourth, Knibbs conjectured that a definite law could be expected to appear only when very large numbers of cases were studied. So, the possibility of a secular change with time and any improved knowledge of the phenomena of sex ratios in human reproduction depended on a systematic study carried out on more extensive scale.

The review so far has highlighted Knibbs’s applications of the sex ratio as a measure of matters of fact. In later papers he returned to the topic of masculinity of first births (1927a), multiple births (1927b) and a 'proof of the laws of twin-births' (Knibbs, 1927c). The next section stresses Knibbs's use of the sex ratio as an algorithm process or explanatory resource in sketching his theory of nuptiality and fertility.

From natality to fertility through nuptiality

Before turning to the core of Knibbs's theory of the probability of marriages according to pairs of ages it is useful to give some attention to he conceptualized demographic analysis in general. Following some broad considerations on population in the aggregate, Chapter 11 of *The Mathematical Theory of Population* concentrates on 'Natality'.

> The phenomena of human reproduction, as affecting population, and the whole system of relations involved therein, may be subsumed under the term 'natality'. In one aspect they measure the reproductive effort of a population; in another they disclose the rate at which losses by death are made good; in a third they focus attention upon social phenomena of high importance (*e.g.*, nuptial and ex-nuptial natality); in yet another they bring to light the *mode* of the reproductive effort (*e.g.*, the varying of fecundity with age, the fluctuation of the frequency of multiple-birth, etc.) (Knibbs, 1917: 142).

Following this broad definition of the scope of natality, Knibbs detailed each of the three features associated with natality in three separated sections. First, the study of birth-rates as part of natality in its narrow sense and in association with the Malthusian law concerning the arithmetical increase of food production as opposed to the geometrical increase of population(Chapter 11); secondly, the role of 'Nuptiality' (Chapter 12); and only then, in third place, do two chapters focus on fertility strictly speaking: Chapters 13 on 'Fertility and fecundity and reproductive efficiency' and Chapter 14 on 'Complex elements of fertility and fecundity'.

Clearly, this conceptualization of demographic analysis and, especially the way fertility is placed in Knibbs's conceptualization of demographic reproduction, contrasts with most contemporary textbooks in demographic teaching. In particular, it contrasts with the view that nuptiality is not in itself of particular interest to demographers. According to Newell,

> Marriage, separation, divorce, widowhood and remarriage, collectively called 'nuptiality' in demography, are not in themselves of particular interest to demographers. Rather, their importance arises partly from their relationship with the age at which sexual relations begin and end, and partly with the formation and dissolution of families and households (Newell, 1988: 90).

This statement is completely at odds with Knibbs's position concerning the role of nuptiality in demographic analysis:

> The phenomena of reproduction have a double aspect, viz., one a sociological and the other a physiological. Thus, from the standpoint of a theory of population, both are important. The women of reproductive age in any community furnish the potential element of reproduction; but the resolution into fact depends also upon social facts as well as upon physiological; for example, the relative proportion of married and single, i.e., the nuptial-ratio, even more profoundly affect the result than physiological variations of fecundity (Knibbs, 1917: 175).

Knibbs started by conceiving natality in the context of the whole demographic system and in the standard and neuter perspective used in demographic teaching nowadays. Yet, contrary to the mainstream approach even before attempting to control and strip off the effect of population structure by focusing, for instance, on one-sex models and measures

Knibbs considered some fundamental mechanisms in the interaction between the sexes. So, contrary to the widely accepted view, as it is depicted by Newell' statement quoted above, Knibbs addressed the demographic reproduction moving from natality to fertility not directly but through nuptiality:

$$\left\langle \begin{matrix} \text{DEMOGRAPHIC} \\ \text{REPRODUCTION} \end{matrix} \right\rangle \rightarrow \langle \text{Natality} \rangle \rightarrow \langle \text{Nuptiality} \rangle \Rightarrow \langle \text{Fertility} \rangle$$

A sketch of a two-sex approach on nuptiality: theoretical, formal and empirical

If the authorship of the last quotation from Knibbs's 1917 work were not known, one could well imagine it to have come from any of the contemporary authors who, in recent years, have admonished demographers to admit that social factors may more profoundly affect population change than physiological factors.

Although Knibbs's sketch of his analytical framework relevant to a two-sex approach is brief, fundamentally it is consistent with the principle of complementarity between the sexes discussed here. Moreover, Knibbs dealt with the interaction between the sexes and the age combination with elegance in its threefold dimension: theoretical, formal and empirical. Theoretically, Knibbs formulated and addressed key issues and concepts relevant to nuptiality analysis. Formally, Knibbs dealt with demographic theory and techniques of population using not just algebra and calculus but also geometrical and graphical representations.[4] Empirically, Knibbs applied population theory and technique especially to the data from the 1911 Australian Census.

Knibbs's *Mathematical Theory of Population* resumed and expanded, in an unprecedented way, the centrality of marriage and couples so cherished by earlier demographers such as Graunt and Malthus. Both in his main work of 1917 and several papers published in the 1920s, Knibbs revealed an explicit interest in the dual nature of demographic reproduction, namely the social and the physiological.

> As at present constituted, the social organism is the theatre of a conflict between controls and traditions (which are generally supposed to be of great social interest and value) and the gonad urges of the individual human organisms. Biological facts, which throw any light upon the features and trends of this conflict, have been at all times of scientific importance. Owing to the advance of knowledge in respect to the functioning of the endocrine and sex glands, and in respect to the technique of the control of their unrestricted play, the analysis of facts which reveal the features and drift of this conflict has become, quite recently, of very special importance. And certain aspects of this are accentuated in significance by existing and threatening

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[4] 'In general', Knibbs wrote about graphs of data and smoothing,

> we are concerned with two kinds of alteration; one may be called the '*redistribution of the data without alteration of their aggregate*;' and the other may be called the '*alteration of data to coincide with what is deemed the most probable result,'* having regard to all the facts (Knibbs, 1917: 85).

> difficulties arising from population-growth. These difficulties are world-wide (Knibbs, 1927a: 73-74).

This statement suggests that demographers' reliance on biological determinism may have been much more recent than it seems at first. Before the scientific discoveries, such as those of Darwin, there was little basis for a population approach based on biological determinism. Many interpretations of demographic phenomena were attributed to mystic or providential interventions. In addition, Knibbs's treatment of the subject of nuptiality before fertility indicates that he gave a privileged place to the role of social reproductive mechanisms in the overall process of demographic reproduction:

> The nuptial-ratio in any community may be regarded as a measure of the social instinct, and also a measure of the reproductive instinct, modified by social traditions as well as facilitated or hindered by economic conditions. This ratio, for the case of females, is, of course, specially important in relation to fecundity (Knibbs, 1917: 175).

The concept of 'nuptial-ratio' corresponds, in current terminology, to the measure of 'general marriage rate' (GMR). Knibbs discussed this measure while he identified the limitations of the crude marriage rate, particular its 'uncertain significance' as a measure, due to the fact that it is insensitive to the lack of homogeneity in populations. 'The heterogeneity', Knibbs (1917: 176) explained, 'arises largely from divergences of social life and tradition, in respect of the relative frequency of marriage, and the frequency according to age'.

Chapter 12, entitled 'Nuptiality', presents a systematic analysis on four major issues: it starts by providing some considerations on the concept of nuptiality and its specific operational definitions; these are followed by a discussion on marital status and composition of population, what Knibbs called 'conjugal constitution of the population'.

> The significance of marriage in respect of reproductive activity depends upon the relative frequency of nuptial and ex-nuptial births, as well as upon the relative proportions of the married and unmarried (Knibbs, 1917: 175).

In other words, it depends not merely upon the nuptial ratio, but also upon nuptial and ex-nuptial fertility, particularly during the reproductive period of life.

Secondly, Knibbs considered the norm of conjugal relations, especially divorce. On the latter issue, Knibbs discussed the secular increase of divorce, the abnormality of the divorce curve, and the desirable form of divorce statistics in order 'to be of high value from the standpoint of sociology' (Knibbs, 1917: 189).

> The frequency of divorce is of sociological interest. The effect of Divorce Act (55 Vict., No. 37) of New South Wales, and of Victoria (53 Vict., No. 1056), which came into force on 6th August, 1892, and 13th May, 1890, respectively, have had a conspicuous influence in increasing its frequency (Knibbs, 1917: 186).

Knibbs added that the sociological value of statistical data required the data to be classified at least according to age *per se*, to difference of age and to duration of marriage. These three aspects should make it possible to 'expose the conditions which are of danger from the standpoint of social stability' (Knibbs, 1917: 189).

Thirdly, Knibbs addressed the question of the interaction between the sexes under the title ‘Frequency of marriages according to pairs of ages’. Finally, he outlined briefly his general theory of protogamic and gamic surfaces. These two last issues deserve to be reviewed at some length here. On the one hand, they have been widely neglected in the demographic literature on nuptiality, including in the debates on the ‘two-sex problem’; on the other hand, they provide a broad and consistent background to Part II on the principles on which conventional demography has stood so far as compared with the aftermath of the complementarity between the sexes.

Nuptiality according to pairs of ages: the first two-sex mathematical model

In the section entitled ‘Frequency of marriages according to pairs of ages’ Knibbs (1917: 189-201) moved to the heart of the complementarity between the sexes. He started by saying that ‘The frequency of marriage according to pairs of ages can be well determined only for a considerable number of instances’ (Knibbs, 1917: 189). After illustrating this point with some examples, Knibbs provided a table for single year groups of number of marriages arranged according to the ages of the contracting parties and based on Australian data for 1907-1914; the data were drawn from the 1911 Australian Census and provided the empirical grounds for a detailed debate on nuptiality in Chapter 12. As part of this debate, Knibbs pointed out the various irregularities in the data, and discussed the errors in the ages at marriage and the ways of correcting such errors. He then considered the ‘Probability of marriage of bride or bridegroom of a given age, to a bridegroom or bride of any (unspecified) age’:

> The correction of the data, as indicated in the preceding section, admits of the construction of a table shewing in say 100,000 marriages the number occurring for bridegrooms of any given ages, and for brides of any given ages, the age of the other partner to the union being unspecified (Knibbs, 1917: 198).

Yet, as Knibbs pointed out a few pages below, grouping the data according to age-groups for single years

> is by no means perfectly satisfactory for the purpose of very accurately determining the frequency of conjugal-groups according to various *differences of age*. It is obvious that when all bridegrooms, whose age was say x last birthday, and brides whose age was say y last birthday (x and y being integers), are grouped, the group contains brides who are one-half year older than the difference x-y, as well as brides one-half year younger than this difference (Knibbs, 1917: 192).

So Knibbs considered the most possible and satisfactory way ‘To properly determine the law of nuptial frequency according to specified differences of age’; after determining the marriage rates for the Australian population in the period 1907-1914 he represented ‘the probability of a marriage occurring in a population of males, females, or persons’ (Knibbs, 1917: 193). Because the number of 300,000 marriages was not sufficient for the determination of adequate data for single years, particularly at the higher ages, Knibbs tabulated the data by 5-year groups (uncorrected data). These data are reproduced in Table 1.7.1.

Table 1.7.1 Number of marriages arranged according to age at marriage in five year groups. Australia, 1907-14

Bride-	Brides' age															Total	Ratio of bri-
groom's age	10-14	15-19!	20-24!	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	70-74	75-79	80-84	10-84	des to total
15-19	9	3,302	1,395	124	17	3	2									4,852	1,608
20-24	44	23,130	56,029	11,302	1,437	325	60	22	4	1						92,354	30,603
25-29	18	10,637	50,597	34,896	6,739	1,369	282	78	20	1	1	1				104,639	34,673
30-34	1	2,795	15,513	17,366	9,130	2,476	525	146	26	4	1					47,983	15,900
35-39	3	917	5,134	7,298	5,672	3,621	1,038	*313*	65	15	2	2				24,080	7,979
40-44	1	237	1576	2,564	2,811	2,473	1,502	510	112	26	8	1				11,821	3,917
45-49	2	115	598	1,077	1,313	1,653	1,279	859	263	74	36	8				7,277	2,411
50-54		41	183	384	538	768	754	675	406	117	37	20	2	1		3,926	1,301
55-59		11	73	129	197	313	360	445	289	218	65	26	4	2		2,132	706
60-64		6	28	71	79	152	162	207	208	144	106	60	16	2	1	1,242	412
65-69		1	15	24	43	66	80	133	122	113	105	97	19	7	1	826	274
70-74			6	16	17	30	50	47	65	41	50	59	28	6		415	138
75-79		1	2	3	8	6	11	13	17	31	14	21	25	8	4	164	54
80-84			2	2	2	2	8	10	7	4	9	4	8	4		62	21
85-90				1			1	4	1	1	1	1	1		1	12	4
Total*	78	41,193	131,151	75,257	28,003	13,257	6,114	3,462	1,605	790	435	300	103	30	7	301,785	100,000
Ratio of Brides to total	26	13,650	43,458	24,937	9,279	4,393	2,026	1,147	532	262	144	99	34	10	2	100,000	0.3313617

* Brides over 85 and bridegrooms over 95, and unspecified cases are omitted.

The bordered numbers denote the maximum on the vertical lines;

The shadowed numbers denote the maximum on the horizontal lines

! The values corrected for mistatement of ages 18, 19, 20 and 21 give the following results: for 3,302 and 1,395, 3502 and 1,481; and for 23,130 and 56,029, 23,172 and 55,701. In the totals 41,193 and 131,151 become 41,435 and 130,909; and 4,852 and 92,354 become 5,138 and 92,068. The ratios 13,650 and 43,459 become 13,730 and 43,378; and 1,608 and 30,602 become 1,703 and 30,508.

'0.3313617' - Factor of reduction to 100,000

Source: Knibbs, 1917: 199

Were these data smoothed, 'they would give the probabilities of a marriage occurring within the year groups of specified ages or specified quinquennia' (Knibbs, 1917: 198). Table 1.7.1 is discussed more fully below.

Knibbs moved on to a more sophisticated mathematical discussion on the 'Frequency of marriage according to age representable by a system of curved lines':

> Frequency according to pairs of ages (bride and bridegroom) can best be represented by a surface, the vertical height of which, above a reference plane, is the frequency for any pair of ages denoted by *x*, *y* co-ordinates. The numbers marrying in any given period, whose ages range between $x-\frac{1}{2}k$ and $x+\frac{1}{2}k$ (for bridegrooms), and between $y-\frac{1}{2}k$ and $y+\frac{1}{2}k$ (for brides), as ordinarily furnished by the data, are denoted by *Z*, the height of the parallelepiped. This frequency may, of course, be expressed as for the *exact age*, or it may be for the *age-groups.* When *k* is not infinitesimally small, the difference between the two is *sensible* and *important*. We shall assume for the present that the frequency varies only with age (*x*) in question, instead of being of various ages between $x-\frac{1}{2}k$ and $x+\frac{1}{2}k$. The age-group

> frequency denotes the frequency with the ages distributed between the limits referred to (Knibbs, 1917: 199).

For most practical purposes, Knibbs continued, the age-group frequency is the most important. Hence, supposing the exact frequency, *z*, for the population *P*, to be $\frac{z}{P} = F(x.y)$ Knibbs proposed the following equation for any group-value:

$$Z = P\iint F(x, y)dxdy\,.$$

To my knowledge this formula was the first two-sex mathematical equation ever proposed in demography in any treatment on the interaction between the sexes. Important aspects of the demographic debate upon marriage, since the late 1940s and from a two-sex point of view, are explicitly touched on in *The Mathematical Theory of Population.* For instance, Schoen's (1988) recent review of the mathematical theory on the interaction between the sexes discussed the concept of 'magnitude of marriage attraction' and the properties of its harmonic mean solution:

> Let us focus on marriage and articulate the analogous two-sex population concept, the *magnitude of marriage attraction,* which reflects the mutual attraction for marriage between males and females independently of the age-sex composition of the population. The magnitude of marriage attraction differs from the force of decrement to marriage because the force only relates to the behaviour of both sexes (Schoen, 1988: 121).

In his theory on nuptiality, Knibbs discussed the frequency of marriages according to age representable by a system of curved lines; he referred to the errors in dealing with group-ranges and in contrast to the central value of the range of ages proposed to compute the 'weighted mean' of the differences of the groups adjoining on either side of an age group.

However, the depth of Knibbs's two-sex approach on nuptiality can be better grasped when placed in the context of what he called the gamic conditions; a subject discussed in the last part of Chapter 12 of the *Mathematical Theory of Population.* It was here that Knibbs set his theory of probability of marriages in age-groups and applied to what he called the 'protogamic surface' (Knibbs, 1917: 214-228) and the 'gamic surface' (Knibbs, 1917: 228-231).

The gamic conditions: 'General theory of protogamic and gamic surfaces'

Following the definition of the equation *Z* Knibbs moved on to focus on the subject from an empirical point of view and searched for appropriate ways to represent the interaction between bridegrooms and brides statistically:

> The ages of husbands being adopted as abscissae, and those of wives as ordinates, the infinitesimal number *dM* in an infinitesimal group of married couples, consisting of husbands, whose ages lie between *x* and *x+dx*, and their wives, whose ages lie between *y* and *y+dy*, will be:
>
> $$dM = Zdxdy = kF(x, y)dxdy$$
>
> Thus $Z = kF(x, y)$ is represented by a co-ordinate vertical to the *xy* plane. Since *Z* denotes an actual number of persons in a double age-group, between say the earliest age of marriage and the end of life, viz., $(x_1$ to $x_2)$ and $(y_1$ to $y_2)$, it is necessary, if we desire to institute comparisons between different populations, that *Z* should be expressed as a *rate*, *z* say: that is, *z*= either *Z/P;* or *Z/M*; that is to say, the vertical height will represent the relative frequency of married couples whose ages are, in the order of husband and wife, *x* and *y*, in either the whole population *P*, or the married portion of it *M*. Thus we shall have
>
> $$(418) \text{ .... } P, \text{ or } M = k\iint F(x, y)dxdy\,.$$
>
> If the value of the double integral be taken for the limits denoting the range of ages of the married, say about 11 to 105, we shall have either *M/P*, or unity, as the result; according as we denote by frequency in reference to the total population or to the total married (Knibbs, 1917: 201-202).

In this way Knibbs set, for the first time, the concept of ‘conjugal potential’, which today is best known as ‘marriage function’ or, more generally, ‘mating function’. The ‘marital or gamic condition of a community’, Knibbs explained,

> is completely specified by the gamic surface *F(x,y,z)*, where the frequency for any pair of ages of bridegrooms and brides is denoted by *x, y* co-ordinates (the ‘gamic meridians), and *z* corresponds to the exact frequency of marrying numbers denoted by *Z* ... The values of *x*, *y*, *z* for the unique mode of the surface may be called the *gamic mode of the ‘population’*, or of *‘married population’*, according as the constant *k*, in (418) above, gives *M/P*, or unity for the value of the double integral between the widest age limits (Knibbs, 1917: 202).

In addition, the ‘gamic characteristics’ of the population are more briefly, though less completely, defined by two factors: (1) the ‘gamic meridians’, that is the two principal meridians defined by the line joining the modes of the curves *x*=*a* constant and *y*=*a* constant and passing through the unique mode, as well as the curve *z*=any constant passing through the unique mode; (2) the position (and magnitude) of the *gamic mode*.

The term ‘gamic’ may only be used currently in social sciences as part of words such as ‘monogamic’, the habit or practice of having only one mate, or poligamic in case more than one mate is involved. Etymologically ‘gamic’ refers to sexual (opposed to ‘agamic’) and comes from the Greek *gamikós*, ‘of or for marriage’ (*Macquarie Dictionary*, 1985: 724). Knibbs identified two types of surfaces: the protogamic surface, referring to the frequency of marriage at particular pairs of ages, and the gamic surface, referring to the frequency of the number of persons of particular pairs of ages living together in the state of marriage.[5] Later in the text Knibbs (1917: 224) explained with regard to conjugal age-

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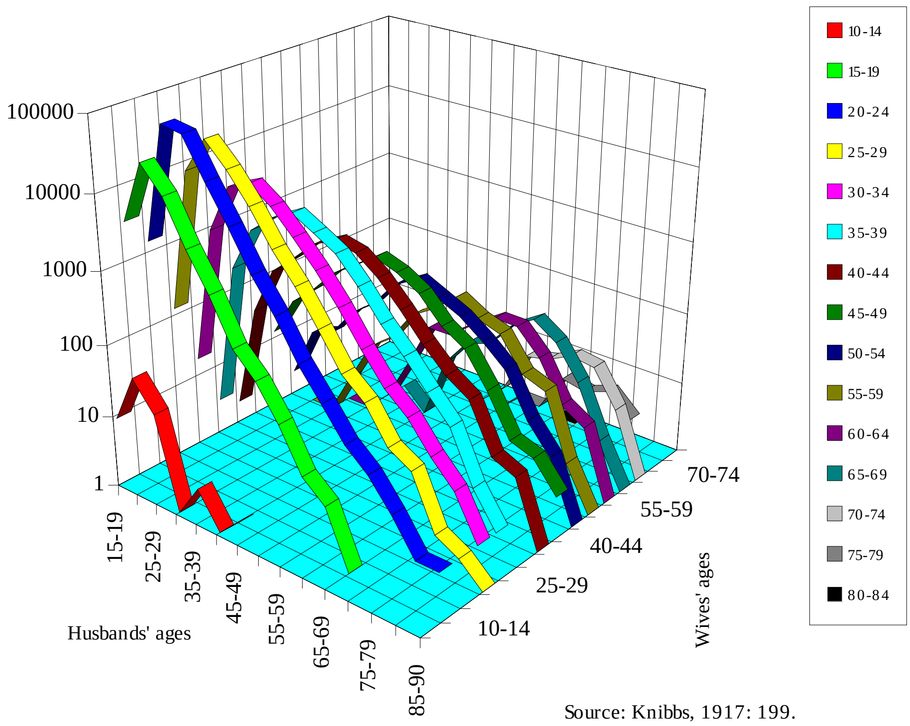
[5] In a footnote, Knibbs explained:
The word ‘isogamy’ has already been appropriated in a different sense in biology, viz., to denote the union of two equal and similar ‘gametes’ in reproduction. This, however, will obviously lead to no confusion. The isogamy of a people might be regarded as of two kinds, *initial* or *nuptial* isogamy (isoprotogamy), and *characteristic* or *marital* isogamy (or simply isogamy) (Knibbs, 1917: 202).

relationships that the protogamic age-relationships may be ascertained from marriage records. In turn, the gamic age-relationships refer to the instantaneous relationships at any moment and are disclosed by a census. Most of the analysis on gamic conditions is focused on the protogamic surface.

Since the characteristics of the protogamic surface are disclosed by the position of the maximum points, Knibbs returned to the data which are depicted in this thesis by Table 1.7.1 and are graphically represented in Figure 1.7.2.

Figure 1.7.2 Number of marriages arranged according to age at marriage, Australia, 1907-1914.



Source: Knibbs, 1917: 199.

As Table 1.7.1, shows the numbers of marriages corresponding to any given age for brides (the columns) show a clearly-defined maximum value; but the corresponding numbers of marriages to any given ages for bridegrooms (the rows) in many cases show two or even three maximum values. In this latter case, too, the maximum is often less clearly defined. Knibbs indicated two ways of estimating the position and frequency at the maximum (or any other point):

> One is to ascertain the position and frequency for the maximum of the frequency integral taken over the range $x - \frac{1}{2}$ to $x + \frac{1}{2}$, or over the range $y - \frac{1}{2}$ to $y + \frac{1}{2}$;

> the other is to determine those elements for the maximum instantaneous frequency ; that is to ascertain the point when the frequency for an indefinitely small range is a maximum (expressed, however, per unit of age-difference, say one year) (Knibbs, 1917: 204).

By applying some formulas introduced in Chapter 7 of the *Mathematical Theory of Population*, Knibbs (1917: 204-211) calculated the position and value of the maximum points, those on the surface for ages of brides constant, those of bridegrooms being variables, or for ages of husbands constant and those of brides variable.[6] The highest point surface derived for the group bridegrooms was about 23.4, and for brides 21.6 years of age; the frequency attaining to about 4,200, or about one seventy-second part (0.013911) of all marriages (Knibbs, 1917: 207).

At this stage Knibbs was hardly satisfied with his results, and admitted their uncertain because of the abnormalities related with misstatement of the age at marriage. In another clear demonstration of his grasp of the difficulties faced when one attempts to deal with aspects of the complementarity between the sexes he remarked:

> It is, of course, much to be regretted that social organisation does not admit of the social-psychological fact of conjugal frequency at equal and disparate ages being accurately ascertained (Knibbs, 1917: 208).

Knibbs did not give up to the subject here, after expressing his regret for adversities of social organization. On the contrary, rather than using this as an excuse to abandon the matter, Knibbs moved on immediately to search for feasible directions aiming to overcome the difficulties he faced. First, Knibbs (1917: 211) admitted that 'For sociologic purposes, a table shewing the relative marriage frequency in various age-groups is of obvious importance'. So, Knibbs took the married and unmarried Australian population by age-groups, from 1907 to 1914, he deduced the relative frequency of marriage for an estimated 1,000,000 marriages as in Table 1.7.2.

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[6] For details of the results that Knibbs computed see Knibbs, 1917: 204-205.

Table 1.7.2 Relative frequency of marriage in various age-groups, Australia, 1907-1914

Bridegroom's Age	Brides' age 10-14	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	70-74	75-79	80-84	85-89	All ages 10-89
15-19	30	11,605	4,920	411	56	12	7	3	2	1							17,048
20-24	146	76,788	184,576	37,452	4,762	1,077	199	73	13	3	1						305,080
25-29	60	35,249	167,668	115,639	22,331	4,537	935	259	66	6	3	2					346,765
30-34	10	9,262	51,407	57,547	30,255	8,205	1,740	484	86	13	7	3					159,019
35-39	7	3039	17,013	24,184	18,795	11,999	3,440	1,037	215	50	13	5					79,797
40-44	5	785	5,222	8,496	9,315	8,195	4,978	1,690	371	86	30	10	1				39,183
45-49	4	381	1,982	3,569	4,351	5,477	4,239	2,827	872	245	80	27	3				24,057
50-54	3	136	607	1,273	1,783	2,499	2,545	2,237	1346	388	166	53	7	3			13,046
55-59	2	43	182	414	686	978	1,293	1,425	1027	697	215	99	17	6	2		7,086
60-64	1	20	93	209	331	457	547	686	689	524	351	199	50	9	3		4,169
65-69	1	7	43	88	143	219	265	365	431	431	315	182	63	13	5		2,571
70-74	1	5	23	40	66	99	146	186	215	215	166	113	73	21	7	1	1,377
75-79	1	3	7	13	20	28	38	48	64	85	92	73	47	27	11	1	558
80-84		1	6	9	10	14	22	28	33	29	23	13	8	4	2	1	202
85-90			1	1	2	3	5	10	8	5	3	2	1	1			42
Total	271	137,324	433,750	249,345	92,906	43,799	20,398	11,358	5,438	2,778	1,465	781	270	84	30	3	1,000,000

Source: Knibbs (1917). *The Mathematical Theory of Population, p. 211*

The totals in the final column of Table 1.7.2, entitled 'All ages 10-89' are about ten times those in the final column of Table 1.7.1. Though in substantial agreement, the totals in the two tables are not absolutely identical because the results in Table 1.7.1 have been slightly smoothed. In the end, based upon the marriages of the 8-year period, 1907 to 1914 inclusive, Knibbs found a middle point of time to be 0 January 1911, while the census was 3 April 1911. The total marriages were 301,922 or about 37,740 annually; half of them had occurred by about April 28, 1911, that is 25 days after the census.

A second solution for the difficulties in dealing with both sexes was more of a theoretical and mathematical nature. Knibbs took the smoothed results of the census just described, the computation of the unmarried at each age, the estimate of ratios of the males to the females (M/F), and the masculinities of the various age-groups, which were required 'hereafter for the computation of the probability of marriage according to pairs of ages' (Knibbs, 1917: 212).

Knibbs's 'theory of the probability of marriages in age-groups'

Regardless of the lack of data for a definite and rigorous determination of the probability of marriage in age-groups, Knibbs commented: 'a fairly accurate estimate is possible by means of a somewhat empirical theory' (Knibbs, 1917: 214).

> Suppose that in any age-group there are M unmarried males and F unmarried females; and that in a unity of time N pairs of these marry. The probability *with F females in the group*, of a particular marriage occurring among the M males is obviously N/M; and *with M males in the group*, the probability of a particular marriage occurring among the F females is similarly N/F. Such a statement of probability, however, lacks generality. To obtain a more general one, an expression is needed which, given a definitive tendency towards the conjugal state in males and in females, though not

necessarily of the same strength (or potential) in each sex, and not necessarily independent of the relative numbers of the sexes, nor even independent of the lapse of time, will give the number of marriages occurring in a group, constituted in any manner whatever in regard to the numbers of either sex. We shall call the tendency to marry the *conjugal potential* under a given condition. In the case of males let the conjugal potential be denoted by γ, and in the case of females by γ'; γ and γ' vary with age, doubtless also with time, and (we may assume) with the relative frequency of *M* and *F*. (Knibbs, 1917: 214).

Then Knibbs discussed the specific conditions of application of the 'conjugal potential': (1) when the conjugal potential is assumed to vary somewhat as some constant; (2) when the numbers of unmarried of either sex are equal or not; (3) if the conjugal potential vary with age; and (4) assuming that the marriage of particular pairs is equally probable, and that the relative magnitude of M and F does not influence the probability, *p*. In addition, he proposed some additional conditions, those which should lead to the expression that will readily 'enable the number of marriages likely to occur in each age-group to be computed when the numbers of unmarried males and females in the group are known' (Knibbs, 1917: 214). Thus, *q* considered the tabular number, the number of marriages, *N*, could be computed by means of the following formula:

$$N_{xy} = q_{xy} \cdot M^{\frac{F}{M+F}} \cdot F^{\frac{M}{M+F}} = q_{xy} \cdot M\phi^{\frac{1}{1+\phi}} = q_{xy} \cdot F\mu^{\frac{1}{1+\mu}}$$

From here, to find *q* from the results furnished in his tables of unmarried males and females and the masculinity at each year of age as well as for computing the effect of unequal numbers of unmarried males and females on the frequency of marriage Knibbs proposed:

$$\log q_{xy} = \log N_{xy} - \frac{1}{1+\mu}\log M - \frac{1}{1+\phi}\log F$$

x and *y* denoting the central values of the age-groups, that is $x \pm \frac{1}{2}k, y \pm \frac{1}{2}k$

where *k* is the range of the group. In order to be more easily applicable Knibbs proposed the following simplification:

Let $S_{xy} = M_x + F_y$, that is, let S_{xy} denote the total number of single persons in the groups of males of age *x* and females of age *y*, and let the masculinity (or femininity) of S be denoted by M/F (or F/M); then assuming that the probability is identical for *A* males and *B* females, with that for *B* males and *A* females (which, however, though by no means certain, is not determinable from existing data) we may compute the value of the ratio

(433)....

$$R_\mu = R_\phi = (M^{\frac{F}{M+F}} \cdot F^{\frac{M}{M+F}}) / \frac{1}{2}(M+F) = F\mu^{\frac{1}{1+\mu}} / \frac{1}{2}S = M\phi^{\frac{1}{1+\phi}} / \frac{1}{2}S$$

which depends merely upon the masculinity, μ (or the femininity ϕ), and is independent of the absolute value of *S*, or of *M* and *F*. Consequently with a table of values of *R* arranged according to the argument μ (or ϕ),we have, by simply dividing *M* by *F* , (or *F* by *M*) and entering the table,

$$(434) \ldots\ldots\ N_{xy} = \frac{1}{2}S_{xy} \cdot R_\mu \cdot q_{xy} = say \frac{1}{2}S_{xy} \cdot Q_{xy}$$

> Q itself could be tabulated but for the fact that the masculinity in age-groups may differ appreciably with the lapse of time.
>
> (Knibbs, 1917: 216-217)

Knibbs then calculated two tables, one for R, depending upon the masculinity (or femininity), and the other for q depending on the frequency of marriage for the age-groups in question. 'After preparing the table of values of R, those of q can readily be calculated', Knibbs (1917: 217) remarked. Moreover, he concluded that in using the values of R, it is 'a matter of indifference whether it the argument 'masculinity' or 'femininity' when determining the frequency of marriage for the age-groups in question.

Following the computation of the masculinity of the unmarried for any combined age-groups Knibbs established the 'probability of marriage according to pairs of ages' as follows:

> Assuming that the 'conjugal potential' does not change in any community, the number of marriages likely to occur among groups of the unmarried of given ages can be computed by means of formula (434) ... If the conjugal potential are the same for A males and B females as for B males and A females, and the law of variation is, as by hypothesis,
>
> $$(437) \; ..... (\gamma + \gamma^{'})\alpha M^{\phi_2} \cdot D^{\mu_2} = M\phi_1^{\mu_2} = F\mu_1^{\phi_2}$$
>
> then the qualification as to masculinity being approximately identical disappears. It is not unimportant, however, to remember, that the fundamental assumption would have to be very erroneous (and that would seem to be impossible) in order to seriously prejudice the precision of the result obtained by the application of the formula (434). The error in any real application of the formula can be a differential one only, and if the constitution as regards numbers of the population be approximately therefore that from which it was derived, any defect in the theory of variation with relative numbers of the sexes, formula (430), has no sensible effect (Knibbs, 1917: 223).[7]

Even by current standards, Knibbs's reasoning remains highly sophisticated and complex; among other things, the formal and empirical aspects of theoretical issues were faced with equal seriousness.

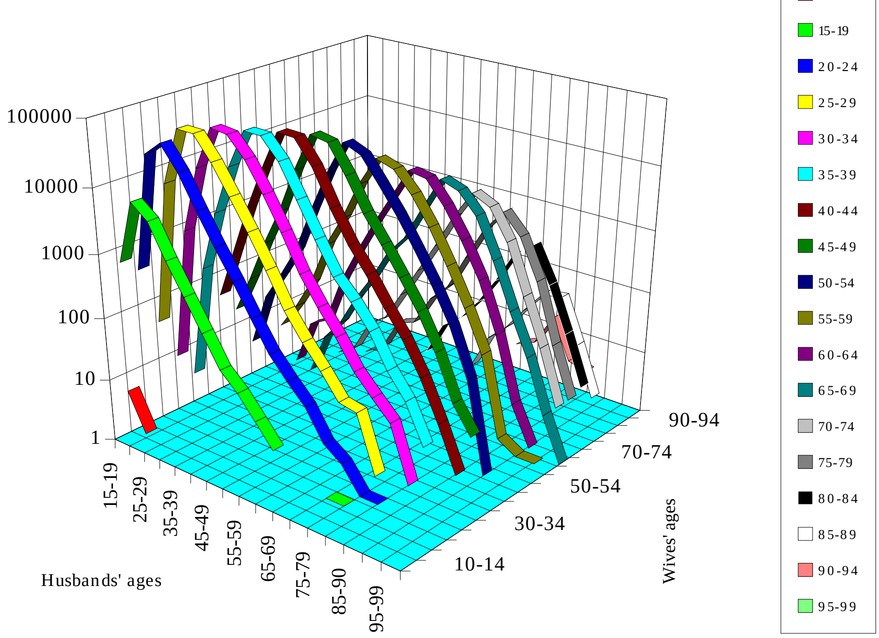
The non-homogeneous groupings

By taking the total number of 616,738 married persons living together whose ages were fully specified, and who were living together on the night of 3 April 1991, Knibbs computed a table of numbers of married persons per 1,000,000 married couples in five-year age groups. The results are shown graphically in Figure 1.7.3.

---

[7] γ denotes the conjugal potential in the case of males and $\gamma^{'}$ in the case of females. μ_1 and ϕ_1 are the same as in formula (433) and refer, respectively to masculinity and femininity; μ_2 and ϕ_2 correspond also to the masculinity and femininity but are drawn from another method which Knibbs (1917: 132) discussed earlier in the chapter on 'masculinity of population'.

Figure 1.7.3 Number of married persons per 1,000,000 married couples, living together on the night of the Census, 3 April 1911



Source: Knibbs, 1917: 224.

These data prompted Knibbs to point out that in calculating the age-groups the sex taken as argument is not irrelevant. The results differ if the age of the husband instead of the wife is used and they 'have no obvious direct mutual relation' (Knibbs, 1917: 224). In this and other 'analogous groupings of a non-homogeneous character', Knibbs admitted that a one-sex approach may be more adequate:

> In cases of the kind under consideration two formulae are needed; in one the argument is the age of the husband (or bridegroom), in the other the age of the wife (or bride) (Knibbs, 1917: 224)

Upon the non-homogeneous groupings of data Knibbs remarked about the differences in the results based on the argument *x* (husband) as compared those based on the argument *y* (wife):

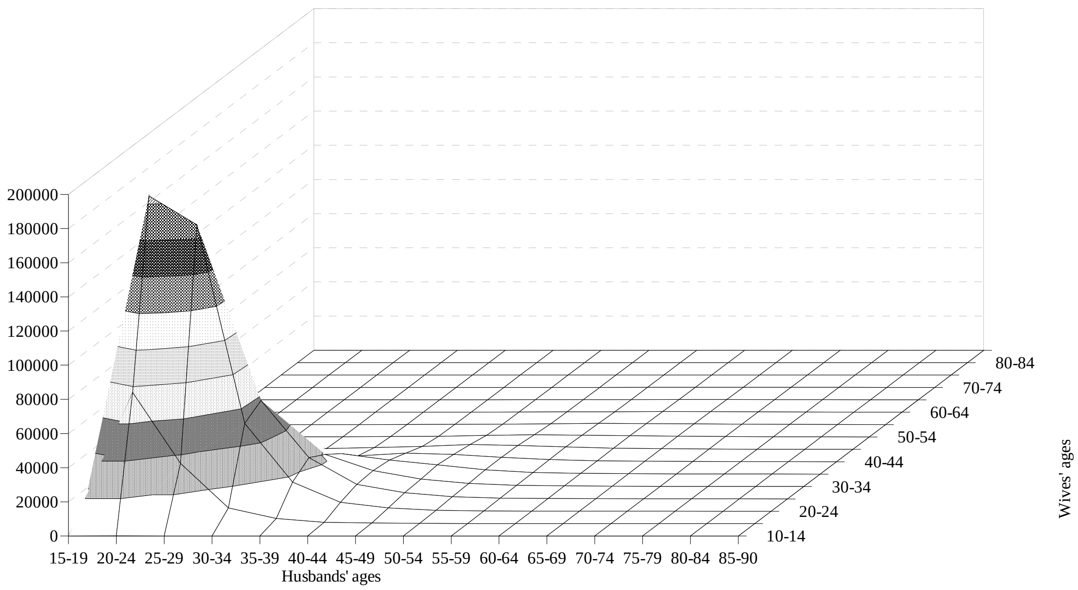
> If the distribution about the mode in such cases be not symmetrical in each, in fact *if it be not similar in all respects*, no direct functional relationship subsists between results for groupings arranged according to the values of *x*, and those for groupings arranged according to the values of *y*. Groupings subject to this limitation may be called *non-homogeneous groupings*, and require special consideration (Knibbs, 1917: 225).

Knibbs specified the average differences between ages of husbands of any age and the average ages of their wives, and vice-versa between the ages of wives and the average ages of their husbands.

Based on the same data used to draw Figure 1.7.4 Knibbs constructed the gamic surface, on the same principles applied to the construction of the protogamic surface. Figure

1.7.4 depicts the protogamic surface chart, which can be compared with the gamic surface chart in Figure 1.7.5. These images are intended to show the reader the level of complexity of Knibbs's formal analysis, which included not just elaborate mathematical reasoning but also geometrical and graphical reasoning.

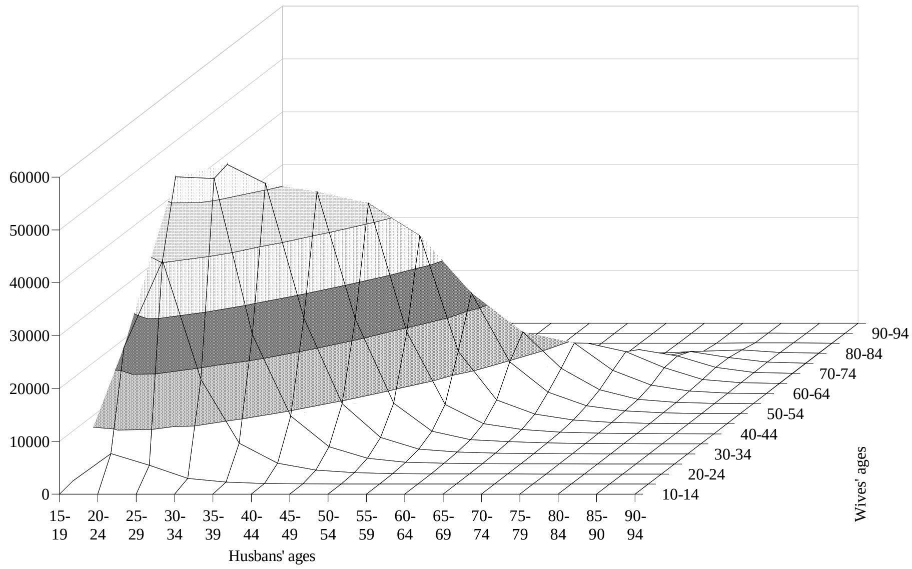
Figure 1.7.4 The protogamic surface, Australia 1907-1914



Source: Knibbs, 1917: 211

In Figure 1.7.4 the isoprotogams are less elliptical and regular than the isogams in Figure 1.7.5. The interpretation of the curves for isogams is, *mutatis mutandis*, the same as that for the isoprotogams. However, while in Figure 1.7.4 the data apply to persons 'living in the state of marriage', in Figure 1.7.5 the data apply to 'persons at the moment of marrying' (Knibbs, 1917: 228).

Figure 1.7.5 The gamic surface, Australia 1907-1914



Source: Knibbs, 1917: 224

Knibbs finished his Chapter 12 with a summary on the two gamic conditions in his theory of nuptiality: (1) the protogamic norm or nuptiality, based on the aggregation of the marriages of a large number of people; (2) the conjugality or gamic condition based on the census results. He added that the protogamic norm should reflect the trend in regard to the early institution of marriage, while the gamic norm should reflect the modification of this by factors such as the change in longevity and the frequency of divorce. These norms could include the curves of the totals according to the age of the males (bridegrooms and husbands), and according to the age of the females (brides and wives), as well as the frequency of the group-pairs. Likewise, the norms of conjugal states such as 'never married', 'divorced', and 'widowed', might also give the frequencies according to group-pairs. Knibbs concluded Chapter 12 writing:

> the probability of marriage depends, among other things, upon the relative numbers among the unmarried of the sexes. So long, however, as a population does not greatly change its constitution according to sex and age, the crude probability of marriage according to sex and age may be regarded as varying approximately as the annual rate. This probability may be called *pheithogamic*[8] *coefficient* for the sex and age in question (Knibbs, 1917: 232).

[8] From the Greek term meaning ‘to prevail upon’, the Goddess of Persuasion, and of or for marriage (Knibbs, 1917: 232).

The monogenous approach: fecundity, sterility and fertility

Following the previous discussion on natality and nuptiality, Chapters 13 and 14 of *The Mathematical Theory of Population* deal with reproductive efficiency through the concepts of fecundity and fertility. The phenomena which directly concern the measure of the reproductive power, one can read at the beginning of Chapter 13,

> are in general complex, the variation of the reproductive power being in part of physiological origin, and in part of the result of the reaction of social traditions upon human conduct. This will appear in any attempt to determine the laws of what has been called bigenous (better, digenous) natality, or natality as affected by the ages of both parents, as distinguished from those affecting merely monogenous natality, or natality as related to the producing sex (Knibbs, 1917: 233).

The review of Körösi's paper provided above should allow the reader to trace the origin of the framework of reproduction sketched in this quotation around the two operational concepts digenous (Körösi called it bigenous) and monogenous fertility. Like Körösi and perhaps all demographers at the turn of the nineteenth to the twentieth century, Knibbs was mostly concerned in establishing the measurement, the methodology and description of fertility as a demographic output. Perhaps the main difference of these two demographers, as compared with others such as Böckh, Lotka and Kuczynski, is that the latter did not hesitate in reducing the study of reproductive efficiency to the producing sex only. As Kuczynski (1935: 206) put it, ‘Since we are concerned here with births only, it suffices to take into account the female population’.

Throughout Chapter 13 and, above all, Chapter 14, Knibbs returned, time and again, to the interaction between the ages of both sexes and the role of nuptiality. Contrary to Körösi, Knibbs did not restricted his analysis of the census data to an empirical one; he also provided a brief but systematic insight on the formalization of demographic reproduction. In deducing the most probable value for certain demographic phenomena, Knibbs remarked, it will be necessary first to minimize the effect of misstatement of age; and secondly, to treat demographic reproductive efficiency as a derivative and dependent ‘upon the age-distribution and conjugal condition of the producing sex’ (Knibbs, 1917: 233). The following sentence indicates Knibbs’s view as to the place of an analysis of fertility from a one-sex and a two-sex perspective:

> Many questions concerning the measurement of fertility and fecundity can be settled with sufficient precision without recourse to a differentiation depending on the age of the father, the better in Australia, perhaps, inasmuch as the decay of virility with the age is not well marked, and in this aspect the digenous fertility stands in marked contrast with that of Hungary ... *Digenous fertility and digenous fecundity* will denote the fertility and fecundity of the female, as modified by the age of the associated male, and therefore is considered in relation to the ages of both males and females. Consequently computations of monogenous fertility or fecundity will be based upon the age of the female (Knibbs, 1917: 233).

It is important to recall an aspect already mentioned in Chapter 6 regarding Knibbs’s definition of the terms 'fertility’ and ‘fecundity’; he used these terms in the reverse way from the current English usage, that is in the same way they are currently applied in Latin languages, such as French, Italian, Spanish and Portuguese. To avoid confusion with the remainder of this thesis, the references made here to fertility and fecundity correspond to their usage in English in contemporary times and this is opposite to the way found in Knibbs's *Mathematical Theory of Population*.

Table 1.7.3 includes Knibbs's compilation of the available methods of measuring reproductive efficiency, which he saw as being ' all more or less defective'; he concluded: 'A more satisfactory scheme is to construct a monogenous age-group "natality table" for married, and one for unmarried, females' (Knibbs, 1917: 236). Even in this case, Knibbs was not completely satisfied: 'It is, however, not perfectly satisfactory, because, as already indicated, it would appear that the age of the father as well as that of mother affects the probability of maternity' (Knibbs, 1917: 136).

Before returning to the effect of father's age upon the probability of maternity, Knibbs went first through a lengthy and detailed analysis of a variety of issues focused on the monogenous female only.[9] In particular, about the 'theory of fecundity, sterility and fertility', Knibbs remarked:

> The *fertility-ratio* [read fecundity-ratio] or *probability of maternity in a unit of time* may be defined as the proportion of cases, which, subjected to a given degree of risk for a unit of time, result in maternity; and similarly, the *sterility ratio* or *probability of maternity* is the arithmetical complement of the probability; or calling these respectively p and q, $p+q = 1$ (Knibbs, 1917: 319).

In 1977, Smith and Keyfitz claimed that Corrado Gini (1924) was the first to explore the distinctions and implications of the fact that pregnancy and birth distributions are mathematically separated by interval of non-risk. If this statement is true it should be only as to Gini's proposal 'that birth intervals be treated as waiting time problems dependent on fecundability' (Smith and Keyfitz, 1917: 365). However, by reading *The Mathematical Theory of Population* it becomes apparent that Knibbs already raised and debated most of the 'probability models of conception and birth' later developed and formalized by authors such as Louis Henry (1953), Basu (1955), Tietze (1962), Potter (1963), and Sheps (1964) (see Smith and Keyfitz, 1977: 365-395). Among the issues addressed by Knibbs's work are the following: probability of a first birth occurring within a series of years after marriage (p. 245); maximum probability of a first birth (p. 248); positions of average intervals for groups of all first-births (p. 267); range of gestation period (p. 276); and proportion of births attributable to pre-nuptial insemination (p. 278).[10]

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[9] Some of such issues were the following: norms of population for estimating reproductive efficiency and the genetic index; the natality index; age of beginning and of ending; the maternity frequency, nuptial and ex-nuptial, according to age, and the female and male nuptial-ratios; maximum probabilities of marriage and maternity; maximum probabilities of first-birth; the nuptial and ex-nuptial protogenesis; initial and terminal non-linear character of the average issue according to duration of marriage; crude fecundity; secular trend of reproductivity; crude and corrected reproductivity; theory of fecundity, sterility, and fertility; fertility according to age and duration of marriage (Knibbs, 1917: 136-344).

[10] The investigation on fecundability lacks much investigation even today. Gray (1995), in a recent seminar at the ANU, presented his work-in-progress entitled 'Returning to fecundability'. Gray attempted to measure the strength of fecundity through a measure of fecundability or the probability of conception in the

Table 1.7.3 Knibbs's compilation of methods of measuring reproductive efficiency

Rate measured by: Numerator	Rate measured by: Denominator	Deduced result know as	Remarks
Total births, *B*	Total population, *P*	Crude birth rate, B/P	Is dependent on age, sex, and conjugal constitution of total population, and therefore not strictly comparable as between different populations; it measures merely one element determining increase.
Total births, *B*	Total female population, *F*	Birth-rate referred to total number of women, B/F	Is dependent on female population only and is affected of course by the age and conjugal conditions of that population.
Total births, *B*	Female population of reproductive age (viz., from about 10 to 60), F, say	Birth-rate referred to women of reproductive age only, B/F	Indicates reproductive efficiency of all women within the reproductive period. Owing, however, to the limits of this period being ill-defined at the initial and terminal ages, to the largeness of the number of women at those ages, and to the fact that it is independent on the age-constitution within the group chosen to represent the reproductive age, the rate is not as definite as is desirable. The denominator, however, is a good crude measure of the potential of reproductive efficiency of the population.
Births in each age-group, B_X	The women in same groups, F_X	Birth-rate referred to women of each age-group in question, B_X/F_X	Is uncertain for comparison because the ratio of married to unmarried women may vary, and the relative frequency of maternity in each is not identical.
Nuptial births in each age-group of unmarried women, B_x^m	Married women in same groups, M_X	Nuptial maternity rate for each age-group, $\frac{B_x}{M_x}$	Shows only the average frequency of maternity (average probability of maternity) for married women in each age-group.
Ex-nuptial births in each age-group of unmarried women, B_x^m	Unmarried women in age-group, U_X	Ex-nuptial maternity rate for each age-group, $\frac{B_x^{m'}}{U_x}$	Shows only the average frequency of maternity (average probability of maternity for married women in each age-group.
Appropriately weighted sum of birth-rates of the married and unmarried	Unity	Modified 'Nuptial Index of Natality'	This attributes the reproductive facts of an existing population to a supposititious 'standard' population, in which the relative number of married and unmarried females is the general average (norm) for the groups of populations to be compared. The comparison so attained may be regarded a suitable comparative measure of reproductive efficiency (natality).

Source: Knibbs, 1917: 236

length of prospective birth intervals after the end of whatever periods of amenorrhoea and abstinence from sexual relations were reported by the respondents in the survey; he applied this to data from the Demographic and Health Survey carried out in Indonesia in 1991.

Complete versus partial tables of fertility: the digenesic approach on fertility

Still in reference to the theory of fecundity, sterility and fertility, Knibbs asserted that the 'degree of risk' of fecundity not just decreases after a certain age of women, but it also 'varies with the age of the husband' (Knibbs, 1917: 319). Yet, in some countries fecundity may vary but slightly with the age of husband, Knibbs acknowledged. Hence, by ignoring the issue of age of husband, Knibbs proposed that in place of complete tables of fecundity and fertility, partial tables may serve 'all general practical purposes' (Knibbs, 1917: 320). Table 1.7.4 summarizes and compares the information required by complete and partial tables of fecundity and fertility.

Table 1.7.4 Complete versus partial tables of fecundity, sterility and fertility

Arguments of complete tables	Arguments of partial tables (i.e. ignoring the effect of husband's age)
(i) Age of wife, with (ii) age of husband (iii) Duration of marriage	(i) Age of wife only (i.e. with husbands of all ages) (ii) Duration of marriage
	Knibbs, 1917: 320

In case of fecundity and sterility, the tables should show, Knibbs proposed, for each combination of age and duration of marriage, the proportion of married women who have born one child. Likewise, in case of fertility, the tables should show, for each combination of age and duration of marriage, the proportion of married women who have born *n* children, where *n* referred to the successively parity 0, 1, 2, 3, 4, etc.

With regard to the 'digenesic surfaces and diisogenic contours', Knibbs explained:

> If the husband's age be not ignored fecundity (read fertility) relations become greatly increased in complexity. For example, instead of a maternity rate or a birth-rate according to the age of wife, we have a series for each age of the husbands; the compilation-table becomes one of double entry, and the various fertility and fecundity-relations become correspondingly multiplied (Knibbs, 1917: 349-350).

The reasoning displayed by this statement corresponds to that of Körösi. Knibbs reviewed the issues treated by Körösi, but added to the subject his much more sophisticated and formal reasoning. From his theoretical discussion and then the comparison of the results for Australian population and those provided by Körösi for the population of Budapest, Knibbs made two important inferences. First, that for a given difference of age in the wife, the equivalent difference of age in the husband is not the same. To make one equal the other, Knibbs proposed to introduce a factor called 'the masculine factor of age-equivalence'. And vice-versa, to make the difference in the wives' age equal, for a given difference in the age of husband, a factor called 'the feminine factor of equivalence' should also be need.

The second inference was more a generalization on diisogeny drawn from the comparison of the results of the diisogeny in Australia with the diisogeny in Budapest:

> For ages greater than that of the maximum fertility of women and for those combinations of ages of husband and wife which are most common, the fertility-ratio may be regarded as represented - very roughly of course - by straight lines: that is to say, *x* and *y* being respectively the ages of husband and wife at the time of the birth, the fertility-ratio is constant when *kx+y* is constant ...The pairs of ages, *x* and *y*, which give identical fertility-ratios, may be called *corresponding age-pairs* ... Moreover the *fertility-ratio* (and thus the value of *k*) *diminishes with increase of the sum of the corresponding age pairs* (the age of maximum value having been passed. Obviously, also, *k* differs for various populations (Knibbs, 1917: 362).

Chapter 14 of *The Mathematical Theory of Population* finishes with a discussion on six issues which Knibbs followed in his subsequent work during the 1920s: multiple ‘diisogeny’, that is the equal frequency of twins, or of triplets, etc., according to pairs of ages, the series of ages giving equal frequency being in this case also known as ‘corresponding pairs’; twin and triplet frequency according to ages; apparent increase of frequency of twins with age of husbands; triplet ‘disogeny’; frequency of twins according to age and according to order of confinement; unexplored elements of fertility. As to the latter issue and in conclusion of the analysis on fecundity and fertility, Knibbs remarked:

> To distinguish between the *effect of previous births* and age upon the frequency of maternity, of twins, etc., more comprehensive data are required than at present exist for Australia. The effect is one which, so far as the maternity-ratio is concerned, reflects social tradition in a larger measure than the physiological law; the latter is modified but not obliterated. In the case of twins, triplets, etc., the physiological laws doubtless alone operate (Knibbs, 1917: 369).[11]

Complementarity and two-sex demography: searching for a purpose

With this Chapter 7, I have concluded the review of the strands depicted in Figure 1.1. The six chapters included in Part I have placed the envisaged two-sex demography in the wider context of the development of demographic theory since its birth and earlier growth; they are expected to yield a valuable contribution to the development of a comprehensive two-sex perspective in three ways. First, the strands reviewed between Chapters 2 and 7 are consistent with the principle of complementarity between the sexes. In particular, they demonstrate that nothing could be more self-defeating for the development of a two-sex perspective than the idea that anything learnt elsewhere could be relevant to demography, even without finding any support in the history of its own ideas. Moreover, the chapters above reveal the utility of defining and following explicitly a guiding theoretical principle and avoid cutting adrift from the history of demography in general.

[11] *The Mathematical Theory of Population* contains four final chapters, 'Mortality', 'Migration', 'Miscellaneous', and 'Conclusion'.

Secondly, the above historical review makes it clear that a coherent two-sex perspective can be developed in close association with the analytical bodies already in use in demography. However unstated and implicit are the theoretical principles in which conventional demography stands, none of its concepts, measures, methods and specific theories can be considered mindless and short of ideas. After all, even the most technical and formal tools in demography can and should be seen as part of specific analytical bodies used to study certain aspects of demographic reality.

There is a third and far-reaching valuable contribution that the historical review provided in Part I is expected to accomplish. I have tried to discuss in a logically coherent manner the strands depicted in Figure 1.1; this seems to be the best way to avoid bringing together several concepts haphazardly and in an *ad hoc* fashion. The concepts in Figure 1.1 follow a sequence which is historically and theoretically consistent with the development of demography. Historically, the sex ratio seems to have been the first demographic measure ever created in the scientific study of population.

Chapters 2 to 4 have focused on the most simple measure of complementarity between the sexes and, in particular, revealed the Janus-like nature of the sex ratio: a measure of matters of fact and an explanatory resource in theory construction. Chapter 5 focused on the 'passion between the sexes', which Malthus used as an important demographic principle in the design of sexual reproduction and associated with reproductive mechanisms like 'marriage' and ‘couple’. Chapter 6 is, perhaps, the pivotal chapter of this thesis because it has already raised the central issue in all this discussion: ‘When, why and how should the complementarity between the sexes matter to demography?’. This question has never been adequately addressed by earlier demographers, not even in current times by demographers who have been interested in important research areas such as the ‘determinants of fertility’ and the ‘two-sex problem’. Chapter 6 traces the evolution of the concept of fertility in demography revealing a periodization called 'three scientific breakthroughs in leaps of one hundred years’; this evolution is consistent with the three bifurcations depicted in Figure 1.6.1.

Chapter 7 reviewed expressions of the complementarity between the sexes such as nuptiality according to pairs of ages, the gamic conditions, the probability of marriages in age-groups, and the mating functions. These concepts have been reviewed in association with Knibbs’s anticipation of a two-sex methodology. Knibbs perceived that demography had much to offer to the knowledge of population change even when the two sexes are studied separately from one another. But following Quételet and Körösi, Knibbs not only wondered about but discussed the feasibility of taking into consideration the role of both sexes in studies of population. Although Knibbs did not suggest any direction towards the conceptualization of fertility in the way proposed in Chapter 6, his *Mathematical Theory of Population* clearly anticipates the necessary condition for a two-sex demography: that for certain purposes the methodological frameworks should explicitly take into consideration the numbers and behaviour of both males and females.

This means that the strands illustrated in Figure 1.1 are mainly relevant to the definition of the necessary conditions for a two-sex perspective and finish where most of the two-sex methods usually start: nuptiality and mating functions. However, Chapter 6 already indicates that two-sex models should exist neither for their own sake, nor even to improve demographic measures that are reasonably produced on the basis of one-sex models.

Behind the idea on the three bifurcations in the development of the demographic concept of fertility lies the view that a two-sex approach needs to be justified in terms of two types of conditions, respectively the necessary and sufficient conditions. While the necessary condition refers to methodological requirements, particularly *when* and *how* a two-sex model should be used, the sufficient condition sets the *whys* for the application of a two-sex methodology itself; it includes the research issue that needs explanation, specific theoretical issues and empirical puzzles, as well as the operational definitions, research hypotheses, and two-sex measures.

Demographers are aware that the complementarity between the sexes works in the daily life of population change. But this awareness is usually drawn from simple commonsense or perhaps the individual experience of researchers. Frequently demographers who investigate demographic change refer to the everyday reproductive role of males and females; but conventional demographic teaching provides no guidelines, nor even discusses when and why demographers should use either neuter, one-sex or two-sex methods. Basic and advanced textbooks and the demographic literature in general continue to shy away from any attempt to explain when and why both sexes should, or should not, be taken into consideration in any scientific study of population.

In the end, even when a two-sex approach would, at least intuitively, seem feasible and appropriate, demography has nonetheless developed as long as demographers have been able to identify the necessary and sufficient conditions to describe and explain specific empirical puzzles. For some purposes a neuter measure (i.e. crude birth rates), equations (i.e. basic demographic equations of population growth) or even theory (i.e. classical demographic transition, Lotka’s neuter stable population theory) can provided satisfactory answers; for other purposes one-sex methods and theories (i.e. net reproduction rate, total fertility rate, one-sex stable population theory) are required. In this context, the history of demographic ideas reviewed in Part I and, in particular the revelation in Chapter 6 concerning the evolution of the demographic concept of fertility according to its three bifurcations seems paramount.

These issues are discussed in more detail in Part II and in theoretical terms. However, it has been the historical journey described in Part I that has led to inference that the leitmotif of the evolution of the demographic concept of fertility seems to be the investigations on the feasibility, usefulness, reliability and validity of the neuter, one-sex and two-sex methods in demographic analysis.

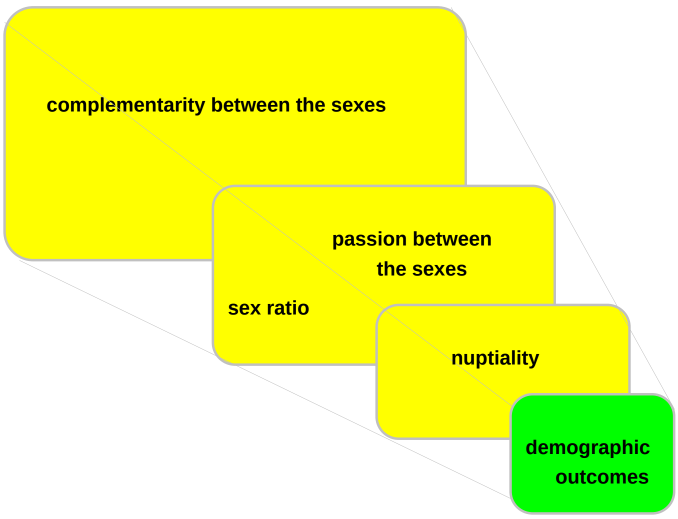
The concept of fertility has not developed by definition once and for all; instead, it has grown out of discoveries and by virtue of two main processes: the requirements necessary to apply specific concepts to the analysis of demographic phenomena, and the functions that new operational definitions perform in the explanatory process. Although working concepts such as nuptiality and mating function are important mechanisms in the functioning of demographic reproduction, it is extraordinary that current literature on fertility determinants does not contemplate the feasibility, usefulness, reliability and validity of the two-sex perspective.

PART II.

When, why and how should both sexes matter to demography?

Demographers measure and describe changes in demographic outputs as derivative and dependent upon the female component of the population and leave the contribution of males implicit. This is so because in demographic reproduction it is women, not men, who bear children. Yet, although men do not produce children the outcome of their practices, attitudes and knowledge are as much important as those of women to explain the causal relationships and mechanisms of demographic change. On the path from description to explanation there is a point in which neither sex can *a priori* be considered eligible to represent, theoretical and statistically, the whole population and be used independently to explain, for instance, why and how fertility rises and falls over time. In that point lies the theoretical necessity to distinguish what may be called *demographic outputs* from *demographic outcomes*. All demographic activities have demographic outcomes, but not all outcomes result in demographic outputs. This is the reason females are found so indispensable at all levels of demographic analysis: descriptive, explanatory and predictive. Perhaps demographers will never be able to specify the total set of contingencies on which a demographic event or relationship depends, but at least they can aspire to ascertain the conditions which are necessary and sufficient to comprehend the whys and wherefores of demographic change. A two-sex demography entails identifying the necessary and sufficient conditions for the use of both sexes.

Figure 2.1 Two-sex demography and complementarity: searching for a purpose



8.

A sketch of a two-sex approach on population composition

> The principles by which progress in a science proceeds can only be reached by observing that progress. They cannot be deduced a priori or prescribed in advance (R. F. Harrod, in Hauser and Duncan, 1959: 27).

The two most important theoretical principles in conventional demography

Today, mainstream demography stands on two principles. The first principle, which I shall call the absolute differentiation among individuals, is the principle that provides the necessary criterion for population to be dealt with as if it was of a neuter gender. This principle has set the grounds, at least tacitly, for the development of what may be called the neuter demography, that is the conceptual and methodological body of analysis within demography constituted by a set of concepts, measures, methods and theories that are neuter by their nature.

The second principle, which I shall call the strict separation of the sexes, acknowledges that population has not one but two natures, male and female. During the twentieth century the body of demographic analysis most successful and dominant in demography has relied mainly on a principle designated here 'separation of the sexes'. This principle has set the conceptual and methodological basis for the emergence of an analytical body of concepts, measures, methods and theories which constitute what I call 'one-sex demography'.

In this context, the body of demography of interest to this thesis associated with the principle of complementarity between the sexes should be seen as part of a three-dimensional epistemological set of principles: differentiation-separation-complementarity (DSC). Together, these three principles cover the content and issues that it makes sense to call demographic phenomena. Simultaneously, to the DSC setting corresponds a threefold methodological approach that I shall call the neuter-one-two-sex methodology.

Each specific methodology has its proper domain of validity drawn from one of the three demographic principles mentioned above. A three-dimensional demographic framework such as this seems to be the most adequate to deal with the question of: why, when, and how both sexes should matter to demographers. It does not reject completely the existing classification of demography into formal demography and population studies, but shows that such a classification is valid only when the neuter and the one-sex approaches are taken for granted; or better, that it works when the core of demographic analysis focuses on the demographic output, such as population size, growth and structure or, in the case of fertility, its levels and trends. Demographic outputs have generally been drawn from frameworks based either on the togetherness and conflation of the sexes, or on the separation and differentiation of the sexes.

These two ways of studying population change have been very useful for the measurement and description of the elements that characterize size, structure, levels and trends of demographic change. However, a description of demographic phenomena does not entail in itself an explanation of the cause and the determinant mechanisms of demographic change. In other words, population change cannot be adequately understood away from a set of relations, activities and characteristics that are intrinsically demographic but not necessarily part of the structure. In particular, the view that nuptiality is not in itself of particular interest to demographers can only be justified on the grounds that demographers are just concerned with population structure. In any case, today demographers can no longer overlook the study of nuptiality. Although nuptiality is not part of demographic structure, it is the locus where the demographic outputs are produced; some authors see in this a certain similarity to the economic market and for that reason the metaphor ‘marriage market’ is now widely used by demographers. Nuptiality sets the core of demographic organization and, thus, is a part of population composition.

The chapters constituting Part II of the thesis supplement the historical overview provided in Parts I and II with a more in-depth logical discussion on the broad theoretical framework of the envisaged two-sex demography.

Chapter 8 is mainly concerned in outlining an approach on population composition suitable to a two-sex perspective. The alternative definition of population composition discussed in this chapter should have immediate implications for the overall demographic analysis. First, a twofold character of population composition comprising structure and organization provides a more coherent basis for the idea that the demographic system is open to the overall societal environment which is changing and evolving. Secondly, in order to make sense of a two-sex demography, the study of the relationship between demographic events and non-demographic phenomena can be reconceptualized; not all variables that are likely to affect and explain demographic change should be regarded as external to the demographic system, a view that derives from the conflation of structure and composition of population.

Within this perspective I explain the success as well as the failure of conventional demography. Chapter 9 focuses on neuter demography, Chapter 10 focuses on one-sex demography. The success of the neuter and one-sex bodies of demography are explained as the results of adequate matching between their conceptualization of the subject matter and the methodology by which they analyse population change. The power of the neuter framework of demography in which the differentiation of the sexes and population composition are merely implicit is simpler than that in which the separation of the sexes is explicitly taken into consideration. The latter, the one-sex demography, has become the dominant approach in the overall twentieth-century demography because of the consistency and ability between its premises, objectives and research questions and the methodological framework in which is built up.

Similarly, a demographic analysis in which both the togetherness and differentiation on the basis of the sexes are explicit is yet more complex but more concrete than the other two. Demographers have to relinquish the principle of separation and independence of the sexes whenever they have to explain demographic phenomena in terms of the interaction, combination or shared relationships between both sexes and single-complementary outcomes.

Figure 2.1 highlights the necessary and the sufficient conditions for a two-sex perspective in demography. The former specifies that the issue or the empirical puzzle that requires explanation is determined by the complementarity between the sexes; the latter establishes that the methods should deal with both sexes not for its own sake but to compute clearly and reliably measured phenomena through measures representing the cluster or clusters of relations of interaction between behaviours from both sexes and single-complementary demographic outcomes. So model and theories can be considered of a two-sex nature as long they express demographic phenomena as a relationship between both sexes and a single complementary demographic outcome. This is the subject of Chapter 11.

Asking demographic questions: what has happened, how, and why?

Ever since Graunt, the scope of demography has been set by the research questions demographers ask about population. Such questions are generally for three quite distinctive purposes: descriptive, explanatory and predictive. The scientific answer to these purposes takes the form of a set of rules, which has its specific properties and modes of generation, irrespective of being used in the purely theoretical, mathematical or empirical areas of demography.

Descriptive demography provides an account of population characteristics and events that have happened to population at a given point or period in time; it deals with questions of the type, 'What?', 'How many?', and 'What has happened?'. This sort of question points to an investigation which establishes the state of population: is it increasing, constant, or decreasing? This can be and has often been accomplished through indicators about the levels and trends of vital data.

Wunsch (1984: 3) was probably right in his assertion that ‘description is not knowledge’. Descriptions deal basically with the set of features which characterize a given phenomenon, though their causes and underlying mechanisms of change are not immediately captured.[1] This interpretation entails a rejection of the naive positivism which regards description as the authentic method of science and explanation as an illegitimate and speculative search for ultimate causes.

The causes or reasons and the mechanisms that constitute the domain of any explanatory science provide the grounds to answer to questions of the type, 'How?' and 'Why?'. Although the 'how' questions are often opposed to the 'why' questions and regarded as part of what has happened, I prefer to place them at the edge between the descriptive and explanatory purposes. To understand the causes of changes in levels and trends of demographic phenomena it is indispensable to study in what way, as well as to what extent demographic change occurs. The 'how' questions are essential, conceptually and methodologically, to the validity of the two-sex approach.

The third types of demographic question are those of a predictive purpose; they rely on the understanding obtained through the descriptive and explanatory results and deal with questions that foretell the likelihood of future observations. Current predictions in demography, namely forecasts and projections, rely mostly on descriptive demography; but their quality depends a great deal on the level of understanding of mechanisms and causal contingencies on which a demographic event depends. In this thesis I concentrate on descriptive demography versus explanatory demography.

'Structure of demography as a subject'[2]

It may be argued that beneath the placid surface of the description of population characteristics and the explanation of its mechanisms of change lies always at least one important ‘why' question. Graunt asked 13 important demographic questions (see Annex

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1 Mora (1978: 96) indicated that already the ancient philosophers regarded a description as an 'insufficient definition' in the cognitive process. They described something that could not be defined. Since the nineteenth century the characteristics of descriptive operations and a clear distinction between description and other cognitive operations, such as definition, explanation and demonstration have been investigated (Mora, 1978: 96-97).

2 This title is borrowed from Schofield and Coleman (1986: 5).

A); while 12 of them were about 'What' and 'How many' questions the last asked 'Why the burials in London exceed the christenings, when the contrary is visible in the country?'.

Indeed, this seems to be the way any science really works: for each answer to a 'why' question a dozen or so questions on 'what' and 'how' questions may need to be asked and answered first. Beyond that, the ‘why’ questions are the ones that provide the motivation for the overall research and prevent science being transformed into a sacred mythology.

Even in modern physics and biology, scientists seem increasingly happy to abandon the brave view that they should live their professional lives steeped in 'what' and, at most, ‘how’ questions.[3] Although this shift in physics has not occurred without controversies,[4] why should demographers remain squarely outside the mainstream? In fact, they do not, though this is not apparent from reading contemporary textbooks and other teaching sources. The predominant view taught to new students is that demographic analysis is mostly concerned with 'What' rather than 'Why' questions:

> The main features of formal demography are thus that it is fundamentally descriptive or analytic rather than explanatory in nature, and that is concerned with demographic phenomena in isolation, not with their interaction with economic, social and other phenomena. In other words, it tries to answer questions which begin 'What is ...', rather than questions which begin 'Why ...' (Newell, 1988: 4).

This view seems to have gained wide acceptance among demographers, at least since the 1950s, when authors such as the United Nations (1958) and Hauser and Duncan (1959: 2) classified demography into two main groups: 'formal demography' or 'demographic analysis', and 'population studies'. The former is said to be confined 'to the study of components of population variation and change' (Hauser and Duncan, 1959: 2); or, in the words of U.N's *Multilingual Demographic Dictionary* to 'the treatment of quantitative relations among demographic phenomena in abstraction from their association with other phenomena'. In turn, population studies are said to be

> concerned not only with population variables but also with relationships between population changes and other variables - social, economic, political, biological, genetic, geographical, and the like (Hausen and Duncan, 1959: 2)

Little wonder that the two last references are rather old; more recent formulations on this matter have changed in wording but not in content. This can be confirmed in current

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3 In past decades prominent theoretical physicists have been busy trying to explain the initial conditions of the universe, including 'why it is that we and the universe exist'. As Hawking (1988: 175) put it, 'if we find the answer to that, it would be the ultimate triumph of human reason - for then we would know the mind of God'. Ridley (1993) confronted the 'why' and 'how' questions concluding: 'But the why question is to me more interesting, because the answer gets to the heart of how human nature came to be what it is' (Ridley, 1993: 16).

4 In a response to Hawking, Peacock dedicated a chapter to the questions 'How or why?' and maintained that 'Science is simply a knowledge of what *is*' (Peacock, 1989: 21). As well, Cohen and Stewart (1994: 431) distanced themselves from Hawking ultimate aspiration and, in particular, what they designated 'The fallacy of seeing God in the Big Bang'.

textbooks such as those of Lucas and Meyer (1994: 1-2), Newell (1988: 3-5), and Tapinos (1985: 5-6). In addition to this, the classification of demography into 'formal demography' and 'population studies' found in current textbooks is supported and strengthened by more analytical papers in the field. For instance, Schofield and Coleman's (1986: 5, 7, 11) discussion of the ‘structure of demography as a subject' indicates no substantive distinction from the one offered by Hauser and Duncan in 1959. Perhaps the difference now compared to four decades ago is the increasing reliance on a more metaphoric language. Instead of formal or mathematical demography, Schofield and Coleman called it 'hard mathematical core' or 'internal theory'; and instead of population studies, they called it the 'outer structure of theory and fact' or 'external theory'.

Hauser and Duncan explained that 'The omission of reference to population "quality" is deliberate, to avoid bringing normative considerations into play'; but they expected that the potential for confusion arising

> from the fact that both demographers and non-demographers study human population in relation to other systems of variables is dissipated if one distinguishes between 'demographic analysis' and 'population studies' (Hauser and Duncan, 1959: 2).

More than three decades have already passed and, while the classification into formal demography and population studies is now widely accepted, a distrust between the two groups has grown. In 1993, Keyfitz commented about this distrust:

> One group thinks that the other is using demography as an excuse for doing mathematics not sufficiently original to be recognized in a mathematical department, and the other is equally unkind, believing that the qualitative and empirical groups are at best missing much in their investigations, at worst not doing science at all. Whether the separation between demography and population study (Hauser and Duncan 1959) is good or bad touches on a second, wider issue of how far the advantages of specialization in science outweigh its drawbacks. One who sees only the drawbacks would have to say not only that there should be no separation of demography from population study, but that there should not be two separate disciplines of mathematics and physics; practitioners of physics should know all the mathematics that they need, just as Newton did. Once it is conceded that specialization has taken us past where Newton was, then why not within the field of population a methodology subdiscipline and a substantive subdiscipline? Fortunately I am not called on to arbitrate this difficult matter (Keyfitz, 1993b: 547-8).

Curiously, between the two groups of demography only one allows demographers to identify themselves by a proper name: mathematical or formal demographers. Conversely, no one dares to call demographers from the other group anything like 'population' or 'informal' demographers. Since the latter are said to rely heavily on the 'interdisciplinarity', at least metaphorically I refer to them as 'interdisciplinary demographers'.

Keyfitz attributed the classification between mathematical and interdisciplinary demographers to specialization, but this justification seem to offer only part of the explanation. First, contrary to what has happened in demography it is hard to imagine that the type of rigid separation between the theoretical and mathematical found in demography would ever get any credit in Newton's field. Secondly, in physics the relationship between quantitative and qualitative analysis does not seem to be framed so rigidly in terms of ‘internal’, referring to the first, and ‘external’, referring to the second.

In any case, in spite of the difference in emphasis, Hauser and Duncan (1959) and Keyfitz (1993b) seem to agree that the locus of explanatory demography should be sought outside the demographic system itself. In fact, this view is consistent with the most commonly used definition of demography: 'the scientific study of human populations, primarily with respect to their size, their structure and their development' (van de Walle (1982) in Newell, 1986: 1; 1982; see also Lucas, 1994a: 1; Namboodiri, 1991: 1; Schofield and Coleman, 1986: 5-6; Tapinos, 1985: 5-6; Wunsch and Termote, 1978: 1).

As Ryder put it in 1964, 'The backbone of population study is formal demography' (Ryder, 1964: 448). If this analogy between population and biological organisms is transposed to conventional classification of demography one is led to think that demographic analysis is concerned basically with the 'skeleton' of population and has no soul and body of its own. Thus, the latter are assumed to be beyond the scope of demography and this is the reason demographers rely so heavily on the 'interdisciplinary approach' (Schofield and Coleman, 1986: 5, 6).

> after decades of concentration on the 'internal' mathematical theory of demography, much greater attention is now being paid to the 'external' theory of the 'initial conditions' imposed by the social, economic and moral context. This development brings both methodological and substantive advantages. In relating population processes to the social context demographers can draw on theories derived from several disciplines, notably biology, economics, sociology and history’ and get inspiration ‘to develop hypotheses’ (Schoefield and Coleman, 1986: 11).

This is a rather sanguine judgement of demographers' work in past decades. Above all, it seems guilty of the misconception that the explanatory resource of the mechanisms and causes of demographic change lie outside the demographic system. The focus of the so-called internal demography is reduced to population structure, and this in turn is assumed to interact directly with the phenomena considered non-demographic.

Another but less used classification of demography can be traced at least as far back as Lotka's work called *Théorie Analytique des Associations Biologiques,* published in two parts in 1934 and 1939. Lotka gave the title *La statistique démographique* to the part of demography focused on the arithmetical examination of empirical data, and *L'analyse démographique* the part referring to the 'necessary relations' among the quantities used to describe the state and the changes in the state of population and imposed by the logic and physical laws (Lotka, 1939: 6-7).

Lotka's classification is very similar to that used in statistics which distinguish 'descriptive' from 'analytical' statistics. Tapinos, in a textbook published in 1985, remarked that there has been more progress in descriptive than in explanatory demographic analysis. Tapinos set the goal for his textbook to seek an equilibrium between the descriptive and explanatory purposes, though he then associated the former with the designation *démarche statistique* and the latter with the term *démarche explicative*.

The association of the term 'statistics' with descriptive demography only, both in Lotka's and Tapinos's classifications of demography, seems somewhat misleading. Even from a purely statistical point of view it is known that statistical analysis is concerned not only with the description of characteristics; its other important component is the testing of hypotheses and the explanation of existing specific relationships.[5]

In the case of demography, the classification into descriptive and explanatory seems acceptable as long as one does not associate only the former with statistics or any other quantitative analysis, and leave the latter to be seen as connected with qualitative, logical and narrative sort of analysis.

In short, Harrod's statement quoted at the beginning of this chapter was borrowed from Hauser and Duncan's (1959: 27) chapter on 'Demography as a science'. Its repetition here is meant to highlight the point that this part of the thesis is, in part, returning to a relatively old and unsettled debate. Not many demographers really believe that they should focus, at all levels of demographic analysis, only on 'what' rather than on 'how' and 'why' questions. More often than not, demographers put forward their own inferences or hypotheses and try to make sense of their descriptions about what has happened to population. As well, just as certain quantitative relations are indispensable for an adequate understanding of population dynamics but are beyond the scope of demography (e.g., conventional economic variables, such as gross national product and income), there are also qualitative relations that are an intrinsic part of the demographic domain. Some relationships in the demographic system are not immediately measurable, but this is not a sufficient reason to consider them beyond the scope of demographic analysis. So, rather than cutting adrift the qualitative from the quantitative aspects of demographic relations, there must be a way to account for both of them as an integral part of the 'internal' composition of the demographic system. In order to do that I relate the descriptive-versus-explanatory purposes in demography and the quantitative-versus-qualitative analysis of demographic relations, with the way population composition has been defined in conventional demography.

Are composition and structure of population the same thing? A reconceptualization

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[5] Kalton wrote about surveys and statistics:

> Since most surveys have both analytic and descriptive components, the distinction is somewhat blurred, but nevertheless it is of great importance for two main reasons. First, analytic and descriptive surveys demand very different types of sample design. Secondly, it proves useful in distinguishing two types of statistical analysis: tests of significance are used for the analytic aspects of a survey while confidence intervals are used for the descriptive aspects (Kalton, 1973: 2).

Conventional demography has used the term population structure interchangeably with the term population composition. Elizaga (1979: 32) wrote in a chapter entitled 'The structure of population by sex and age': 'The composition of the individuals comprising a population according to sex and age constitutes its core quality'. Likewise, more recently, Namboodiri wrote:

> The terms *size* and *composition* refer respectively to the number of people and their makeup in terms of one or more traits (e.g. age and sex) ....The conditions that produce and the implications of changes in population structure (size, makeup, and spatial spread) are of particular interest to demographers. In investigating these, demographers borrow heavily from disciplines such as economics, geography, political science, psychology, and sociology (Namboodiri, 1991: 1).

However, for this thesis, the conflation between composition and structure of population is misleading and needs to be revised and placed in a wider context. Such a conflation has been somewhat irrelevant in the past because the bulk of demography has generally been set around demographic output and fertility output: that is, the description, on the one hand, of the size and development of population structure, and on the other, of the number of births related to the producing sex or the part of population which is actually exposed and has the ability to produce children. For this reason, for many the final goal of demography has generally been accomplished by dealing with births, deaths, marriages and migration as demographic outputs.

The commonly used definition of demography (Lucas, 1994a: 1; Namboodiri, 1991: 1; van de Walle, 1982 in Newell, 1986:1; Tapinos, 1985: 5-6) is consistent with the view of demography as a descriptive discipline; it is also consistent with the two types of classification of demography discussed in the previous section: the one about formal demography versus population studies, and the other put in terms of statistical *démarche statistique* and *démarche explicative*. In this context, though, the fact that population studies are generally considered interdisciplinary, much of the explanatory *démarche* relating demographic with non-demographic variables is often subordinated to a broader descriptive purpose. This seems to be why Burch (1995: 10), in a recent article, remarked: 'The phrase *explaining fertility decline* is ambiguous'.[6] In his attempt to overcome the existing ambiguities, Burch spoke of ‘an abstract theory to explain the fact of continuing decline in overall fertility of national-states in general'; and of 'a theory to explain the timing of the beginning of decline in marital fertility at the local level in a particular time

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6 Burch wrote this while discussing what he called 'Shifting explananda' in association with a set of questions enumerated by Mason:

> As Mason (1992) has asked, What question are we trying to answer? [p.2]. Her answer, in effect: many different questions, often dealt with somewhat indiscriminately. 'Explaining fertility decline' can refer to: 1] overall fertility or marital fertility; 2] large population aggregates such as national-states, or to smaller sub-populations; 3] the fact of fertility decline, or various aspects of timing of decline, including its initiation as contrasted with its continuation (Caldwell, 1976; for a similar distinction regarding migration theory, see Massey et al., 1993) 4] fertility decline processes generally (and therefore abstractly) or to specific (concrete) historical cases (Burch, 1995: 10-11).

and place' (Burch, 1995: 11). Unfortunately, this suggestion is prisoner of the oft-told dichotomy between general theories and particular theories,[7] a problem that undermines Burch’s important debate about the need for theoretical precision and systematic numerical simulation models. 'Demographers are schizoid in the matter of precision', Burch asserted (1995: 5), ‘But in theoretical writing, we accept fuzzy statements of what is being explained.’

As far as the consistency of this thesis is concerned, the problem of fuzzy statements goes far beyond the problem of writing in everyday language because it deals with the basic conceptualization of the demography system in population. Regardless of the language used, the conflation between structure and composition in studying population illustrates more convincingly the casualness in the handling of theory which Burch (1995: 6) considers to be endemic to the discipline.

A reconceptualization towards a precise distinction of the scope of population structure within its composition can have important conceptual and methodological implications. An immediate implication would be, for instance, the identification of an adequate place for Burch’s debate upon 'Reflections on Demographic Theory of Fertility Decline'. Seen from the viewpoint of the existing classification of demography, a great deal of Burch’s debate is part of population studies. Indeed, Burch (1995: 7) is obvious uncomfortable with the conventional, among other things because of the tendency to rely mostly, as he put it, on ‘everyday language, with turns of phrases characteristic of journalism or of nineteenth century essay, and an abundance of metaphors’ (Burch, 1995: 6). The complexity of fertility process and demographic change in general need, as Burch maintained, ‘a resort to mathematics, or something akin to mathematics’ (Burch, 1995: 6). But although 'a translation into a more rigorous and formal language forces theoretical assertions to be more precise' (Burch, 1995: 7), formalization is not a panacea for all ill of demographic theory.

Conventional demographic teaching defines formal demography as the backbone of demographic analysis and, by implication, the structure of population defined by sex and age as the backbone of population composition. I accept this conceptualization, though my departure from orthodoxy leads to the rejection of the tendency to reduce population composition to its structure. For instance, the *International Encyclopedia of Population* (1982) in the entry ‘structure’ says ‘see composition’. As opposed to this I assume in this thesis that the demographic composition of population should be defined as comprising a given structure and organization. Together, these two building-blocks aggregate characteristics, such as age, sex, race, ethnicity, and marital status, which are designed according to a specific demographic goal or set of goals. The implication of this alternative approach is that the body of the demographic system is no longer made of structure only,

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[7] The remainder of Burch’s article dealt with issues such as 'overall or marital fertility decline'; 'high versus low levels of aggregation'; 'decline or the timing of decline'; 'abstract theory and concrete history' (Burch, 1995: 11-20).

nor should everything else used to explain demographic change be assumed to be external to the demographic system. Before returning to a more detailed consideration of the distinction between composition and structure, it may be useful to start with an abstract sketch of a demographic system adequate to this thesis.

The standard demographic system from a two-sex perspective

Like any system supposedly evolving and dynamical, a demographic system of the composition of population should have a given structure and a given organization, as well as be relatively independent of other societal relationships, economic, political, social and ideological.[8] A population can only be seen as alive and functioning when its structure is articulated, rather than abstracted, with its self-organizational subsystem and when the whole system is assumed to be open to the environment in which it is living. While the external environment can affect population dynamics in a variety of degrees, the state of the demographic system is principally defined by its internal setting rather than by the environmental factors exogenous to it.

From this point of view, a population becomes a dynamic complex system comprising two interactive subsystems: structure and organization. By 'complex system' I mean a set of coherent and evolving interactive processes, of which the structure is only one part. In addition, there is a set of coherent, evolving, and interactive processes in which the temporarily global state and inertial structures have nothing to do with a rigid equilibrium and static population organization.

Population change can be generally described in terms of clearly designated and reliable measures such as those depicting the size, growth and structure of population: that is, through the measurement and description of demographic outputs or what is produced by population. However, an adequate understanding of the causation and mechanisms of population change can hardly be understood away from a set of relations, activities and characteristics that are intrinsically demographic but not necessarily part of the structure.

For this reason I consider that population structure refers to demographic relations defined first of all by the categories sex and age, while population organization refers to demographic relations defined mainly by the categories gender and generation. In this context, any demographic measure implies a defined relation between a given quantitative and qualitative information, or between an abstract concept and the observable event. Away from its qualitative relations, the mathematics of population renewal would be meaningless.

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[8] 'Dynamics' is the science of matter in motion, while the designation 'dynamical systems' is the general term for systems whose properties change with time. Dynamical systems have been classified into two kinds, conservative and dissipative. In the former the time evolution is reversible, in the latter it is irreversible (Coveney and Highfield, 1991: 361).

Whether one relies or not on a mathematical language should not be the criterion of classification of demography. The use of mathematics depends just on the possibility of transforming abstract concepts into measurable variables. Moreover, what I mean here by qualitative information has nothing to do with what is generally defined as non-demographic phenomena.[9]

The concept of population can be conveyed by words, but equations, symbols, models and diagrams are not fancy devices. Besides facilitating and refining communication, formalization is undoubtedly the most powerful tool whenever accurate quantitative values and logical relationships are needed. Beyond that, both the quantitative and qualitative languages are used on the grounds that '[any] self-renewing aggregate that generates new members ("births") and loses existing members ("deaths") is governed by certain logical relationships' (Coale, 1971: v).

Before specific details are given about the structure and organization of population, Figure 17 depicts the basic sketch of population composition from a two-sex perspective. Formally, the generic logical relationships in demography can be represented as a system consisting of a set of elements $\vec{a}_i$ representing the number of i individuals in a given population. A demographic system $\vec{a}_i = \{a_1, a_2, \cdots, a_n\}$ is the (finite) set of individuals $\vec{a}_i$ comprising a population. If $\vec{D}_l$, standing for dimension, refers to the set of *l* conditions, attributes and characteristics which link the individuals $\vec{a}_i$, then $S_d = \{\vec{a}_i; \vec{D}_l\}$. That is, the demographic system denoted S_d is the set of all individuals which satisfy the condition $\vec{D}_l$, namely the continued existence (births and deaths) occurring within the time interval per person-year of exposure, so that for all members $\vec{a}_i$

$$\vec{a}_i \in S_d \Leftrightarrow \vec{D}_l(a_i)$$

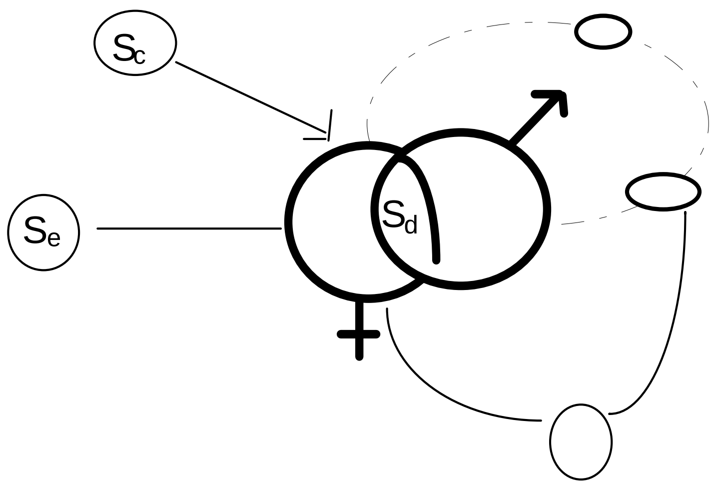
The internal environment of the demographic system is relatively independent, though it articulates with other specific societal relations, such as economic ones $\vec{E}_l$, which make up the economic system $S_e = \{a_i; E_l\}$, or the cultural ones $\vec{C}_l$, which make up the cultural system $S_c = \{a_i; C_l\}$. Moreover, economic, demographic, and other socio-cultural systems can be interpreted as subsystems of a more complex supersystem, the

9 This issue has become a matter of concern in other fields. For instance, the relationship between economic growth and environment has recently gathered momentum in the international agenda, specially because of the question whether or not free trade harms the environment. The following passage is somehow relevant to the debate touched on here about the difference and the relationship between quantitative and qualitative changes:

> We economists need to make the elementary distinction between growth (a quantitative increase in size resulting from the accretion or assimilation of materials) and development (the qualitative evolution to a fuller, better or different state). Quantitative and qualitative changes follow different laws. Conflating the two, as we currently do in the GNP, has led to much confusion (Daly, 1993: 29).

overall society S_*. Therefore, $S_d \in S_*; S_e \in S_*; S_c \in S_*$. These logical relationships are depicted in Figure 2.8.1. [10]

Figure 2.8.1 The standard demographic system from a two-sex approach



The purpose of population studies, as has been stated in conventional demographic literature, is to analyse the relationships between demographic and non-demographic variables. The internal setting of the demographic system is reduced to the set of variables and components necessary to describe population structure. However, for this thesis to be coherent the notion of 'demographic organization' is introduced as a complementary part of population structure within population composition. The lack of this notion in mainstream demographers can be attributed to two main factors. First, most twentieth-century demography has been concerned in measuring and adequately describing population change, and for that it was necessary to abstract and control for the effects of its structure and organization. Secondly, both 'formal demography' and 'population studies' have focused on the state and change in demographic outputs; this has been accomplished by studying variables such as size, growth, structure, fertility, mortality and migration while the mechanisms of interaction are abstract and implicit.

[10] This view of the demography system is drawn from general system theory and, in particular, the authors Berlinski (1976), Jantsch (1975, 1980, 1981), and Vishnevsky (1991). The notation used is mostly inspired on Vishnevsky's (1991) paper. What Vishnevsky calls the 'terminal approach' is in its general form similar to the perspective of demographic system outlined here; particularly the idea that demographic relations need to be seen as a relatively autonomous and sovereign self-organizing system. However, Vishnevsky did not elaborate on the content of the demographic relations, at least in the way this is done here. The diagram is inspired by Jantsch's (1980: 33) notion of adaptation of the cyclical dissipative organization based on the so-called Belousov-Zhabotinsky reaction.

Descriptive demography versus explanatory demography

A two-sex demography cannot make sense if one reduces the content of population composition to its backbone and the core of its study to formal demography; its explanatory power would become as much limited as it is in the neuter or even the one-sex approach. Likewise, the potential for adequate consideration of interdisciplinary relationships with relevant social, economic and cultural variables becomes fuzzy and obscure when the demographic system is assumed to react to external factors. I agree, at least in part, with Hawley's assertion that

> the individual characteristics to which composition refers include sex, age, marital status, place of birth, education, occupation, labour force status, industry, relation to head of household and other such features (Hawley, 1959: 361).

However, following the approach outlined here the individual characteristics considered by Hawley need to be classified into two main groups. On the one hand, there is the set of characteristics which make up the content of population structure, including the standard variables sex and age, fertility, mortality and migration. The second group refers to the set of characteristics which make up the content of demographic organization such as nuptiality and family formation. Contrary to the category of demographic output, I have already proposed the notion of demographic outcome which is expected to account for the total cluster of practices, attitudes and knowledge of both sexes likely to explain the reasons for demographic output. The concept of outcome seems to be the most adequate for understanding why and when both sexes should be taken into consideration. Only females produce demographic outputs, but as far as demographic outcomes are concerned the consequences of the demographic behaviour of males can be as relevant to explaining demographic change as those of females.

Hence, the development of an adequate explanatory demography requires the study of the mechanisms and causes of demographic change. This implies that even basic concepts in demography which have hitherto been accepted as unambiguous need to be reconsidered, namely: the definition of descriptive demography compared with an explanatory demography; the need to distinguish population structure from population composition; the recognition that the composition of population includes relationships which are indispensable to the demographic system, such as those embodied by the terms nuptiality and family formation.

The study of population structure has been successfully undertaken around concepts and measures which in their fundamental way constitute the content of the category demographic output. However, once one accepts that the composition of population should not be restricted to its structure it seems inevitable that the category of demographic output needs to be expanded. I have already proposed the term 'demographic outcome', which can be interpreted as a working concept intended to into consideration the demographic practices, attitudes and knowledge that do not result immediately in demographic outputs but can affect them in a variety of ways. Table 2.8.1 confronts the view of population composition consistent with the descriptive demography and the alternative more adequate for explanatory demography. The former draws on the objectives suggested by Hawley (1959) and the latter is my own restatement of Hawley's objectives on the basis of the two-sex approach outlined in this chapter.

Table 2.8.1 Population composition from a two-sex approach

Descriptive demography	Explanatory demography
1. Data on composition make possible an elaboration of the description of population and therefore permit detailed interpopulation comparisons. They are frequently used in testing the representativeness of strata in a sample drawn from a known universe.	1. Data on composition should make possible an elaboration of the explanatory variables by drawing attention to the set of contingencies and causes that determined the structure and organization of population. They should be used in testing the representativeness of strata in a sample drawn from a known universe and depicting the mediating processes that causally link demographic output and demographic outcome.
2. Such data also constitute an inventory of the human resources of a society.	2. Such data should also provide an account of the flows within the demographic system.
3. The data describe the variables essential for analyzing demographic processes, e.g., birth, death, migration, and growth. In the absence of direct information on demographic processes, composition data, particularly age and sex data, provide a means for estimating the incidence of birth and death.	3. The data should describe the variables essential for analyzing demographic processes, not just in descriptive terms, but an explanation of the causal components of demographic outcomes which, by definition, are of two-sex nature. In the absence of direct information on demographic processes, composition data should complement age and sex data with data on the clusters of gender and generation likely to provide the means for explaining the mechanism and interaction of estimated incidence of birth and death.
4. Demographic variables, together with population size, are important conditions affecting the formation and change of social structure. (Hawley, 1959: 361)	4. Demographic variables, including those dealing with demographic output (i.e., population size, fertility) and those concerned with demographic outcome (i.e. nuptiality, sex ratio and family life cycle from a two-sex view point), affect and are affected by the variables of other social systems (i.e., economic, cultural, environmental).
	5. The study of the components of effect on which the internal and external causes of demographic change have impact entails the identification of the total set of contingencies on which single demographic output depend. This set of contingencies should take into consideration both sexes, for the demographic outcomes are the derivative of the gender-generational between males and females at different institutional levels.

Structure, or why the body of population does not fall down

The title of this section is inspired by Gordon's (1978) book *Structures: Or, Why Things Don't Fall Down*. Gordon defined a structure as 'any assemblage of materials which is intended to sustain a load'. By implication, population structure can be seen as the assemblage of individuals in various categories or classes of specific traits or variables which is assumed to sustain the load of population composition.

The standard variables of population structure are sex and age. These two variables have set the basis for descriptive demography, both epistemologically and methodologically. From the epistemological viewpoint, sex and age play in real life as much as in theory a function not paralleled by other individual traits, such as weight, size, and colour. Methodologically, sex and age can be seen as the single most important factors influencing the construction of all the existing partial demographic theories. And formally, especially in mathematical and statistical terms, sex and age have provided the most important reference frames; without them certainly the mathematical and statistical expressions of population could hardly be grasped.

Figure 2.8.2 depicts how the shape of population structure is generally represented graphically by what has been conventionally called 'population pyramid'. This graphic is constituted of two ordinary histograms placed on their sides and back to back, the left representing the age distribution of male population and the right representing the female age distribution. In conventional the age-sex pyramid is generally considered in studies on the relationships between age structure and components of population change (i.e. fertility, mortality and migration), and in association with indicators which summarize age distribution such as sex ratio, dependency ratio.[11]

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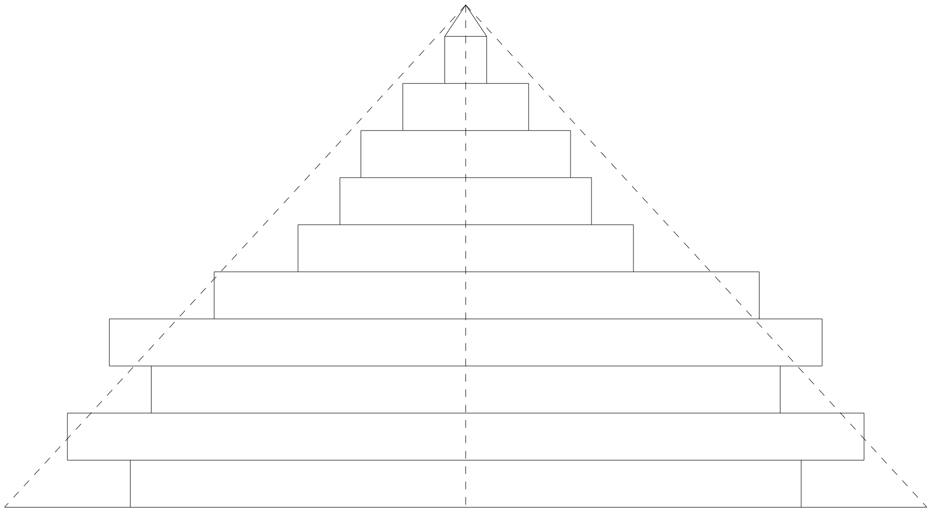
[11] A recent special issue of *Demography* was dedicated to 'family and household demography' and focused its attention on 'intergenerational relations and 'gender relations'. Curiously, a summary paper from Goldscheider (1995) used the age-sex population pyramid to admonish demographers to pay more attention for the generational and gender relationships in a population. As this Chapter shows this use of population pyramid is quite loose. Goldscheider did not provide any clear suggestion on how demographers should articulate 'sex' and 'gender', 'age' and 'generation', and 'structure' and 'organization' in their analysis. Likewise, Goldscheider's generalizations drawn from the individual papers included in the special issue provide a rather naive explanation on why 'demographers are left to focus their analyses of fertility change wholly on women and the changes in their lives' (Goldscheider, 1995: 476).

Figure 2.8.2 the sex-age distribution of population pyramid



So the standard variables sex and age have always been relevant not just for a demographic analysis, in quantitative terms. All areas of demography, whether mathematical, theoretical, empirical or historical, have been deeply affected by sex and age. And unless the full theoretical implications of their role in demographic theory is taken into consideration, demographers will continue to have difficulties understanding how facts and theory interact with social context; by implication, the biases in our own thinking about the belief that the one-sex theory triumphed in demography by extensive fieldwork will remain obscured.

The reference frame of population structure: sex, age and time

Ever since Graunt's *Observations* the sex-age system has set the fundamental demographic frame of reference of population structure. The term sex refers to differing physical characteristics which affect directly the classification of individuals into women and men, and the incidence of births, deaths and marriage: chromosomal patterns, genital characteristics, reproductive ability, mammary development, menstruation and menopause, and other secondary body characteristics. The categories of sex are 'female', the fertilized, and 'male', the fertilizer (Fontaine, 1978; Lucas, 1994b: 44-55; Miller, 1993; Richmond-Abbott, 1992: 33-64; Waters and Crook, 1994: 235-236).

It is widely known that the simple dichotomy between females and males is not always clear-cut. Modern biological science has gathered convincing evidences to allow authors, such as Fausto-Sterling (1993), to make the point that 'there are many gradations running from female to male; and depending on how one calls the shots, one can argue that along that spectrum lie at least five sexes - and perhaps even more'. Despite this, to infer that demographers should apologize for assuming that human reproduction is determined by two sexes would be the same as charging any scientist for relying on fundamental relations even when they now that everything in the universe depends on everything in its fundamental way.

The variable sex in demography and the way it is used have important conceptual and methodological implications.[12] First, while sexuality has increasingly become separated from biological reproduction the opposite or the separation of reproduction from sexuality remains irrelevant to demographic analysis. Human reproduction continues to be sexually determined and requires the union of two sexes based on casual or long-term mating relations. Second, despite the fact that the concepts of sex and sexuality refer to males and females, a demographic analysis does not always have to deal with both. In general, all demographic and even non-demographic approaches used in the study of population may be classified according to the way sex is taken into account or ignored in of specific models. Third, from a mathematical point of view, demographic analysis can be more or less complicated depending on the assumptions made and the methods applied when sex is related with the variable time: linear or non-linear, deterministic or stochastic, continuous or discrete time. But the most simply and widely used demographic measure of the sex composition is the sex ratio and its aftermath, femininity and masculinity of population.

In turn, with regard to age, as Hawley (1959: 364) put it: 'Age shares the universal recognition accorded to sex'. The importance of age for demography is of two types: first, like sex, age determines the physiological characteristics which directly affect the classification of individuals into categories such as children, young, and ageing population. Secondly, age determines the direct presence in reproduction by fixing the outer limits of fecundity. Thirdly, from a mathematical point of view, while the range of ages over the life span of individuals is wide, as far as mensuration is concerned it has a privileged position for allowing precision in quantitative analyses. An individual's age at birth always has the fixed value zero and it increases linearly with time: $da/dt = 1$. The consideration of age can also be possible through several methods using continuous or discrete-time, linear or non-linear approaches, and deterministic or stochastic models.

In short, the sex-age system constitutes the basic reference frame of population structure; this system is enmeshed in the organizational system set by gender-generation,

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12 As the astrophysicist Hawking (1988) put it:

> If everything in the universe depends on everything else in a fundamental way, it might be impossible to get close to a full solution by investigating parts of the problem in isolation. Nevertheless, it is certainly the way we have made progress in the past. The classic example again is the Newtonian theory of gravity, which tells us that the gravitational force between two bodies depends only on one number associted with each body, its mass, but is otherwise independent of what the bodies are mode of. Thus one does not need to have a theory of the structure and constitution of the sun and the planets in order to calculate their orbits (Hawking, 1988: 11).

which can be seen as the frame of reference that defines population organization and is linked with other societal systems.

Demographic organization, or why population composition is not just bones

Broadly speaking, the word organization refers to a set of activities and their arrangement which fit together well from the point of view of certain goals or purposes. By implication, the demographic organization of population can be seen as the subsystem in the whole system of population composition; it comprises the cluster of practices, attitudes and knowledge of both sexes that contribute to a specific demographic goal or set of goals.

Similarly to what conventional demography has done for population structure, it seems important to identify a set of characteristics, qualities and traits which are intrinsically part of the subject matter of demography. This includes, for instance, those aspects which conventional demography have addressed as proto or quasi-demographic, such as marital status and family life cycle.

Nevertheless, the view that nuptiality is not in itself of particular interest to demographers remains influential in the discipline (see Newell, 1988: 91). This can only be justified on the grounds that demography is mainly concerned with population structure and, above all, the unspoken reliance on the neuter and one-sex approaches to demographic phenomena.

However, demographers of today can no longer overlook the study of nuptiality and family formation. Although nuptiality is not part of demographic structure, at least demographers recognize that nuptiality and family formation provide the locus where the demographic outputs are produced (Hawley, 1959: 366; Newell, 1988: 90; VandenHeuvel and McDonald, 1994: 69). Some authors have found certain similarity between demographic relations and, for instance, the economic market; for that reason the metaphor ‘marriage market’ is now widely used by demographers.

Nuptiality and marriages are at the edge between the dominant one-sex demography and the envisaged two-sex demography. Even though some of the demographic relations cannot be easily enumerated and measured, they directly affect population structure and are essential to make sense of any demographic estimate. In particular, they provide the necessary links between individuals and a basis for understanding the behaviour of population in specific processes, such as the formation and dissolution of marital unions and families.

Nuptiality sets the core of demographic organization and, thus, is a part of population composition. In this way, the definition of composition of population emerges as the interaction of two subsystems comprising variables which entails the identification of the set of contingencies not only in population structure but also in its organization and overall functions. This approach offers an important alternative for the putative approach of population of composition which has been generally undisputed for the past four decades.

'When the term "composition" appears in discussion about population’, so Hawley (1959: 361) wrote, ‘it usually marks a turning point from the gross and general to the defined and specific'. The dichotomy between general and specific as well as between high and lower levels of aggregate data have constituted the centre of most discussion and critics to important analytical frameworks in demography, including the theory of demographic transition, the economic theories of fertility and other theories of fertility determinants.

Somehow demographers have to accept the fact that population is neither an aggregate of isolated individuals, nor a collection of sub-populations which reproduce separately from one another. The fact that real population includes two sexes is not an anomaly. The nature of sexual reproduction determines that both sexes matter to whatever happens to fertility and population change. The crucial question may be: when should a one-sex or a two-sex approach really matter?

What is called here population organization should not be confused with the broad societal relationships generally defined as economic political, social and cultural. While these external environments, so to speak, affect the state of the demographic system, the internal environment has a dynamics of its own and is not just a derivative of exogenous or non-demographic factors.

The reference frame of population organization: gender, generation and time

Following the conceptualization of population structure around the standard variables sex and age, the content of population organization can be defined around two closely related categories: gender and generation. To my knowledge these latter variables have never been used in the perspective proposed here, and thus their usefulness cannot be as immediately apparent as the long-used variables age and sex. It may be easier to start from 'generation', a category rather more familiar and less controversial for demographers than gender.

The term generation has long been given a precise meaning in demography and developed far beyond its loose usage in everyday language denoting persons of similar age at the same time (United Nations, 1958: 6). Throughout the twentieth century formal demography has converted the term generation into a powerful working concept and precise applications: an indicator that measures demographic reproductivity, or the extent to which one generation is reproducing itself; a measure of 'generation time', or the average span of time between the birth of parents and the birth of their offspring; even more specific, measure of the 'length of generation', as proposed by Lotka (1939: 70; see also Shryock and Siegel, 1971: 527) to denote the approximate mean interval between the birth of a female or a male and the birth of their children; and still Keyfitz’s (1968: 102) ‘mean age of childbearing’, a measure of the timing of fertility as the mean age of mothers at the birth of their children.

Anyone not familiar with demographic techniques should at least be advised that demography uses the concept generation not as many other concepts, such as individual, women, births and deaths; the latter concepts have their own meaning, but they are not necessarily drawn from any precise and complex methodological apparatus. Even the variable ‘age’, whose utility to demographic analysis has been undisputed for more than three centuries, is not rooted in such a huge methodological construction as ‘generation’. A significant part of this thesis is, directly or indirectly, concerned with the methodologies lying beneath the category ‘generation’, namely the one-sex and two-sex methodologies. Therefore, however devious and clumsy the term generation may appear when compared with age, this appearance is actually the source of its strength. It is generation that absorbs age rather than the reverse, because generation is broader in its conceptual and methodological representation of relations relevant for demographic analysis.

Since the 1960s, the term generation has expanded to new fields of demographic phenomena. From a highly aggregate and technical indicator, extremely useful to dig deeper into the study of vital rates and population growth, the term generation began to be applied to micro-level issues and thus lower aggregation level. This represented a very important shift in demographic theorization and many conceptual and methodological implications. New areas of research have developed, such as those concerned with lifetime experience of different types of cohorts and households, as well as the timing and duration of stages in the family life cycle (Bongaarts, 1983; Bumpass, 1990; Burch, 1979; Ryder, 1964, 1992; Young, 1977, 1994).

At the same time, the work of Caldwell (1976a, 1978, 1983, 1985b, 1988, 1994; Caldwell and Caldwell, 1992) has transformed the category ‘generation’ into a more operational definition for studies of family demography. With Caldwell's famous paper of 1976, the notion of ‘intergenerational wealth flows’ entered the demographic lexicon to inspire new investigations in demography and, perhaps, elsewhere, such as the variant of wealth flows theory outlined by the anthropologist Handwerker (1986, 1989) (see also Berquo and Xenos, 1992; Cain, 1981, 1982, 1983, 1988, 1993; Cain, Khanem and Nahar, 1979; Dyson and Moore, 1983; Federici, Mason and Scogner, 1993; Folbre, 1983; Malhotra, 1991; Mason, 1987). Indeed, an aftermath of demographic research focused on micro-level relationships has led to an increasing appreciation of the anthropological investigation on kinship and cultural systems (Baxter and Almagor, 1978; Caldwell, 1976a; 1978; Collier and Yanagisako, 1987; La Fontaine, 1978; Hammel, 1990; Hartman, 1981; Lesthaeghe, 1989; Makarius, 1977; Mead, 1967; Meillassoux, 1981; Robertson, 1991; Sansom, 1978; Wilson, 1989; Miller, 1993).[13]

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[13] Gessain pointed out in 1948 that the term 'anthropology' was born a year after Guillard created the term 'demography' in his book *Eléments de Statistique Humaine ou Démographie Comparée.* Gessain stated that on 17 June 1856, Alban de Quatrefages started the first course of anthropology in the 'Muséum d'Histoire Naturelle'. The sciences of anthropology and demography, Gessain maintained in his article, have developed the knowledge about 'man', which is their common subject matter, but from different points of view (Gessain, 1948: 485-500).

'It is no accident', so Hobcraft (1985: 84) wrote in reference to the World Fertility Survey (WFS), 'that the major shift of emphasis in demography from a macro-level approach coincided with the WFS'. In fairness, such a shift seems to have preceded the WFS, if not even inspired it. About four decades ago Davis and Blake (1956) sketched a framework of 'intermediate variables' which was intended to systematize the study of fertility determinants. Bongaarts's important operationalization of Davis and Blake's framework led to his elegant formalization of the 'proximate determinants' of fertility (Bongaarts, 1978, 1993a, b; Bongaarts and Menken, 1983; Bulatao and Lee, 1983).

At this stage, especially after the discussion of the development of the fertility concept in demography in Chapter 8, it should be easier to start drawing raising attention to the fact that either Davis and Blake's (1956) framework or Bongaarts's transformation provided a clear distinction between fertility output and fertility outcome proposed in this thesis. From this distinction is it possible to infer that particularly Bongaart's model provides an elegant analytical framework to study the proximate determinants of fertility output rather than fertility outcome; it deals with the intermediate variables associated with the female part of the population in reproductive age and affecting the length of childbearing experience, including conception, pregnancy and parturition. To some extent, the extensive empirical research carried out in the 1970s and 1980s by WFS can be seen as the culmination of the development of the concept of fertility as output; as Caldwell (1985b: 45) put it, 'WFS during the 1970s could be described as a rather elaborate international fertility census ... a "world fertility intermediate variable survey"'.

Some important features can already be emphasized as to the conceptualization of the term 'demographic organization' in population composition, and the association of its content with the category 'generation'. The following words from Cleland et al. (1985: 3), commenting on Caldwell's paper 'Strengths and limitations of the survey approach for measuring and understanding fertility change', should help to get to the point:

> Caldwell ... has been deeply involved in both survey work and other field approaches and is thus uniquely placed to discuss the relative merits of various strategies for research. Naturally, many of the strategies he advocates for research into demographic understanding would have been unacceptable in a major international programme aimed at providing information on levels and trends in fertility and related variables. The views expressed by Caldwell can perhaps be set against the innovative use of results from WFS surveys to confront major demographic theories in some of the later chapters (Cleland et al., 1985: 3).

This statement confirms an inevitable inference from the discussion in this thesis. Most of the analyses on fertility in current times have been limited to fertility output. This is so because they are conceptually and methodologically based on a one-sex approach and have generally been set to describe and measure fertility levels and trends. However, very often the existing fertility theories are set with the aspiration to originate explanatory results about the causes and mechanisms of fertility change. WFS can illustrate this point further.

As Cleland et al. (1985: 3) wrote, WFS 'aimed at providing information on levels and trends in fertility and related variables'. As well, Scott and Chidambaram detailed three specific objectives of WFS:

> to assist each country to obtain the data needed to describe and explain the fertility of its population, to collect data that would be internationally comparable, and to help build up each country's survey capacity (Scott and Chidambaram, 1985: 8).

Following this clarification, Scott and Chidambaram reacted badly to some criticisms of the WFS theoretical strait-jacket:

> There are other critics who seem to say that the WFS should have started from a firmer theoretical foundation. Paul Demeny and Judith Blake in particular, have voiced this kind of complaint, though it is not entirely clear to us whether they are truly criticizing the content of the WFS questionnaire or merely lamenting the underdeveloped state of the science of society. We doubt whether any sophisticated person with a knowledge of the real world and its institutions would seriously suggest that the WFS ought to have been designed as a means to test some specific theory about fertility. In the first place, no one theory could possibly have commanded the acceptance of the funding agencies, the WFS guiding committees and the countries themselves. Only a broadly descriptive survey could hope to gain support as a *world* fertility survey, destined to consume a substantial fraction of all the funds available for social research on Third World fertility over a decade (Scott and Chidambaram, 1985: 20)

This statement suggests that Demeny and Blake have not been clear enough, but it is doubtful that by 'firmer theoretical foundation' they really meant that the WFS should have been designed to test any 'specific theory about fertility'. This disagreement seems to pinpoint the gap between a descriptive and an explanatory survey. It is doubtful that the funding agencies would not have supported the latter, though Scott and Chidambaram (1985: 20) may also be right in saying that the state of knowledge did not get there. However, Scott and Chidambaram's description of the objectives in the quotation above does not make it clear that higher expectations have in fact been created:

> In an authorized report on the programme, it was emphasized that, 'the principal objective of the project was to provide information which would be of value for those policy-makers who aim to change fertility' (Caldwell, 1985b: 45).

Already in 1980 McNicoll had pointed out the anomalous situation in which despite the fact that 'it is widely agreed that we do not have an adequate "theory" of fertility'

> quite important allocative decisions may be influenced by considerations of 'population policy', considerations in turn based on statistical linkages between fertility and other variables whose theoretical interpretation is by no means clear (McNicoll, 1980: 441)'.

It is doubtful that a policy-maker who intends to set an agenda for fertility change will be in position to do so by simply knowing accurately its level and trends. This seems to have been the concern behind Caldwell's dissatisfaction with WFS surveys. Curiously, Cleland et al.'s contempt for Caldwell's 'strategies ... for research into demographic understanding' is reminiscent of the odd situation of Knibbs's conceptualization of fertility described in Chapter 9. Just as Knibbs, though for different reasons, Caldwell's conceptualization of fertility could not be accommodated in the WFS's conceptualization of fertility. The reasons are only partly spelt out in the quotations provided above, among other reasons because the 'firmer theoretical foundation' advocated by some authors for WFS seems to have never been adequately discussed.

After describing the WFS as a 'world fertility intermediate variables survey', Caldwell wrote:

> This is an area where its strength also renders it vulnerable, for there is the feeling that some of the causes of fertility decline have been explained rather than the mechanics of the decline. The basic concern for scholar and planner alike is the nature of change within a society that initiates secular movements in age at first marriage, duration of lactation and periods of abstinence, and the adoption of any methods of fertility control (Caldwell, 1985b: 45-46).

In another part, Caldwell turned to the limitations of surveys and, especially 'the complete omission of an important area of inquiry' (Caldwell, 1985b: 46). 'The questions that survive', Caldwell continued,

> are the ones that are more easily quantified, more easily defined and often simpler in concept. Much of the success of WFS has lain in a concentration on such questions. Nevertheless, it should be stressed that there are other questions that remain of importance even though they are not easily quantified or cannot be answered by a single person. The pressures towards further reproduction were measured in WFS by asking the female respondent, 'Do you want to have another child sometime?' In large families, with complex power relations, such as we widely find in the Third World, the question is deficient, if not ludicrous. Nevertheless, improving it would have meant redesigning the survey so that all persons with a direct influence could be questioned (Caldwell, 1985b: 47).

In reference to questions on the likelihood of non-contraceptors subsequently adopting birth control, Caldwell commented further on a specific question asked in the World Fertility Survey:

> 'Do you think you and your husband may use any method at any time in the future so that you will not become pregnant?' In work in south India, not as part of the WFS programme, it has taken us hours often over months or years, to investigate with women such probabilities, exploring the unspoken attitudes of their husbands and the veto powers of their parents-in-law (Caldwell, 1985b: 47).

Although the above sequence of statements seems to be moving into details that would be more appropriate for another chapter, they are here for the right reason. Just as an image is worth one thousand words, this illustration is worth a long dissertation about how far the new research directions in demography have already gone without being accompanied by an adequate reconceptualization of the scope of population composition; nor by an adequate justification of why in the second half of the twentieth century demographers have become increasingly interested in some methodologies of other disciplines, such as the so-called 'quasi-anthropological methods'.

In conformity with the orthodoxy, the research directions mentioned in this section should be seen as part of the so-called 'population studies'. Yet there is no reason why any attempt to formulate fertility theories should not aspire to become a part of 'formal demography'. In fact, this seems to be Burch's (1994, 1995) position. However, Burch's papers, like any other literature about population composition, provide no indication for a very particular problem: how to move away from the idea that the research directions mentioned in this section are not about population structure and, simultaneously, avoid the situation of displacing them to the fuzzy plateau of interdiscipline because they do not fit in the orthodox view of demographic system.

So far, I have made no direct reference to the category 'gender'. Together with generation, I consider gender an important component of demographic organization because it can integrate the set of relations of complementarity conferred on the basis of the assumed sex differences. This does not mean that different gender systems are exclusively determined by sex differences, just as structure should not be reduced to the organization of population. The following definition from Wilson seems adequate here:

> Gender is a basis for defining the different contributions that men and women make to culture and collective life by dint of who they are as men and women. It is gender that absorbs sex rather than the reverse, because gender is the basis for the only sensible allocation of functions throughout a culture, rather than simply in its work and labour (civil) system (Wilson, 1989: 2)

Gender relations can be and are often developed more or less independently of physical characteristics of individuals. For instance, only women bear and breastfeed children because only females have the physiological capacity to do so; however, whether women are the ones who primarily raise, care and educate children as well is determined by the system of gender relations in multiple levels of population organization. The same can be said about the decision-making affecting reproductive performance, decisions about when and how many children women will actually have. Besides the roles set by the physiological factors of sex (maleness and femaleness), individuals are far from being genderless as to their practices, attitudes and knowledge concerning reproduction and survivorship.

Yet, one cannot assume that the term 'gender' itself is as much accepted in demography as the category 'generation', to say nothing about its development in studies at the macro and micro-levels of demographic analysis. In part, some of the most prominent attempts to introduce the term 'gender' in demography have originated more confusion than understanding. Many demographers have resisted even the use of the word itself, because some have replaced the term 'sex' by 'gender' overnight without any clear justification. For instance, demographers who have been used to read Ryder's papers, such has those already mentioned in this thesis, cannot miss the difference in an article he published in 1985. Where, in the past, Ryder used the term 'sex' he substituted the term 'gender'; thus, 'population by age and sex' became 'population by age and gender'; and 'one-sex model' turned into 'one-gender model'. Ryder gave no explanation for this change, leaving the reader to wonder wether he regarded the change as irrelevant, or perhaps too obvious to deserve being explicitly presented as one of 'recent developments in the formal demography of the family'.

In this context, the proposal of 'gender' as part of the basic frame of reference as far as demographic organization is concerned requires some additional considerations. Is gender just in grammar and sociology? Does demography need both 'sex' and 'gender' or should it use only one of the two? What is the main approach on gender in contemporary demography?

Is gender just in grammar and sociology? A third application

In contemporary times the term 'gender' has been applied for two quite distinct purposes: grammatically, to classify nouns and, sociologically, to classify behaviour on the basis of assumed sex differences; as a sociological category ‘gender’ is currently and increasingly recognized in all social sciences, psychology, sociology, anthropology, history, literature, economics and demography.

Yet, there is a third application of ‘gender’, which has been tacitly used in theory construction but not explicitly recognized for its own value. I call this usage the ‘theoretical’ gender because it is applied in all stages of theory construction, especially in epistemological and methodological terms. In this thesis I have been using the ‘theoretical gender’ in the classification of demographic analysis as ‘neuter’, ‘one-sex’ and ‘two-sex’. Table 2.8.2 places this new meaning in the context of the other two.

Table 2.8.2 Three different usages of the sex-gender system

	Neuter	Female/Feminine	Male/Masculine
Grammatical sense (vernacular gender)	x	x	x
Sociological sense (social gender)		x	x
Epistemological/methodological sense (theoretical gender)	x	x (one-sex)	x (one-sex)
		Two-sex	

Historically, the first application of the term 'gender' has been in the grammatical classification of nouns. As Illich (1983: 3) put it

> English nouns belong to masculine and feminine, or neuter gender. I have adopted this term to designate a distinction in behavior, a distinction universal in vernacular cultures. It distinguishes places, times, tools, task, forms of speech, gestures, and

> perceptions that are associated with men from those associated with women. This association constitutes *social* gender because it is specific to a time and place. I call it vernacular gender because this set of associations is as peculiar to a traditional people (in Latin, a *gens*) as is their vernacular speech.
>
> I use gender, then, in a new way to designate a duality that in the past was too obvious even to be named, and is so far removed from us today that it is often confused with sex (Illich, 1983: 3-4).

Elsewhere in the same book, Illich (1983: 138) mentioned that Jespersen published in 1965 *The Philosophy of Grammar* and suggested 'the use of the terms feminine/masculine to refer to gender (the grammatical classification of nouns), and female/male to refer to the sex of the object designated by the noun'. Illich pointed out several inconsistencies in Jespersen's proposal and insisted on the need for a distinction between 'social' gender and what he called the 'vernacular' gender.

The application of 'gender' as a grammatical category continues to be used in modern English; nouns belong to one of three grammatical kinds, masculine, feminine, and neuter gender. In turn, the usage of 'social' gender can be traced at least to the work of the American anthropologist Mead (1935, 1967), although this author still called it 'sex' rather than 'gender'; especially her book *Male and Female* (1967) provides much insight on the distinction and the interdependence between the biological and social traits and the differentiation of individuals on the basis of assumed sex differences.

The psychologist Stoller seems to have been one of the first authors to call for a better distinction between the categories of sex and those of gender. In 1968, Stoller published a book called *Sex and Gender* and, in 1972, discussed the issue of 'bisexuality' as 'the bedrock of masculinity and femininity'. Also, in 1972, Oakley published *Sex, Gender and Society,* and insisted on the usefulness and social dimensions of the distinction between sex and gender. In 1975 the anthropologist Gayle Rubin elaborated on the same issue and proposed the expression 'sex-gender system', in which she placed marital relations as the core instrument of a male-dominated social organization.

During the 1980s and 1990s the literature on 'social' gender has grown exponentially and with relatively different tendencies but all predominantly concerned in understanding the role of gender relations in society: Apter, 1985; Boulding, 1992; Brettell and Sargent, 1993; Charles, 1993; Connel, 1987; Epstein, 1988; Ferree, 1990; Friedl, 1975; La Fontaine, 1978; Goldberg, 1993; Greenhalgh, 1994; Greenhalgh and Li, 1995; Hartman, 1981; Illich, 1983; Keller, 1985; Lesthaeghe, 1989; Mafeje, 1991; Mason, 1986, 1995; Miller, 1993; Richmond-Abbott, 1992: 33-64; Seccombe, 1993; Waters and Crook, 1994: 235-236; Vuorela, 1987; Waring, 1989; Wilson, 1989. These, among many other references, are far from homogeneous as to their theoretical and philosophical positions and the political and ideological proposals. Indeed, it is increasingly more difficult even to speak of a monolithic meaning within the ‘social’ gender. The original meaning, as pointed out above, was intended to recognize and stress the difference between the categories and the working of ‘sex’ and ‘gender’; for this reason Rubin even proposed the expression ‘sex-gender system’. However, it was not long before some authors began to use ‘gender’ to replace ‘sex’ altogether; I have already mentioned two examples more relevant to demography, one borrowed from Lucas (1985: 7) and, the other in Ryder’s article of 1985.[14]

Other authors have become interested in gender as an euphemism for ‘women’, or as Nelson (1993: 122) put it, ‘pertaining to women’;[15] among these, some have been particularly concerned to credit American feminists with the most recent usage of gender. Scott (1988, cited by Watkins, 1993: 570), for instance, wrote that ‘In its most recent usage, “gender” seems to have first appeared among American feminists who wanted to insist on the fundamentally social quality of distinctions based on sex’. While in the passage below, Nelson writes as if feminism were a monolithic approach, it is apparent that she tries to distance herself from a ‘popular association’ that she does not specified:

> First, a central insight of feminism is ... the recognition that many traditional social divisions between men and women are socially created and malleable ... Second, the term ‘gender’ is seen as systematically referring to both masculinity and femininity; the popular association of gender with only the feminine side of the dualism implicitly assumes that masculinity is unmarked or universal, and only femininity is ‘tainted’ with gender (Nelson, 1993: 122).

This view, although not necessarily false, is hardly applicable to a significant part of current literature on gender and non-feminists only. In any case, just like Nelson and probably in the last five years only, a few authors have started to acknowledge some of the inconsistencies in the use of the sociological notion of gender. Watkins borrowed from Scott one of the most interesting definitions of gender. Following the statement above, Scott defined gender as follows:

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14 A more institutionalized example can be found in the publications of the Australian Bureau of Immigration and Population Research (BIPR). Since the early 1990s the BIPR has abandoned completely the term 'sex' (see Borowski and Shu, 1992; Shu et al., 1995). Curiously, this decision has not been supported by the Australian Bureau of Statistics which has continued to rely mainly on the term 'sex'. The BIPR was the institution directly responsible for the 1994 report taken to the Cairo Conference: *Australia National Report on Population* (National Committee Secretariat, 1994), which replaced all ‘sex’ by ‘gender’ and treated this shift so naturally that it provides no explanation.

15 In the 1960s and 1970s 'women’s studies' developed around the notions of 'women in development' (WID), which came into use after the publication of Boserup's (1970) book *Woman's Role in Economic Development*; in the second half of the 1970s, Marxist feminists proposed an alternative of their own, 'women and development' (WAD). According to Rathgeber (1990: 18) WAD 'grew out of a concern with the explanatory limitations of modernization theory and its proselytization of the idea that the exclusion of women from earlier development strategies had been an inadvertent oversight'. In the 1980s the term 'gender' started to be used instead of 'sex' and 'women'; in association with this appeared the expression 'gender and development' (GAD), an alternative of the socialist feminists to WID and WAD. Rathergeber maintained that WAD is not concerned with women *per se*, but with the 'social construction of gender', its assignment to women, and the objective to change the position of women in society. For more details on this and other aspects of 'women's studies' see Rathgeber (1990), Richardson and Robinson (1993). Whatever these and other authors mean by 'social construction of gender', many have been more concerned in changing the wording or simply covering up their ideological affiliations than proposing new ways to deal with gender as relational category and transcend, like Bem (1993: 130-131) the artificial gender polarization that men are said to have invented.

> The word denoted rejection of the biological determinism implicit in the use of such terms as 'sex' or 'sexual difference'. 'Gender' also stressed the relational aspect of normative definitions as femininity ... according to this view women and men were defined in terms of one another, and no understanding of either could be achieved by entirely separate study (Scott, 1988: 29, cited by Watkins, 1993:570).

Scott was ambiguous and somewhat misleading when she asserted that ‘In its most recent usage, “gender” ... denoted rejection of the biological determinism’. As I have shown above this usage is, in fact, the first of a series of applications for purposes somewhat at odds with that implied by Stoller (1968), Oakley (1972) and Rubin (1975). Furthermore, the second part of Scott’s definition seems to give the most consistent and useful direction in the sociological usage of the term ‘gender’: that is, the view that women and men should be defined in terms of one another, and no understanding of either can be achieved by entirely separate study. That the view has motivated the former use of the concept ‘gender’ is certainly false, but it is misleading to overlook the different directions that have developed in the meantime. Bem, in a book published in 1993, referred to the existing woman-centred studies and admitted their guilt of gender polarization similar to that which can be found in ‘men-centred’ studies:

> With respect to gender polarization the case is clear. For all of its emphasis on a woman’s unique ability to transcend the artificial polarities that men are said to invent, the woman-centered perspective has so completely polarized women and men, along with what it defines as the male and female modes of relating to reality, that for all practical purposes, both men and women are limited by homogenized visions of themselves as ever before ... Granted that it is now men rather than women who are being denigrated, and granted also that the words *masculinity* and *femininity* are not being used explicitly, still, these are not real women being celebrated and real men being pilloried. These homogenized visions are but the flip side of the polarized, gender caricatures of androcentrism (Bem, 1993: 130-131).

Despite the moralist tone of this sentence, its stress on ‘gender polarization’ is consistent with what in this thesis I call the ‘separation of the sexes’ as the underlying principle behind the one-sex approaches in demography. The problem, however, is that at this stage the discussion is already not only about gender relations as a matter of fact but gender as operational resource in theory construction.

This issue should lead to the recognition of a third meaning in the use of the word ‘gender’, which I call the ‘theoretical’ gender. In this case, the classification more appropriate is not the duality masculine-versus-feminine, but even ‘neuter’ should have its role. However useful and real the duality implied by the use of the ‘social’ gender may appear,[16] the question of theory construction raises other types of issues.

At first glance, the recognition of the term 'neuter' in discussing demographic theorization may appear fortuitous and strange. When I started to speak in terms of ‘neuter

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[16] It is impossible to go further into details concerning the aplication of ‘social’ gender. However, the current association of gender presumes that gender identity is restricted to the conventional two sexes: male and female. This seems rather unsatisfactory, for it does not addmit that the existence of more than two gender identities, including those individuals who do not identify themselves with their biological sex at birth such as homosexuals, lesbians, and transsexuals (Buchbinder, 1994).

theories' in the preparation of this thesis, some people questioned the validity of this expression on the grounds that 'neuter' is a gender in itself and there is no such entity in reality. However, this argument is the result of the unspoken but very influential belief in a model of reality independent of theory.

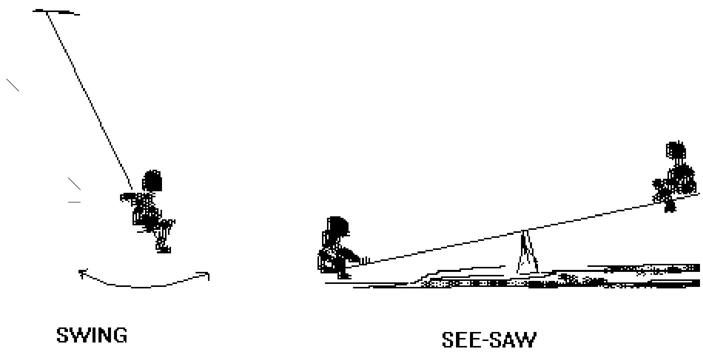
However, it seems important to notice that just as with the nouns in grammatical terms, the process of theory construction in demography and other social sciences includes, implicitly or explicitly, concepts and models that are neuter by their nature. Terms such as 'individual', 'person', 'population', 'gross national product', 'income', 'money', 'birth', 'institutions determinants', 'country', 'migration', 'economy', and 'family' are part of important theories and methodologies which deal with population as if it was of neuter gender. This occurs whenever demographers, like other scholars, conceptualize population as a undifferentiated aggregate and as if population was sexless or of indeterminate sex between masculine and feminine.

In short, I have been using the term 'gender' not in the grammatical sense to classify nouns, nor in the conventional sociological sense to mean 'masculinity' and 'femininity' in social relations, but to distinguish the theorization according the assumptions lying behind specific approaches and methodological frameworks. From a theoretical point of view, and by this I mean both conceptually and methodologically, demographic theories can belong to three relatively different groups. 'Neuter' theories and methods are those which deal with people through abstract and impersonal categories under the assumption that people are human beings, regardless of their sex and gender. 'One-sex' theories and methods are those which are biased towards either sex separately, and thus can be male-centred and female-centred. 'Two-sex' theories and methods are those which assume that neither the female nor the male part of the population can *a priori* be treated as more important and adequate to understand demographic change. This is the reason in Chapter 6 I have pointed out that two-sex approach is imperative in a two-sex demography, but stressed also that that alone should not be considered sufficient.

The 'swing' versus the 'see-saw' gender approaches in demography

I have prepared Figure 2.8.3 to complement the oral presentation of the mid-term review for this thesis and two papers presented elsewhere (Francisco, 1994a, b, c). The term 'swing' seems to be an adequate metaphor to stress the nature of neuter and one-sex approaches. The former deals with population as a point in space and then confronts it with specific variables such as economic growth, environment, income, education, and life expectancy. The latter focuses on either sex separately and applies methodologies which appear to swing the part of the population under consideration back and forth against some variables. In both cases, it is possible to measure the level and trends of , say, population growth and fertility change; it is also possible to conjecture about possible correlations and causes, but there is no way to ascertain anything specific about how (the mechanisms) and why (from the point of view of a given subject) a certain change has occurred.

Figure 2.8.3 The one-sex versus the two-sex approaches on gender



The epistemological and methodological motivations lying behind the swing approach to gender may vary from liberal to more or less radical, but overall the swing approach can lead to two main types of exaggerations. Depending on the philosophical and ideological strategies embraced, it leaves the one-sex perspective intact and the researcher can either over or under-estimate both the experience of power and the experience of powerlessness of the sex being studied at the expense of the sex abstracted. Some economic theories, including particularly those labelled as neoclassical and Marxist, have in the past focused on areas dominated by male experience; notoriously they neglected the role of women, for instance, in societal labour and migration experience. In demography, the physiological role of women in reproduction, especially their role in childbearing, has generally been isolated from both their own social role and the role of men in childrearing. On the other hand, feminist theories either tend to exaggerate women's experience of powerlessness, what Boulding (1992: 5) called 'woman as victim', or to celebrate their uniqueness and power.[17]

In contrast to the ‘swing’ approach on gender, the term 'see-saw' seems an adequate metaphor to stress the principle of complementarity between the sexes and, in particular, the idea that certain phenomena need to be studied on the basis of both sexes simultaneously.

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[17] Wolf (1993), in her recent book, *Fire with Fire*, exhorts women to abandon 'victim feminism' and embrace 'power feminism'.

If one grasps the methodological implications of the see-saw approach on gender the fact that someone uses the term ‘gender’ does not immediately mean much. It can mean that the research is using it in a variety of ways, some of which have been discussed above.

Explicable as the past male-centred economics, on the one hand, and female-centred demography may be, their theoretical basis will remain misleading if not adequately justified epistemologically. Otherwise, any research carried out on behalf of women or men only, rather than on behalf of both sexes, can easily be charged not being seriously committed to transcending the artificial gender polarization that men are said to have invented.

Why does demography need both 'sex' and 'gender'?

This is not the place to enter into much detail about the implications of the use of the ‘theoretical’ gender. Instead, it seems important that demographers should resist to the rejection of the variable 'sex' as much as they should be receptive to the category 'gender'. Just as with age and generation, the term ‘gender’ should not opposed but integrate the term ‘sex’. On the one hand, sex, like age, is used in demography more as a variable and, in particular, as one of the components of the basic reference frame of population structure. Gender, like generation, can be used in demography as a more complex category than a simple variable and, in particular, one of the components of the reference frame of population organization.

From this point of view, the three major misconceptions identified above can be overcome. First, the view that the term 'neuter' is not applicable to demography can be countered with the argument that no social science approaches the reality independent of theory. The important aspect is not to decide whether the neuter approach is wrong or not, but to understand that for some purposes it is useful and has its own domain of validity. The same can be said about the one-sex approaches.

A second misconception refers to the use ‘gender’ as a euphemism for 'women'. Chant and Radcliffe (1992: 2) attested the importance of gender in studying migration and development and justified their slant towards women as aiming ‘to compensate for the relative paucity of detailed studies on women’s experiences in migration compared with those of men’. This view is not different from the so-called WID, WAD and GAD (Rathgeber, 1990). [18] In the 1990s, however, some authors have become, if not declared, feminists, sympathetic to feminism, and appear to despair of demographers' resistance to

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[18] As a result of the feminist and gay political movements, in the 1980s the so-called 'men's studies' have developed as well. In this case, gender is treated conceptually and methodologically 'pertaining to men' (Buchbinder, 1994; Farrell, 1993).

feminist interrogation. The anthropologist Greenhalgh in 1994 raised the issues in the following terms:

> Over the past decade or so, women's activities and lives have played increasingly prominent roles in demographic explanations of fertility. Nevertheless, demography has remained curiously resistant to the sorts of feminist interrogation and transformation that fields such as anthropology, history, and sociology have undergone. In anthropology, for example, the understanding of sex-linked differences has moved from a prefeminist state in which women were neglected or misrepresented, to an ‘anthropology of women’ in the 1970s, in which androcentric biases were corrected and women's experiences retrieved for scholarly analysis, to an ‘anthropology of gender’ in the 1980s and 1990s (Greenhalgh, 1994: 27).

According to Greenhalgh, 'In the mid-1990s, demography lies somewhere between a pre feminist stage and a "demography of women" '; and in 1995 the same author wrote: 'demography has taken only the first step of adding women to the equation' (Greenhalgh and Li, 1995: 602). This seems a mockery of the metaphor ‘invisible women’ so convincingly applied in other fields (Folbre, 1982, 1984, 1986a, b, 1993; Folbre and Hartmann, 1988; Massiah, 1993; Waring, 1989). After all, Greenhalgh is trying to convince demographers that although women are far from being invisible in fertility analyses, they are simply not there.[19]

A third misconception refers to vulgarization of the term 'gender' by simply substituting the term 'sex' or reducing it to a variable like the standard variable ‘sex’. Mason as been one of the most prominent writers on gender issues in demography. Although in one of her latest papers Mason still reduced gender to a variable and, in general, continued to take the one-sex approach for granted (see also Mason, 1992a, b), she raised the following note of caution about precipitate conclusions upon gender systems:

> Indeed, the study of this topic is recent enough that we are only just beginning to understand how to conduct high-quality research on it.
>
> Given this preliminary state of our knowledge, it is surprising and perhaps unfortunate that many in the demographic and feminist communities have already decided that gender change is a *sine qua non* for demographic change (Mason, 1995: 1).

This comment was motivated by the Program of Action of the United Nations International Conference on Population and Development, held in Cairo in 1994, which Mason considered ‘clearly unwarranted in light of our current state of knowledge’ (Mason, 1995: 1).

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[19] At the mid-term seminar on this PhD thesis the historian feminist Allison McKinnon defended this view arguing that women are used by demographers as bodies not identities. As I have pointed out elsewhere (Francisco, 1994c) implicit to this is a view of males as ‘ghosts’, for even when they are not counted as numbers and bodies they are assumed to be in the equations manipulating fertility decision-making. The suggestion that men are not counted in most demographic analyses is certainly controversial for someone who naively embraces the ideology of women's total subordination to the patriarchal regime. On these grounds, it is hard to expect recognition of the usefulness of work done in formal demography on the interaction between the sexes. This is consistent with the idea that while demographers have little to offer to the development of gender theory, feminists alone can teach them a great deal.

Between entropy and ergodicity: demographic functioning, growth and evolution

In general, demographic measures are intended to depict the state of population, its changes and evolution. The distinction between change and evolution in population dynamics seems important, though textbooks do not make it clear. One of the objectives of demographic analysis is the study of population growth. This is basically achieved through measures of the level and trend in size of population, whether increase or decrease. But this aspect of population change depicted by the growth of population size expresses merely the multiplication or the self-renewal of one and the same space-time structure and says little about demographic evolution.

In demographic terms, evolution is associated with the mutation or change in population structure. As far back as the 1920s, Lotka referred to the evolution of a given system as the history of that system in the course of irreversible transformation. Yet, Lotka added, although evolution and history are related not all history is evolution; systems in purely periodic motion are not necessarily evolving, for they repeat in endless succession the same series of events. So the important feature in terms of evolution, Lotka maintained, is that the changes in the system are the result of irreversible transformations in a certain direction, the direction of increasing entropy (in thermodynamics) or increasing probability (in statistical terms) (Lotka, 1956: 20-29, 38-40; see also Jantsch, 1981: 84-85). More often than not, the mathematical equations used to measure population growth usually do not depict the irreversibility transformation mentioned by Lotka; they are of linear and deterministic nature as well as dealing with time as a reversible rather than irreversible variable.

In demographic analysis the methods are not distinguished on the basis of the core principles on which they stand; this fact contributes to the difficulty of relating them with the distinction mentioned here between growth and evolution. Later I discuss the distinction between a neuter and a one-sex approach, though it may be already advanced that in the study of population the former is not supposed to control the effect of population composition.

In turn, the study of population structure has led to the development of what I call one-sex demography. Although the one-sex models rely strongly on the existing linear mathematical techniques, their main objective and strength is to control the effect of population structure.[20] This means that despite the fact that the one-sex approach abstracts

[20] In 1974, Lee made an interesting discussion on the patterns of population fluctuations based on determinist and constant variables and the alternatives provided by linear stochastic difference equations.

from the demographic organization in a population (i.e. nuptiality), it already considers the evolution in the demographic system as Lotka defined it.[21]

Authors like Keyfitz (1977b) and Goldman and Lord (1986) applied the concept of entropy to the life table seen as negative feedback or as time's forward movement in the one-way process of aging. Goldman and Lord asserted that the concept of entropy

> can help us to assess the impact of changes in demographic behavior on vital rates, specially, the effect of variations in fertility on the intrinsic rate of growth (Demetrius 1979; Demetrius and Ziehe 1984), of changes in mortality on life expectancy (Keyfitz 1977; Demetrius 1979) and of changes in marriage dissolution rates on the average duration of marriage (Goldman, 1984) (Goldman and Lord, 1986: 275).

In a more recent article, Hill (1993: 43) proposed an alternative measure of entropy of the survival curve, on the grounds that the measure of entropy of life table 'introduced by Keyfitz as the elasticity of the expectation of life ... is not a true measure of entropy in the probability sense'. According to Hill (1993: 44)

> This use of the term is strictly incorrect, since the survival curve is not a probability density. However, as shown by Demetrius (1979), H_k plus the logarithm of the expectation of life (i. e., H_s) is the entropy of the table age distribution generated by the survival curve. A probability distribution more directly related to the survival curve is that of the age at death, the entropy of which will be denoted by H_f (Hill, 1993: 44).

Contrary to the negative feedback produced by the force of mortality and referring to the irreversible transformation, the increasing entropy or decreasing the output in a system, sexuality may function in the demographic system as a positive feedback.[22] With regard to the structure of population the phenomenon of ergodicity indicates that, as time passes, the birth history in a birth sequence and any vital events tend to cease to determine

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[21] About the link between the concepts of evolution and time Lotka wrote:

> The failure of the differential equations of dynamics to discriminate between *t* and *-t* raises the question as to the physical significance and origin of our subjective conviction of a fundamental difference between the forward and the backward direction in time, - a conviction that is intimately bound up with the concept of evolution, for, whatever may ultimately be found to be the law of evolution, it is plain that no trend of any kind can be defined or even described without reference to a favoured direction in time ... The concept of evolution ... applies principally, if not exclusively, to systems that outwardly at least affect the aperiodic habit, systems that do not return periodically to their initial state, but show a definite trend, whereby yesterday and tomorrow are never alike, and differ moreover in some definite and characteristic fashion, even though we may not be fully competent, at the present epoch of science, to specify exactly wherein lies the characteristic difference (Lotka, 1956: 37-39).

[22] I was the German physicist Rudolf Clausius who introduced the concept of entropy in science in 1850-60. His objective was to distinguish reversible and irreversible processes in association with the second law of thermodynamics. As Coveney and Highfield (1990: 150-151) explained, while the entropy in a reversible process is zero, during an irreversible process it relentlessly grows with dissipation and attains its maximum value when all the potential for further work is spent. Clausius named entropy from the Greek words *en* (in) and *trope* (turning), with the intention of representing the 'transformation content' or 'capacity for change'. Ever since the concept of entropy has increasingly been used in disciplines as diverse as thermodynamics, statistics, mathematics and demography to measure the capacity for a system to change.

the future course of population change. In Coale’s words 'age distributions gradually "forget" the past' (Coale, 1972: 3; see also Arthur, 1981, 1982; Lopez, 1967; McFarland, 1969). Moreover, the forces of fertility (sexuality and ergodicity) can produce self-organization in three qualitatively different ways: temporal organization by gender and generation; spatial organization corresponding to gender-differential patterns of territorial movements; and a combination of the two, when waves of activity generate institutional and organizational changes in the society in general. So, the intergender-generational relations can be perceived as the optimistic or positive feedback in population dynamics. This means that the demographic dynamics of population is not driven only by a blind destructive direction in a steady and increasing descent into disorder, as might be implied by considering just the aging process in the passage from birth to death. There is also the arrow of life, as manifested by intergender-generational relations with their own regulations and intricate patterns of organizational processes.

Demographic goals and population composition: reproduction and survivorship

A goal is an intended future state of affairs which may be either intrinsic to a certain organizational process or outcome, or an intended change in its environment. To accept that population composition obeys a certain design implies that the demographic system is ordered for a purpose.

Population dynamics has always attracted wide interest. In part, this is because through its manifestations people can feel the magnitude and direction of the relationships concerning their reproduction and survival. Malthus envisaged that population composition should be designed by certain principles: reproduction and survival. At the heart of population dynamics lie two synthetic measures related, respectively, to the number of children born to a woman or a couple, and the life expectancy at birth. Beneath them is a complex web of relational processes, in which men and women interact permanently and exercise their reproductive capacity, especially their capacity to renew themselves and regenerate the human species, through bearing and rearing children. Such relations comprise a set of mechanisms, which are determined by at least two important universal factors: the sexual nature of reproduction and the irreversibility of time as manifested in the aging process between birth and death.

It is generally perceived, though not always well understood and explained, that a population can be in danger of perishing in two extreme situations: on the one hand, when there is a sustained decline leading it to disappear partly or entirely, which can happen because of catastrophic events, such as epidemics, wars and natural disasters, or the population's own inability to bring its offspring to their reproductive age; on the other hand, the existence of a sustained and rapid increase in people beyond the capacity of the institutional and social organization and the available resources.

Even though during the twentieth century the problem of population, reproduction and development has come to mean the problem of population control, as Margaret Mead wrote almost half a century ago:

> Every human society is faced not with one population problem, but with two: how to beget and rear enough children, and how not to beget and rear too many ... When fecundity threatens vigour, social pressures against childbearing may become apparent... And at the primitive level, as in our complex modern societies, there is also the fear that the reproduction rate will fall so low that the society will die out (Mead, 1967: 210-211).

While throughout history some populations have disappeared entirely, others have continuously increased and expanded their numbers and boundaries. In any case, population dynamics depends mainly on its ability to handle the self-organizing mechanisms, which determine the outcomes of the main demographic processes: deaths (mortality), births (fertility) and migrations (territorial movement). Such mechanisms are essentially manifested at two levels: in the conservative and dissipative relations within the structure and organization of the total population system, and in the adaptive and transformation relations established by a given population with other populations and the environment in which is living.

A summary table of the demographic design of population composition

Table 2.8.3 summarizes the discussed of demographic design and population composition in this chapter. Seven characteristics have been discussed above, if not extensively at least in a way sufficient to move towards a more detailed characterization of the three-dimensional set of principles, and association with their conceptual approach and methodological frameworks.

Table 2.8.3 The demographic design of population composition

Characteristics	Reference frame	Variables
1. Composition	The total demographic system comprises two subsystems, structure and organization. Such a system exists in a space-time context and interacts with its environment.	Sex ratio Rate of population growth
2. Structure	Sex and age	Sex, age Population pyramid, Demographic output
3. Organization	Gender and generation	Nuptiality, family formation, family life-cycle, demographic outcome
4. Functioning	Change and evolution.	Growth and evolution, the entropy of life table and ergodicity
5. Demographic goals	Reproduction and survivorship.	Reproductive rates, fertility rates, life expectancy
6. Internal state	(i) Equilibrium-stagnated (ii) Near equilibrium, and (iii) Non-equilibrium	Intrinsic rates, stable and stationary population
7. Relation with the environment	(i) Isolated or without any type of exchange with its environment; (ii) Open to and dependent on external factors in which growth is possible; (iii) Open but in continuous balanced exchange.	Closed population population vis-à-vis environment

9.

Neuter demography and demographic outputs

> We are continually hearing declamations against theory and theorists, by men who pride themselves upon the distinction of being practical. It must be acknowledged that bad theories are very bad things, and the authors of them useless and sometimes pernicious members of society. But these advocates of practices do not seem to be aware that they themselves very often come under this description, and that a great part of them may be classed among the most mischievous theorists of their time. When a man faithfully relates any facts which have come within the scope of his own observation, however confined it may have been, he undoubtedly adds to the sum of general knowledge, and confers a benefit on society. But when, from this confined experience, from the management of his own little farm, or the details of the workhouse in his neighbourhood, he draws a general inference, as is very frequently the case, he then at once erects himself into a theorist (Malthus, 1817/1989: 185).

The principle of differentiation and identity among individuals

However evident the concept of population may at first glance appear, demographers cannot explain what the reality of population is independent of theory: that is, without a body of analysis that makes it possible to create, interpret, describe and explain a certain class of observations, as well as to generalize from them and predict new observations.

I have already used the term ‘neuter demography’ several times in this thesis, in association with the three-dimensional set of principles regarding the study of population composition discussed in Chapter 8: differentiation-separation-complementarity. By neuter demography I mean specifically the analytical body of demography comprising concepts, measures, methods and theories which in their fundamental way abstract from individual traits relevant to demographic analysis such as sex, age and marital status. That is, neuter demography includes both the conceptualization of demographic phenomena, as well as methodologies and measures consistent with them. The common feature in such an analytical body of demography is the principle I call it here the ‘principle of differentiation among individuals’.

Whatever similarities may exist among people, individuals in a population can all be regarded as different in behaviour and physical appearance. People are equally humans and it is on the basis of this neuter human nature that each single member in a given area is counted, for instance, in a national census. Basic textbooks apply this reasoning to explain to students the difference between the term 'population' in demography and statistics.

> In demography the term relates to the number of people in a given area, while in statistical usage, particularly when talking about sampling, it means the universe of units under consideration, which may be people, light bulbs, rats or whatever. The demographic usage is thus very similar to normal English usage (Newell, 1988: 9)

Following this explanation, Newell admitted that the phrase ‘number of people in a given area’ is too vague for demographic purposes and indicated, as others used to do, two broad types of 'population counts’, *de facto* and *de jure*; the former for the population actually present at some moment, and the latter to mean the population belonging to a certain area. Not surprisingly, demographers usually are silent with regard to the implications of the neuter subtleties associated with terms like ‘population’, ‘total persons’, ‘usual residents’ and ‘people in a given area’.

Of course, the idea that the neuter approach relies on the principle of differentiation among individuals may appear contradictory at first glance. This occurs because individuals are all taken in consideration as independent entities as if this was because they were identical rather than all different. So the aggregate measure results on the surface suggest that they represent identical and undifferentiated features. This is the reason several theories have provoked contempt and much criticism when they deal with population, at the macro-level, and family and households, at the micro-level, as if individuals were impersonal and undifferentiated entities. A familiar example to demographers, as well as other social scientists, is the 'Beckerian family’; one of the variants proposed by Becker with Tomes, in 1979, even concluded that children may have the same utility function as their parents when they are assumed to be produced without mating, or asexually (Becker and Tomes, 1979).

The apparent contradiction between the two characteristics of the neuter approach of population, that is the hidden differentiation and the visible identity in the aggregate measure, is a feature that can easily become clear if one understands that population reality can never be studied scientifically independent of theory. However fancy and unrealistic the concept ‘neuter’ will appear for some scholars, the neuter approach in as useful in theory construction as the ‘neuter’ gender is found practical in the ordinary language of communication. [1]

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[1] Curiously, recent developments in evolutionary biology are helping to clarify why human individuals are all different to the extent that one can speak of an undifferentiated and universal 'human nature'. I will not discuss further this matter here, but the following sentence from Ridley should help the reader to grasp how the neuter approach is as much determined by the sexual nature of population as the one-sex and the two-sex approaches.

> In behaviour, as in appearance, every human individual is unique. How can this be? How can there be a universal, species-specific human nature when every human being is unique? The solution to

As I have already explained in Chapter 8, the validity of a neuter approach in the theory of knowledge has more to do with the nature of the issues under consideration and the methods associated with them than with the reality of families and population themselves. Thus, while individuals are brought together to constitute a single aggregate as long as they share a common feature, it is mainly because they can be singled out as independent entities that each individual is counted individually.

Population without age, sex and marriage

As Keyfitz (1977b: viii) wrote in his classical book *Applied Mathematical Demography*, 'The art of theory construction is to start with simple assumptions and then to introduce greater realism, which means more complexity, as required'. There may be nothing so simple in demography as the unspoken assumption lying beneath the neuter concept of population: all individuals counted in a given area are human beings, regardless of their individual differences by sex, age, and race.

The principle of differentiation is a sensible abstraction which has proved to be very useful insofar as it actually picks out what is common among individuals in a population, fixes it, and consequently spares repetition. Demographers pick out the feature that individuals share the same human nature, that is all individuals are human beings, and then they calculate, for instance, the size and the growth of population. These measures can be calculated reasonably well, and without introducing the greater realism implicit in Keyfitz’s quotation above, namely the complexity originated by two-sex, or even one-sex, methods.

Even though the designation 'neuter demography' is meant to refer to situations in which the sexual nature of reproduction is taken into consideration, I also use it in reference to situations in which populations are treated as ageless. As to the disregard of age, Keyfitz called it 'population without age':

> Abstraction is necessary in demographic as in other theory; is it possible to abstract even from age and still obtain results of value? To represent a population as a number varying in time, and in disregard even of its age composition, is like treating the earth as a point in space - though too abstract for most purposes, it is useful for some (Keyfitz, 1977b:1).

Keyfitz did not expand and generalize his remarks to other individual traits, but he could as well speak of 'population without sex' or 'population without mating'. In another book, Keyfitz with Smith commented: 'Even John Graunt believed that the right way to describe the dynamics of a population was the ratio of births to deaths without considering age' (Smith and Keyfitz, 1977: VII). Curiously, this is what contemporary demographers

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this paradox lies in the process known as sex. For it is sex that mixes together the genes of two people and discards half of the mixture, so ensuring that no child is exactly like either of its parents. And it is also sex that causes all genes to be contributed eventually to the pool of the whole species by such mixing. Sex causes the differences between individuals but ensures that those differences never diverge far from a golden mean for the whole species (Ridley, 1993: 11-12).

presume when they apply equations and models without considering age. This, among other factors already discussed, shows that the model of demography's whole design set by Graunt is remarkably powerful today, to the extent that many demographers still believe they should be committed only to the naive seeing and the dispassionate noting and accounting inherited from Graunt's *Observations*. [2]

The 'Malthusian population': Lotka's formalization of the neuter population model

The idea that the patterns in population that can be drawn from empirical observations are determined always by a given theoretical framework is indispensable for overcoming certain misconceptions about some concepts and theories very familiar to demographers, including the notion of Malthusian population, the neuter stable population theory, the idea of demographic transition, and specific theories on family.

From a neuter perspective, the formalization of population through mathematical and geometrical representations stands on the principle of differentiation among individuals discussed in the previous section. Regardless of the age, sex, race and other traits, individuals are aggregated as 'persons', and so the study of population turns into a collection of just two pairs of entities: the number of members and time defined in deterministic or stochastic terms.

Following the sketch of the abstract demographic system outlined in Chapter 8, the neuter model can be represented by the set of pairs $\{\vec{a}_i;t\}$, where $\vec{a}_i$ denotes a given individual or person and *t* denotes time, and hence: $S_d = \{\vec{a}_i;t\}$.

As well, in Chapter 6, I referred to Lotka's work as the culmination, rather than the onset of demographic theorization on fertility. After remaining dormant for about 150 years, Lotka resumed Euler's concept of stable age structure and brought mortality and fertility together into a single and elegant unified model.

> Euler' s virtually unknown article, published ... in 1760, anticipates important parts of modern stable population theory for a one-sex population closed to migration .... Euler uses 'hommes' for the population and 'enfants' for births, which could be interpreted as meaning that the sexes were combined in his model ... Essentially a one-sex model seems to be intended, with males as the illustration (Smith and Keyfitz, 1977: 84-5).

During the eight years following Lotka's arrival in the United States in 1902, he published his first papers on the mathematical theory of self-renewal and evolutionary processes, including his first seminal paper in demography. 'To Dr. Lotka's work', so Notestein (1950: 23) summed up a few months after his death in December 5, 1949:

[2] As I will stress in Chapter 10, Graunt's intuition for a description of the dynamics of a population without considering age seems to have been vindicated convincingly in Feeney's (1983) model of 'Population dynamics based on birth intervals and parity progression'.

> the field of demography owes virtually its entire central core of analytical development. In 1907 he showed that a closed population with a fixed age distribution and fixed mortality increases in geometric progression with time. In 1911, with F.R. Sharpe, he demonstrated that a closed population submitted to fixed schedules of birth and death would develop a stable age distribution with a characteristic rate of increase. In 1925, in an article with Louis I. Dublin, 'On the true rate of natural increase', Lotka showed for the first time the method of computing the stable age distribution and the 'true' rate of natural increase ... He went on to the age distributions of populations growing according to the logistic law, and to many studies of self-renewing aggregates. Much of this work was summarized in the *Théorie Analytique des Associations Biologiques.* In his last months he was busily engaged in the preparation in the preparation of a systematic English edition of his demographic work, which was unfortunately left incomplete by his final illness (Notestein, 1950: 23).

A great deal of the work described in this brief overview from Notestein is even more appropriate to characterize Lotka's seminal contribution to the one-sex formal demography discussed in Chapter 10. But his first contribution in demography was relevant first of all to the neuter approach. In a two-page article published in 1907 in the still-existing journal called *Science*, Lotka made two important contributions: first, he set up the basic neuter mathematical model of stable population; and secondly, he showed the way to test empirically stable theory, as is illustrated in Table 2.9.1, by comparing his calculated results with the empirical observations for England and Wales in 1880s.[3]

Later, in his *Théorie Analytique* Lotka (1939) systematized his work since 1907 and placed the neuter population model in the context of what he called the 'Malthusian population'. By 'Malthusian population' Lotka meant a population under the regime of a constant life table *p(a)*, a constant age distribution *c(a)*, the number in the population and the number of births increase or decrease according to Malthus's law, the law of compound interest (Lotka, 1939: 17); he proposed the following deterministic first-order differential equation: $\frac{dN}{dt}=B-D$

where *N* denoted population size, *t* denoted time and *r* denoted the birth rate. These are among the most aggregated demographic results.

[3] Lotka (1907: 94) presented a mathematical expression for the relation between the crude birth rate, the crude death rate, and age distribution in a population 'where the general conditions of the community are constant, and the influence of emigration and immigration is negligible'; he considered c(a) as the coefficient that out of the total number N_t of individuals in the community at time *t*, the number whose age lies between the values *a* and *(a+da)* given by $N_t c(a)da$, and hence he proposed $c(a)=\frac{B_{(t-a)}}{N_t}p(a)$, where $B_{(t-a)}$ denoted the total birth rate at time *(t-a)*, and *p(a)* the probability at its birth, that any individual reaches age *a*, the number of the survivors of the individuals born in time *da* is $B_{(t-a)}p(a)da$. Under constant general conditions in a given community, Lotka demonstrated both population and births will increase in geometric progression with time at a constant growth rate. In two additional equations, Lotka represented the fixed age-distribution and the relationships between crude vital rates.

Table 2.9.1 England and Wales 1871 -80 (Mean)		
$a_1 a_2$	$1000\int_{a_1}^{a_2} c(a)da$	
	Observed	Calculated
0-5	136	138
5-10	120	116
10-15	107	106
15-20	97	97
20-25	89	87
25-35	147	148
35-45	113	116
45-55	86	87
55-65	59	59
65-75	33	33
75-∞	13	13
England and Wales 1871 -80 (Mean)	Observed	Calculated
Birth-rate head b	0.03546	0.0352
Death-rate head d	0.02139	0.0211
Excess (b-d) = r	0.01407	(0.0141)
p(a) from Supplement to 45th Ann. Reg. Births, etc., England and Wales. vii and viii, assuming ratio: $\frac{\text{male births}}{\text{feamle births}} = 1.04$		
Source: Lotka, 1907: 22		

Karmel (1948c), in his unpublished Ph.D thesis, referred to the neuter model of population when he introduced the debate on whether fertility and mortality conditions should be dealt with either through one-sex or two-sex models: 'We will avoid this difficulty at this stage by referring simply to the "individual members" of the population, assuming that an individual member is capable of reproduction on his own' (Karmel, 1948c: 2). In 1964, Ryder wrote explicitly about the basic population model as a 'non-sexual or monosexual', and stressed that such a model is completed by linking together three kinds of functions:

> The first of these is the number of person-years of exposure of the population within each age interval and time interval; the second is the number of births occurring within each age interval and time interval per person-year of exposure; the third is the number of deaths occurring within each age interval and time interval per person-year of exposure. In brief, they are the age-time structure of the population, and the age-time processes of fertility and mortality. These three dimensions represent a network of identities within a complete deterministic model. The formal theory of demography is concerned with working out the logico-mathematical relationships among these components and elaborating schemes for their analysis in terms suggested by the structure of the model ... From the standpoint of population size as a function of time, the age of the individual at death is irrelevant. Only the fact and time of death enter the accounting procedure, since the question of which individual dies does not affect the size of the population (Ryder, 1964: 449).

This brief review should be enough to make it clear that the neuter analytical body of formal demographic theory, far from being an abnormal caricature of population, constitutes a sensible and widely used first approximation in the scientific study of demographic reality. In particular, formal neuter demography relies heavily on some of the most elementary neuter demographic concepts and measures taught in basic contemporary courses of demography: the crude death rate (CDR), the crude birth rates (CBR), and the basic demographic equation: ‘Population change = Natural increase + Net migration’ (Newell, 1988: 8; Tapinos, 1985: 24). The fact that the CBR and the CDR relate their respective events in the numerator to both sexes of all ages in the denominator has led demographers to call them ‘crude rates’; but this notion of crudeness is just an euphemism for neuter, or sexless and ageless.

Demographic transition: the ABC typology of the demographic system

Since Thompson’s famous article of 1929, the framework of demographic transition theory has represented for 'population studies' what Lotka's stable theory sketched in 1907 has represented for ‘formal demography’: the culmination of a long process of convergence between fertility and mortality into a stylized and unified model.[4] Of course, the stable population theory has evolved far beyond demographic transition in terms of its formalization, which has involved the development of increasingly sophisticated one-sex and two-sex models and measures. The idea of demographic transition has also provided inspiration for the development of ingenuous one-sex methods and indicators, such as the famous Coale’s Indices (Coale, 1973). But however such an idea is used and interpreted, the concept of ‘demographic transition’ has achieved fame and remained intellectually powerful in contemporary demography relying basically on ordinary and graphical languages. This difference between the two models should not be seen as a shortcoming for demographic transition theory. On the contrary, I find it hard to image that a sophisticated model as it is stable population theory could ever become as handy in creating the interdisciplinary networking that demographic transition has established among professional demographers, professionals of other social sciences, and policy-makers. Regardless of its language, the framework of demographic transition can be used, just as Lotka's neuter stable theory,

> to show that important scientific concepts are not created by definition; they develop out of discoveries in a given science and grow by virtue of two processes: the operations necessary to realize the concept and the function it performs in the explanatory process (Vance, 1959: 295).

Like stable population theory, demographic transition emerged after a long process in the development of demographic theory which can be traced at least as far back as the

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[4] Hodgson (1983), Kirk (1995: 3-6), and Szreter (1993: 663, 694) have gathered enough evidence showing that Notestein and Davis were anticipated not only by Thompson, but by several other authors who in different countries developed the same idea of demographic transition as Thompson.

research tradition set out by Malthus's *Principle of Population.* Both the stable population and demographic transition theories have converted, through their own means, the Malthusian vision of a population design for a purpose into two very elegant models. No wonder that the former usually relies on the best of mathematical sophistication, while the latter uses predominantly ordinary language and graphical representations. What is important is that both models have gone far beyond a simple representation of order, patterns and regularity in the data inherited from Graunt. This does mean that the innocent approach sketched by the *Observations* has been or will ever be thrown away. Graunt’s conceptualization of population as a statistical and observable aggregate was a scientific discovery too important to demography and all social sciences ; it is hard to imagine that will ever be abandoned.

Yet Malthus’s principle of population indicated the way to dig deeper into the order, structure, patterns and regularities of data and search for powerful forces which hold individuals together as an relatively harmonious organism: the power of survivorship and the power of reproduction. Thompson’s ABC typology of demographic transition theory is clearly a by-products of Malthus’s principle of population, on the one hand, and its formalization in what Lotka designated the Malthusian population.

Contrary to the conventional perception about the origin of the ‘idea’ of demographic transition, Thompson’s article published in 1929 was much more theoretical than may appear at first glance. The article shows clearly that Thompson would be the last to entertain the illusion that he deluded Einstein’s epistemological proposition quoted in Chapter 6. Burch, in a recent return to Caldwell’s famous paper published in 1976 mentioned another reason why the theoretical level of Thompson’s ideas may still be dismissed: ‘Caldwell does not consider Thompson’s ideas theoretical, on the ground that they contain no attempt at *explaining* changes in fertility and mortality patterns’. Unfortunately, Burch handed over his remark about Caldwell’s remark to a footnote, among other reasons because he was concerned with the lack of sufficient theoretical sophistication even among the authors that after Thompson have contributed to the development of the idea of demographic transition.

At the beginning of Chapter 8, I have argued the need to link the descriptive power of demography with a set of research issues mainly of the type 'What has happened?' and 'How many?'. In turn, I have asserted that the explanatory purpose of demography needs to be set around questions of the type ‘How?’ and 'Why?'. This distinction is to stress the important implications that the difference between the descriptive and explanatory purposes of demography should have in conceptual and methodological terms. As Cook et al. put it, ‘For most epistemologists, explanation entails identifying the total set of contingencies on which an event or relationship depends (e.g. Bhaskar 1994; Mackie, 1994)’ (Cook et al., 1994: 18).

Few would ever question that Caldwell has been among the most active contemporary authors searching for the mechanisms and causation of fertility change. But it is doubtful that Notestein (1945), Davis (1945) or more recent authors have gone far enough in their attempt to transform the idea of demographic transition into an explanatory theory sufficiently developed to render Thompson's ideas to pure conjectures and plumbing. Despite the advances brought about to the idea of demographic transition by Davis (1945), Notestein (1945) and more recent demographers, demographic transition remained predominantly descriptive not for want of its forerunners but because of the nature of the concepts, measures and methods. As Szreter (1993: 659) and Kirk (1995: 2) remarked, depending on the author, or indeed the same author at different times, the demographic transition has been called an 'idea', a historical model, a predictive model, a theory, or a mere descriptive term. What these authors have not made clear, though, is that such a wide range of attributes are in their own way part of the broader goal of descriptive demography. Demographers have a great anxiety towards explanation, but one cannot find any substantial relevant attempt in demographic literature showing that more recent contributions to demographic transition have become more explanatory than the one provided by Thompson. Certainly, no serious attempt seems to have been made to show how more recent versions of demographic transition differ from the earlier in the way proposed, for instance, by Cook et al. (1994: 18) on the basis of Collingwood's (1994) important distinction about how explanation differs when the phenomena under analysis are historical events, manipulable events, or 'scientific processes': explanation through manipulable agents, explanation through 'scientific processes, explanation through prediction of outcome variability (Cook et al., 1994: 18-34).

It seems important to stress that the distinction between descriptive and explanatory purposes in demography should never be tread as a dichotomy, and an all-or-nothing opposition. The two levels of scientific cognition reflect different levels of theoretical knowledge and, not least important, in demography should lead to substantially different methodological approaches and research strategies. After all, just as the categories 'sex' and 'gender' refer to two sets of aggregated characteristics closely associated with one another, it would be meaningless to establish a rigid dividing line or strict separation between descriptive and explanatory demographic research questions.

In this context, ever since Thompson the idea of demographic transition has become relevant for providing an abstract model first of all to study what has happened in a given population in terms of its two main components affecting demographic change: mortality and fertility, or taken together population growth. Thompson's classical historical description of what has happened to the world's population in terms of its birth and death rates fostered the famous threefold typology similar to the picture in Figure 2.9.1 (Davis, 1945: 1-11; Keyfitz, 1977b: 24; Kirk, 1995: 2-6; McNamara, 1982: 146-147; Notestein, 1945: 36-57; Thompson, 1929: 959-975).[5]

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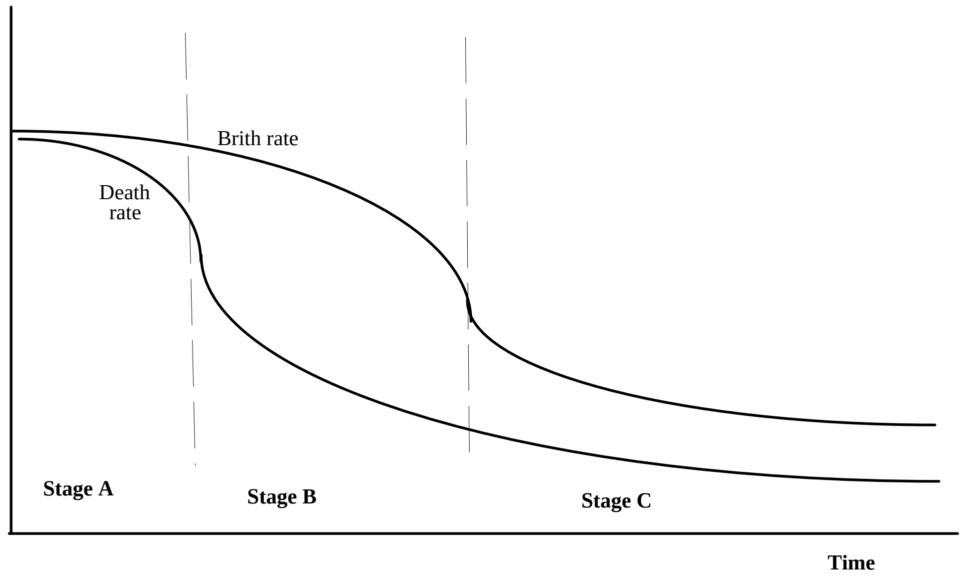
[5] In Figure 2.9.1, I use the letters A, B, C in the reversed way used by Thompson. He considered that countries found in *Group A* had a very rapidly declining birth rate and although their death rates were low their rates of natural increase were declining and they were rapidly approaching a stationary or decreasing

Figure 2.9.1 The ABC-typology of the demographic system



Thompson used the first three letters of the alphabet, rather than the numbers 1-2-3 used by Keyfitz (1977: 24) and McNamara (1982: 146), to characterize the three types of countries in the world with regard to their population growth. There is a certain irony in Thompson's usage of the first three letters of the alphabet to designate the three types of countries. As in other situations the expression 'The ABC of something' is generally used to mean the starting point, and the demographic transition theory can be regarded as the ABC abstract model of the demographic system; this is what I intend to stress with the title of Figure 2.9.1.

However abstract and hypothetical the classical threefold typology turns out to be whenever applied to particular cases, I see no reason why the theoretical relevance of the classical abstract should be dismissed as totally 'unnecessary' and 'inappropriate', to use Szreter's (1993: 692) own terms. The three types of countries, which Thompson claimed to have identified in the world's population of 1929, have become increasingly more, rather than less, relevant to demographic analysis particular after Notestein (1945) and Davis (1945). Such relevance is theoretical and, curiously, despite the fact that in empirical terms the model has found little support. However irritating and confusing this may appear, the reason is mainly because authors still entertain strong expectations about the theoretical tools they take for granted. The alleged contradiction between the theoretical and empirical relevance is due to the Kuhnian paradox, that is that the 'clash of paradigm' eventually will

population because of the general practice of contraception. *Group B* countries, Thompson asserted, were those in which birth rates were coming under control, but rather slowly. Death rates were declining more rapidly than birth rates, however, so that natural increase was rising or at least was not declining to any great extent. *Group C* comprised countries where both birth rates and death rates were subject to little voluntary control as yet and the positive checks determined the growth of population (Thompson, 1929: 959).

result in the disappearance of the old. Moreover, as I will show in a moment, there have been several justifications of the remarkable success of demographic transition, but before turning to them it seems useful to state the one that seems more consistent with this thesis. I consider that demographic transition has become relevant to demographic theorization, and despite the fact that in many particular cases the observation data have shown patterns substantially different from the expectations, having been transformed into a model of the demographic system in its 'laboratory conditions'. In this sense, demographic transition seems as much relevant to demographers as the equilibrium model of supply and demand is for economists, or as Burch (1995: 17-18) noticed recently, the abstract laws of falling bodies set by Newton continue to be relevant to physicists.

The suggestion that Thompson's initial formulation of demographic transition was too primitive to deserve being regarded as theoretical cannot be supported by the spirit of this thesis. Such a view would contradict the sense of history that I have already given to this discussion. Indeed, it would be more or less as if Thompson was not aware of the development of demography during more than two centuries and half and had published a paper based on an analysis similar to Graunt's analysis of his 'Bills of mortality'.

Nothing could be more misleading, distorted and unfair to Thompson's contribution to twentieth-century demography. The title of his paper published in 1929 was as synthetic as no title in demographic literature has ever been: simply 'Population'. But just like Lotka's two-page paper of 1907, Thompson's article stands on the long history of demographic knowledge. It is hard to imagine that it could ever be written, say, in the time of Malthus; the reason is not simply because in Malthus's time there was less data but also because of the lack of Lotka's theory. This assertion may be surprising, just as should my insistence on Lotka's stable theory in the context of a debate on the idea of demographic transition. Such a surprise can be expected because of the way contemporary debates upon the origin and development of the idea of demographic transition give the impression that the idea of demographic transition theory had little to do with Lotka's mathematical stable population theory. However, if one reads Thompson's own 'Population' there is no doubt that this article stands theoretically on three main authors: Malthus's theory of population, Dublin and Lotka's (1925) crucial paper, 'On the true rate of natural increase', and Sauvy's article 'La population française jusqu'en 1956'. The artificial dichotomy between 'formal demography' and 'population studies' has certainly emerged latter. Indeed, this separation has become so pervasive that Newell (1988: 10) felt it to be so unconventional to even speak of 'demographic transition theory' in a textbook on formal demography that he found it convenient to justify himself: 'Although a study of the demographic transition is no part of formal demography', so he remarked before discussing it briefly.

After reading Thompson's 'Population' it is hard to imagine that its author would ever accept the putative gap between formal demography, namely Lotka's theory, and demographic transition. Thompson's ABC typology of three types of countries in the world was based on the same population growth used by Lotka, a measure which Graunt never imagine would one day be. Empiricist or pragmatic though Thompson’s study may appear to be, it was based on Lotka’s logical framework sketched to deal mathematically with what he called ‘Malthusian population’. Thompson was hardly short of generalizations about the three types of countries. In his comparison of C with A and B countries, Thompson remarked: 'we are fully justified in assuming that in the former neither births or deaths have come under voluntary control to anything like the same extent that they have in the latter'. Besides his references in the abstract to the Malthusian term ‘positive checks’, one of the most relevant reference to Malthusian condition is the following:

> As a consequence of this relative lack of voluntary control over births and deaths, it appears that the growth of these Group C peoples, who constitute about 70 to 75 per cent of the population of the world, will, in the near future, be determined largely by the opportunities they have to increase their means of subsistence. Malthus described their processes of growth quite accurately when he said 'that population does invariably increase, where there are means of subsistence ...'. The differences in the means of subsistence are undoubtedly at the base of the differences in the rates of growth of the three countries for which data are given (Thompson, 1929: 971).

The above emphasis on Thompson is in no way intended to dismiss the significance of Notestein and Davis or any other author to the development of the theory of demographic transition. There is more theory behind the idea of demographic transition than most of its critics and supporters admit. The idea of demographic transition represents the culmination and unification of a long process of convergence between mortality and fertility into a single elegant model.

In short, as Malthus asserted (see the quotation at the beginning of this chapter), when from any confined experience researchers draw a general inference, they then at once erect themselves into theorists (Malthus, 1817/1989: 185). Thompson did not claim to have sketched an original theoretical model; perhaps because he believed that was simply testing a theory that was more than a century old. If this is right, Thompson can not be placed among those who Chalmers called ‘naive empiricists’. Instead, he can be seen as a ‘falsificationist’ who 'freely admits that observation is guided by and presupposes theory' (Chalmers, 1988: 38). In the end, Thompson’s ‘Population’ represented one of first most comprehensive tests of Malthus's theory of population, not so much because of the data but mainly because he used the best demographic theorization that was available at the time: that is, the stable population theory developed by Lotka and his co-authors.

Demographic transition theory: ‘As theory, it remains contested territory’

The following quotation from McNicoll summarizes the diversity of insights that the demographic transition theory has inspired for about half a century.

> Demographic transition, as historical description, is the single major 'stylized fact' that demography can claim to. As theory, it remains contested territory. Some demographers, ignoring what little other social theory they have to retreat to, cavalierly dismiss 'the theory' - as if it were one thing and disproven. For others it is

> one thing and proven - usually a simple model in which a regime of 'natural fertility', with its arcadian innocence of birth control, is somehow converted to one of cumulating demographic response to economic growth, societal modernization, and government program measures. Still others favor more elaborate models in which demographic behavior is tied to various institutional or cultural features of the society (family structure, for instance) responding to shifts in real or expected economic conditions or in less tangible cultural configurations. Or, relaxing the assumption of a demographic 'system', there can be separate fertility, health, family, even migration transitions, each with its own theory (McNicoll, 1992: 404).

There is an interesting feature in this snapshot of demographic transition theory. Just as with Malthus, McNicoll appears to see the demographic transition mainly as a source of diverse theoretical developments. If this interpretation is correct, his own words about Malthus's demographic work, quoted in the epigraph of Chapter 5, can be used to characterize demographic transition theory as well: forward from demographic transition theory does not single out a particular direction, not even perhaps a single quadrant.

McNicoll's hesitation to judge demographic transition theory as a theoretical failure is almost unconventional; because of this, his overview presented above has been received with some scorn by those who find demographic transition a distressing embarrassment in twentieth-century demography (Burch, 1994, 1995; Greenhalgh, 1994: 4; Greenhalgh and Li, 1995: 603; Larson, 1994: 1; Newell, 1988: 10-12; Simmons, 1988: 92; Szreter, 1993).

One aspect of McNicoll's statement can be used to demonstrate the usefulness of the three-dimensional framework outlined in this thesis: 'As theory, it remains contested territory'. Although McNicoll's overview of diverse theoretical developments illustrate the ideas of 'contested territory', he did not explain why there is such a contrast with regard to demographic transition seen as the single major 'stylized fact' of historical description. Is it because the professionals of demography and other scholars have simply agreed to disagree? To what extent can the conjecture about 'lack of anything better to replace it' (Newell, 1988: 11) be taken seriously?

The key to these and other questions seems to be in the double standards applied to different aspects of the neuter content of demography. If one can determine where the validity of neuter demography begins and ends, the particular issue of demographic transition remaining 'contested territory' will most probably fade away. Then, one can expect that demographic transition theory will be used, just as the crude vital rates are still used, without motive for embarrassment.

Double-standards about the liabilities of neuter demography

Basic demographic measures such as birth and death rates are generally portrayed as crude measures because they are insensitive to the effects of population structure. As Keyfitz (1980: 47) put it, 'they pertain to the surface of population phenomena, to appearance rather than to demographic reality'. Keyfitz borrowed this explanation from

Marx, who insisted for social life in general, that relations below the surface can be quite different. 'If the world was seen by common sense there would be no need for science', Marx wrote (see Keyfitz, 1980: 47). This remark is as much relevant to science, in general, as it is to demographic science, in particular. Demography has created a space of its own in social sciences as it has accumulated a new knowledge on how to see beneath the surface of demographic phenomena and search for deeper relationships; in particular, how commonsense and even crude measures can become very misleading and what alternatives exist. Part of the historical process behind this view has been illustrated in previous chapters concerned especially with the development of the concept of fertility.

Perhaps the best way to understand the double standards applied to different aspects of the content of neuter demography is to go through a specific example; standard teaching textbooks seem to be a good place to find an example which depicts how basic demographic measures of neuter nature are taught and judged differently from demographic transition theory. Although current textbooks do not enter into historical details, the way they describe the limitations as well as the usefulness of crude vital rates is generally accurate.

First, students and readers in general are taught the definition and procedures to calculate crude birth and death rates. Second, they are advised about the limitations of crude measures. 'The reason it is a "crude" rate', one can read in Newell's textbook about the birth rate,

> is that it includes all ages and both sexes in the denominator ... A major problem with it is that it is affected very greatly by the composition of the population as regards age, sex and other characteristics. Thus it can be very misleading if it is used for comparing different populations, or the same population at different times because they may vary greatly in their composition (Newell, 1988: 37-38).

Or, with regard to crude death rate: 'Although it is easily computed and understood, it is generally a poor measure of mortality as it does not take age structure into account' (Newell, 1988: 33). Finally, the reader is also informed about the usefulness of ‘crude’, For instance, about the crude birth rate Newell's manual explains:

> There are, though, three reasons why it is a useful measure. First, it is easy to understand. Second, it requires few data and is easy to understand. All that is required are the total number of births and the total population. Third, it is possible to subtract the Crude Death Rate from the CBR to get the Crude Rate of Natural Increase, which, along with the Crude Net Migration Rate ... determines the population growth rate (Newell, 1988: 38).

Before turning attention to how demographic transition is exposed to students, it is useful to draw the moral from the way ‘crude rates’ are handle in demographic teaching. Students and readers in general can learn, on the one hand, the definition, the calculation procedures, some applications and examples; on the other hand, they also get to know about the limitations and what to expect or not to expect from them. In other words, students are informed, rather than alerted or even advised, that they should neither create great expectations around crude measures, nor simple throw them away altogether. It is widely agreed that the limitations of crude rates are many, but despite that they continue useful enough to the extent that they are still the first to be introduced in any basic course of demography. Without them it would be rather more difficult to understand the need and usefulness of more sophisticated measures such as total fertility rate, parity progression rate, and net reproduction rate.

How is demographic transition theory introduced to students of demography and readers in general? At most, textbooks on formal demography mention the term ‘demographic transition’ not more than one or two times (Keyfitz, 1968: 74; 1977b: 23-24; Lucas, 1994a: 22-24; Namboodiri, 1991: 156). The reason for this is that demographic transition is not considered mathematical enough to be called formal and thus the usefulness of its framework is generally restricted to 'population studies'.

Newell’s (1988) textbook used above to exemplify the way ‘crude rates’ are taught provides one of the rare exceptions in which demographic transition is introduced at the beginning of a manual of formal demography. Although the author has excused himself for being unconventional, he justification his decision correctly:

> Although a study of the demographic transition is not part of formal demography, it is nevertheless useful to discuss it briefly here, first, because it helps to set the scene for the following chapters, and second because it emphasises the strong interrelationships between population structure, fertility, mortality and migration which lie at the heart of formal demography as defined earlier (Newell, 1988: 10)

These two reasons alone seem important enough for a much better appreciation of demographic transition theory in standard demographic teaching, including in formal demography. But even Newell, before explaining what demographic transition theory is all about, found it necessary to advise the reader about the 'contested territory':

> The so-called Theory of the Demographic Transition was first described in the 1940s. Since then it has been attacked, modified, added to and rewritten many times until now there is no single, clear, generally accepted view of precisely what it comprises (Newell, 1988: 10).

Notice that in case of crude rates lecturer find no reason to introduce this preliminary excuses. The problem, thus, is that after such an ‘invitation card’ it is hard to imagine that students will really try to give any more credit to demographic transition beyond the one expected in the previous sentence. Newell then provided a brief idea of the classical view demographic transition and turn once more to the disputes around the theory. Of course, authors are wise enough to anticipate some of the questions occurring to readers, such as this: ‘If demographic transition theory is as irrelevant as it appears why do you bother to even mention it here anyway?’ The answer to questions like is anticipated by Newell's (1988: 11) as follows: ‘Despite these criticisms, Demographic Transition Theory remains at the heart of demography as an academic discipline, perhaps partly because of the lack of anything better to replace it’. I really cannot imagine any other answer more self-defeating and misleading than this.

In any case, however controversial, modified and rewritten demographic transition theory has been in the past half century, it is interesting to notice that there are some aspects which are generally little disputed. First, regardless of how Thompson's (1929) contribution is rated comparatively with that of Notestein (1945) and Davis (1945), these three authors are widely recognized as the forerunners of demographic transition. Second, sooner or later authors accept it, as McNicoll put it, 'as historical description ... the single major "stylized fact" that demography can claim to'. Time and again, authors still come with new illustrations of this feature; a recent example is Livi-Bacci's (1992) *A Concise History of World Population*, which provides a short but illuminating application of demographic transition as historical description. Third, the neuter nature of demographic transition theory continues to be taken so much for granted that it is still not a motive for debate in the literature. Fourth, there is no dispute that demographic transition was born framed by the crudest concepts and measures in demography: birth rates, death rates, and population growth.

These four features should be enough to expect that the potentials of demographic transition theory are heavily determined by its theoretical framework; as Pagels put it,

> One cannot use a system that is specified by a certain amount of information to prove something about a system with a relatively larger amount of information. In mathematics one never gets out more information than one puts in the first place from the starting axioms and rules. It is like expecting a mouse to swallow an elephant (Pagels, 1989: 60).

Much of the 'contested territory' of demographic transition as theory seems to have grown exactly from the expectation caricatured by Pagels. That is, if a mouse could swallow an elephant, perhaps a descriptive model should also be expected to provide much explanatory power.

In addition to the previous picture, it seems useful to provide some specific examples which illustrate how disillusion and cynicism still run high around demographic transition. Some authors seem convinced that demographic transition theory has failed because of its 'ordinary language' and lack of sufficient mathematical formalization. For instance, recently Burch (1994: 2) has reacted with a sense of guilt both to McNicoll's overview quoted above and to Caldwell's remark made in 1976: 'Modern demographic transition theory was born almost in mature form in a paper written by Frank Notestein in 1945' (Caldwell, 1976a: 323). The latter has been interpreted by Burch (1994: 1) as 'a compliment to Notestein', but

> an indictment of demography theoretical failure. In over thirty years - across a generation or so of demographers - there had been little progress in the statement of one of the discipline's central theoretical ideas (Burch, 1994: 1)

With regard to McNicoll's remarks Burch regretted that demographic transition theory has inspired 'theoretical progress in the sense of proliferation of theoretical insights, but much less by way of unification' (Burch, 1994: 2).

A second example comes from authors who are less interested in ‘sufficient formalization', but are much more anxious about the fact that demographic transition theory has generated little explanatory power. According to Newell, as a theory demographic transition

> has been criticised because it is unclear whether it is purely descriptive or whether it also includes statements about the causes of the transition ... Because it is formulated at a very general level it is very difficult to generate from it hypotheses which can be tested or refuted convincingly, a requirement for any scientific theory (Newell, 1988: 11).

In the same year, another author asserted:

> despite the elaborate detail with which transition theory depicted the process of moving from high to low fertility, it generated little specific explanation power because it had nothing to say about causation (Simmons, 1988: 92).

A third view comes mainly from allied disciplines; or better, from authors who lean on disciplines like history, anthropology and economics for two main reasons: some authors have become increasingly interested in solving problems concerning demography, though they either prefer to be seen as outsiders or they simply continue too attached to their own initial training. Others seem entirely convinced that demographers have become victims of their own methods, so short of ideas, and so motivated by policy objectives that they see no better help than emancipating them from demographic content itself. Two examples can illustrate these stances, one from an historian and the other from an anthropologist.

Szreter published an article in 1993 maintaining strongly that the only road for further intellectual progress in the study of fertility change is through an 'emancipation from the dominance of the abstract idea of "demographic" or "fertility" transition and the associated, too exclusive deference to the covering laws methodology' (Szreter, 1993: 692). Szreter's article is titled 'The idea of demographic transition and the study of fertility change: a critical intellectual history'; because of the second part of this title the article is of some interest here.

As 'a critical intellectual history' of how demographic transition was transformed, in the middle of the twentieth century, into the leitmotif of policies on population control the article is convincing. Szreter contrasted the reception of demographic transition theory in the United States in 1929 and 1945, and demonstrated how Notestein and Davis became increasingly concerned with rendering demographic transition applicable and politically feasible; between 1944 and 1950, Szreter demonstrated through an analysis of Notestein's writings,

> the key conceptual change to the theory of demographic transition was the modification whereby fertility was no longer viewed as the ultimate dependent variable - the final outcome due to other necessarily prior manifestations of economic, social, and cultural modernization. Instead it came to be viewed as something that historically had occasionally changed relatively independently of these other forces and as something that could and should be changed by interventionist policies

> designed to work in advance of other measures aimed at affecting wider social, economic, and cultural change (Szreter, 1993: 670).

However one may be inclined to sympathize with Szreter's critique, the intellectual history under scrutiny in his article is concerned with the politics around demography rather than with the history of demographic ideas themselves. In that case the title should not betray the actual content of the article; a more accurate sub-title would have been ' a critical intellectual history of political demography'.[6] As far as political demography is concerned, Szreter's critique is also supported by the authority of criticisms made by prominent contemporary demographers. In particular, he used wisely Demeny's remarks regarding the transformation of demographic research into 'oxymoron', or the contradiction in terms well caricatured through metaphors like 'goal-oriented "industry"' and 'family planning industry' (Demeny, 1988: 461, 466). To some extend, Szreter's article illustrates well how unrealistic Hauser and Duncan (1959: 17-23) were some three decades ago:

> it is almost universally recognized among population studies that a sharp division of labor must be effected between research with its related scientific activities and 'social engineering' behavior directed toward the formation and implementation of policy (Hauser and Duncan, 1959: 19).

Demeny used exactly this 'postulate of a sharp division of labor' to introduce his own discussion of some of the salient disagreements concerning it. Szreter has chosen one of the most strong, and perhaps disturbing, passages from Demeny's article concerning some the unhealthy intellectual tendencies in contemporary demographic research:

> social science research directed to the developing countries in the field of population has now become almost exclusively harnessed to serve the narrowly conceived short-term interests of programs that embody the existing orthodoxy in international population policy ... Equally, it disdains work that may be critical of existing programs, or research that seeks to explore alternatives to received policy approaches.

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In this regard the demography of Lotka seems to have not been safer and less exposed to the dangers of misuse than the nuclear physics of Einstein.[7]

Yet, as 'a critical intellectual history' of the scientific development of the idea of demographic transition, Szreter's article is disappointing and misleading. No political misuse of a scientific tool seems strong and valid enough to question its scientific usefulness. In one of his footnotes, Szreter (1993: 694) provided evidence from Hodgson's (1983) and his own research indicating that the origin of demographic transition was not a political imposition of the U S Government. The problem, though, was that Szreter was not interested in the scientific origin of the idea of demographic transition; this explains why he found nothing useful in demography itself when he tried to propose directions to overcome the political instrumentalization of demographic transition.

Curiously, in the conclusion of Szreter’s paper there is at least one saving paragraph of a positive reason for the persistence of demographic transition theory:

> the principal virtue and function of the idea of demographic transition has always been in providing a graphic metaphor that summarily describes - and predicts - a long-term overall emergent pattern of change. As such it has enormous justificatory, motivational, and communicative value for agencies and institutions wishing to effect such change (Szreter, 1993: 692).

On this grounds, there is no difficulty in accepting Nisbet’s remark about the limitation of such a virtue and function of the idea of demographic transition:

> But, as Robert Nisbet has argued, a summary description of this metaphorical sort offers no necessary assistance or insight into the causal explanation of *how* such change occurred or occurs in any particular case. Unless an explicit teleology is subscribed to, it is simply an act of faith to base empirical and historical research on its premises. As a heuristic to guide detailed research aimed at understanding how fertility change in specific historical circumstances, the idea of demographic transition is not only unnecessary but also inappropriate (Szreter, 1993: 692).

Szreter concludes that because the virtue and function of demographic transition do not go beyond the scope of a descriptive purpose its whole ‘idea’ itself ‘is not only unnecessary but also inappropriate’. This is what is known as throwing out the baby with the bath water, and for this reason Szreter's discussion seems not only misleading but even cynical; its striking message that is that no development in twentieth-century demography is good enough to set the ‘condition for further intellectual progress in the study of fertility change’ (see Abstract, Szreter, 1993: 903). Despite Szreter's claim ‘that the most promising approach for future progress in the study of the causes of fertility change is historical and empirical research employing a range of broadly realist methodologies’, his article dismisses the usefulness of the history and methodology developed by demography itself.

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[7] This parallelism becomes even less accidental if one recalls alarming titles such the following: *The Population Bomb* (Ehrlich, 1968) and *The Population Explosion* (Ehrlich and Ehrlich, 1990).

One of the best examples of the emancipist alternative admonished by Szreter can be found in the latest writings of the anthropologist Greenhalgh. Within just five years, Greenhalgh has already moved from a proposal towards an anthropological 'political economy of fertility' (Greenhalgh, 1989, 1990) to a 'feminist demography of reproduction' (Greenhalgh, 1994; Greenhalgh and Li, 1995). This is certainly a step far beyond Szreter's (1993: 692) abstract appeal 'for an accumulation of patient, carefully contextualized, investigative projects on fertility change in specific communities'.

Although Greenhalgh’s writings do not judge fertility in advance in the same way the notion of ‘fertility transition’ does, her gender approach as 'pertaining to women' entails another sort of pre-judgement of fertility. In 1994, Greenhalgh commented about the ‘distressingly little empirical support' for demographic transition theory:

> Beginning in the late 1960s, demographers' research showed that the leading theory of the demographic transition from high to low vital rates, a variant of modernization theory, had distressingly little empirical support. The data said that it was not social and economic development, but ‘culture’ - defined as ethnicity or language or geographical region - that seemed to count (Coale, 1969; Coale and Watkins, 1986) (Greenhalgh, 1994: 4).

In the following year Greenhalgh put forward an explicit proposal which expresses an open disdain towards demography's own history:

> The theoretical need for a feminist demography lies in the narrowness and general weakness of demographic theories of reproduction, problems widely recognized by demographers themselves (Schofield and Coleman, 1986; McNicoll, 1994). A highly mathematical discipline known for being long on methods but short on ideas, demography remains wedded to positivist methods of data collection and analysis ... While demographers have made a valuable contribution in exposing the growing discrimination against baby girls, their theoretical framework is limited by their exclusive reliance on aggregate demographic data. Such data tell the end of the story but shed little light on the complex processes leading up to it (Greenhalgh, 1995: 602-603)

This statement does little good both to feminist theory and to demography. After all, nothing that might be learnt elsewhere can be relevant, in this case, to demography if one dismisses its own history of ideas, including its long history of methods, as mindless.

In short, in the previous four examples I have chosen issues which cannot be dismissed as irrelevant to demography, such as: lack of sufficient formalization in the area of population studies; much political manipulation of demographers' research in past years; weak empirical support to decide about causes and mechanisms of fertility change; and the need to improve the explanatory and predictive powers of demographic theory.

No doubt that each of these issues affects specific aspects of demographic theory and should be tackled in their right place. However, those same issues can also be used to put forward partial, misleading and very cynical assessments of demography in general. Partial and misleading, because they do not admit that a descriptive theory remains useful enough even when it is based on crude measures such as classical demographic transition theory. Cynical, because the view that demography has been ‘long on methods but short on ideas' (Greenhalgh and Li, 1995: 602) is in itself misleading and short of vision. First, it implies that methods and models are mindless, rather than part of a broader idea or theory construction. Secondly, it fails to explain, for instance, why a descriptive scientific theory such as the theory of demographic transition cannot be expected to have much explanatory power; or why conventional demography teaches Lotka's renewal equation and not Geiger counters; or yet why mainstream teaching has so far failed to even inform students of the existence of the so-called 'two-sex problem'.

After all, the problem with any partial and cynical assessments of demography's limitations is that they fail to inform readers that demographers are often so involved by their ideas that they even appear to be only concerned with methods. This means that although cynicism alone can seriously damage the reputation of the conventional, it is doubtful that it can do any good to expand the ideas of demography.

The diversity and wealth of neuter demography

Chapter 9 has given a brief characterization of neuter demography, particularly its kernel and domain of validity within demography in general. To stress the strengths and limitations of neuter demography I concentrated the discussion on just two of the most important strands within a neuter demography: the centrality of Lotka's neuter stable population theory in demography in general, and the importance of the demographic transition theory in the scientific study of population change. Since Graunt set the agenda for the study of the two most important components of population change, mortality and fertility, these two fields have grown in parallel. Lotka brought them together into an elegant single model, and about two decades later Thompson (1929: 959) generalized his link between stable theory and observed data with the objective to point out 'the most significant tendencies in the population movements of different countries rather than attempting to state the total growth in the world or in any given part of it'. A quarter of a century later Thompson's own exercise was stylized and converted into the elegant abstract model called demographic transition. Since 1945, the initial link made by Thompson among the Malthusian population, the observational data and Lotka's imprint on both of them almost fade away. The demographic transition turned into an insight so simple and dissociated of the core demographic transition until Thompson that his 'Population' is sometime remembered not so much as a test of demographic theory but an insight not yet theoretical.

Demographers of all ages can in fact be charged of being unable to find no good way to move from commonsense directly to a study of causal explanations of 'why' and 'how' population changes in a given region. I doubt that this is a fair charge. Just as demography should not me restricted to a descriptive discipline, it is doubtful that demographic science could or should dismiss its descriptive body of theory simply because it is not enough explanatory. The process of theory construction in demography has generally passed through a stage in which demographers converted their subjective intuitions into 'educated guesses'. In this context, the theory of demographic transition provides a model of demographers' educated guesses about the most significant patterns in population movement. But, as Keyfitz put it,

> When people of good judgement made what seemed a perfectly safe forecast and something quite different materialized, we know that there must be changes, and variables operating in the depths of our society that strongly influence what appears on the surface (Keyfitz, 1980: 63)

Most of the 'contested territory' of demographic transition will probably become irrelevant when demographers were able to apply its historical descriptive model as they have learned to apply Lotka's neuter stable theory and the crude vital rates. There are several good reason why demographic transition theory should not be dismissed as totally unnecessary and inappropriate. Two such reasons were pointed out by Newell: first, demographic transition theory provides an abstract model for a first approximation to demographic analysis, including in 'formal demography'. Second, it emphasizes the strong interrelationships between population structure, fertility, mortality and migration which lie at the heart of formal, theoretical, and empirical areas of demography. Third, demographic transition theory is very easy to understand and can provide the communicative basis between professionals of demography and scholars of different backgrounds who are interested in population change: professionals of other fields and policy-makers. Fourth, demographic transition reflects the scientific tradition set between Halley and Lotka, that is the tradition of the life table and the stable population theory.

As in any other science, much of the misuse of demographic transition can be attributed to misconceptions about the relationship between abstract models and the reality they are expected to represent. The two tables that follow are an attempt to place the debate in a broader context. Table 2.9.2 gathers some of the best known indicators, demographic and socio-economic, which fall in this thesis into the designation neuter demography. The data refer to Asia only, a region that alone contains more than 60 per cent of the world's population and a diversity of cultural and socio-economic conditions. Ever since at least Malthus this type of data has been correlated in a variety of ways in drawn comparisons, possible hypotheses and generalizations about possible changing conditions in specific populations. A great many of the studies examining the relationships between two or more variables, theoretically and empirically, take the neuter approach for granted.

Table 2.9.2 Some neuter socio-demographic indicators (a)

	Estimated population (millions)		Population Density (1000 ha)	Population growth rate (%)	Life expectancy at birth (years)		Infant mortality per 1,000 live births		Real GDP per capita (PPP$)		Average size of household
	1960	**1991**	**1991**	**1960-91**	**1960**	**1990**	**1960**	**1991**	**1960**	**1990**	**1975-86**
East Asia	700	1250	1120		47.5	70.2	146	28	730.0	2,220	
China	657.5	1170.7	1255	1.9	47.1	70.1	150	29	723	1,990	4.4
Hong Kong	3.1	5.8	58121	2.0	66.2	77.3	44	6	2,323	15,595	4.2
Korea, Dem. R. of	10.8	22.2	1843	2.3	53.9	70.4	85	25			
Korea, Rep. of	25.0	43.8	4435	1.8	53.9	70.1	85	22			4.5
Mongolia	1.0	2.2	14	2.7	46.7	62.5	128	62			
South-East Asia & Oceania	230.0*	460.0*	941*		45.3	62.6	126	58	1,000	2,590	
Brunei Darussalan	0.1	0.3	501	3.8	62.3	73.5	63	9			
Cambodia	5.4	8.6	485	1.5	42.4	49.7	146	120			
Figi	0.4	0.7	401	2.0	59.0	64.8	71	24	2,354	4,427	
Indonesia	96.2	187.7	1036	2.2	41.2	61.5	139	68			4.9
Lao People's De. R.	2.2	4.3	188	2.2	40.4	49.7	155	101			
Malasia	8.1	18.4	561	2.6	53.9	70.1	73	15	1,783	6,140	5.1
Myanmar	21.7	42.7	650	2.2	43.8	61.3	158	85	341	659	
Papua New Guinea	1.9	4.0	88	2.3	40.6	54.9	165	56	1,136	1,786	
Philippines	27.6	63.8	2140	2.7	52.8	64.2	80	42	1,183	2,303	5.9
Samoa	0.1	0.2	558	1.2							
Singapore	1.6	2.7	44902	1.7	64.5	74.0	36	7	2,409	15,880	4.7
Solomon Islands	0.1	0.3	118	3.3	50.3	69.5	139	68			
Thailand	26.4	55.4	1084	2.4	52.3	66.1	103	28	985	3,986	5.2
Vanuatu	0.1	0.2	126	2.8							
Viet Nam	34.7	68.1	2092	2.2	44.2	62.7	147	39			5.3
Australia	10.3	17.3		1.7	70.7	76.5			7,204	16,051	
New Zeland	2.4	3.4		1.2	70.9	75.2			7,222	13,481	
South Asia	600.0	1220.0	1876		43.8	58.4	164	93	700	1,250	
Afghanistan	10.8	17.7	271	1.6	33.4	42.5	215	165	775	714	
Bangladesh	51.4	116.4	8946	2.6	39.5	51.5	156	111	621	872	
Bhutan	0.9	1.6	335	2.0	37.3	48.9	203	133			
India	442.3	862.7	2902	2.2	44.0	59.1	165	90	617	1,072	5.5
Iran, Islamic Rep.	21.6	59.9	366	3.3	49.5	66.2	169	44	1,985	3,253	4.9
Malvides	0.1	0.2	7333	2.6	43.6	62.5	158	58			6.1
Nepal	9.4	20.1	1467	2.4	38.3	52.5	187	102	584	920	5.8
Pakistan	50.0	121.5	1576	2.9	43.1	57.7	163	101	820	1,862	6.6
Sri Lanka	9.9	17.4	2698	1.8	62.0	70.9	71	25	1,389	2,405	5.2

Notes: * excludes Oceania

Source: UNDP (1993): Human Development Report 1993, pp 4, 51, 142, 180, 181, 193

United Nations (1987). 'Demographic Year Book' , pp. 894-9034

(a) This table has been included in Francisco, 1994b

In turn, Table 2.9.3 provides a brief chronological overview of population theories which in their fundamental way can be said to be neuter. The table includes a range from ancient theories to some of the most recent theories proposed such as the asexual theory of inequality and intergenerational mobility outlined by the Nobel Prize-winner in Economics Garry Becker (Becker and Tomes, 1979); McNicoll's theory (1980, 1993b) on the institutional determinants of fertility; and Greenhalgh's (1990) anthropological variant of a political economy of fertility.

Table 2.9.3 Summary of some of the Neuter Population Theories

Rough Dates	**Main Source/Authors**	**Emphasis on**
Ancient theories 1750 B. C to 1300 A.D.	*Atra Hasis* (the Babylonian poem); China: Confucius (551-478) Ancient Greece & India Hippodamus (450-400), Plato (427-347), Aristotle (384-322), Polibio (200-120); Kautilya (321-296) Roman Empire, Judaism and early Christianity: Cicero (50), *Genesis*, Augustine (345-430), Gregory (540-640), Aquinas (1235-1274)	- One of the oldest literary works extant, concerns itself with pressure of numbers, pollution, and the need for an effective population control policy. - Excessive population growth depressing living standards of masses. An optimum relationship between population and the land. Recommended policies to balance numbers against resources. - The optimum size of the City State, defined by Plato as 5,040 (equal to factorial seven), which should be achieved by restricting, or by encouraging births with rewards. Yet Polibio developed opposing ideas to Plato and Aristotle, for he pointed out that the social problems in Greece were accompanied by underpopulation. For him a family with only two children can easily become empty, for it is enough that war kills one and disease kills the other. - Romans stimulated population growth, e.g. by giving privileges to those with children. (More men would mean more military conquests). Population growth ('Go forth, and multiply'). - Marriage unifies husband and wife to generate children. Celibacy morally good (but many births needed to counter high mortality). - Moral disapproval of abortion, infanticide and divorce.
Mercantilism 1400 to 1700	Italy: Machiavelli (1467-1527), Campanela (1568-1639), Botero (1540-1617) France: Jean Bodin (1530-1596); Montchrestien (1575-1621); Vauban (1633-1707) England: T. More: 1478-1535); F. Bacon: 1561-1626); T. Hobbes: 1588-1679; W. Petty (1623-1687); J. Graunt (1620-1670); E. Halley (1656-1742), German: J. Süssmilch: 1707-1767 Physiocrats: F. Quesnay	- The populationist views associated with reinforcing the power of the Prince. Outlines of some eugenic ideas close to those of Plato on the quality of population. To promote a numerous population should be the first concern of the State, through agriculture and other activities. State intervention in economic activity to maximize national wealth. Increased population would mean larger armies, lower hourly wages and increased wealth, and colonization could be the way to avoid the excess of population over food resource. - Defended an intransigent populationism. For them there is no wealth nor strength but in men. - Defended a more rational populationism. - Population is considered as a variable among many others composing the social system. These authors looked for an equilibrium between population and resources. First steps of population studies as subject of independent investigation on its own account. The numerical study of population starting in the 17th century, rather than earlier speculations, marked the beginning of demography. The 17th and 18th centuries saw the beginning of the sustained growth of world populations. - While Graunt may have anticipated Süssmilch in making the growth of population a subject of independent investigation, the latter was perhaps the first who clearly grasped the fact that when and only when sufficiently large numbers are taken into account, order and not accident appears (Hull, 1899: lxxviii). - In favour of 'Rule by nature' or 'laissez-faire' (i.e. no government intervention); population dependent on subsistence, and agriculture the only source of wealth; benefits from social reform would be cancelled by population increase.

1750	Leonhard Euler: 1707-1783 Johann P. Süssmilch	- The inventor of the concept of Stable Population (1760). He anticipated important parts of modern stable population theory, but in his model the sexes were combined. - An illustration of population growth.
1855	Jean Guillard (1799-1876)	- 1855: The year of the creation of the name 'demography'.
1800 to 1900	T. Malthus: 1766-1834 Adam Smith (1723-1790); David Ricardo Karl Marx (1818-1883) F. Engels (1820-1883) Hazlitt	- Unless checked, population will tend to increase faster than subsistence. In explicitly mathematical terms the Malthusian population is expressed by the first-order differential equation: $\frac{dn}{dt} = rn$ where n is population size, t time, and r the rate of pop. increase. - Diminishing returns to labour. (Later writers such as Marshall emphasized increasing returns). - Anti-Malthusian: preventive checks would become more effective. - Socialist and Marxist writers: population and surplus labour are the result of the capitalist economic system. - Neo-Malthusians: Restricting population growth by the use of birth control. (Malthus himself was against birth control).
1900 to 1950	Thompson, 1929 Notestein, 1945 Davis, 1945	- The demographic experience in Western countries gave rise to the *theory of demographic transition* or *transition theory*, which basically stands on one *stationary* situation (where population growth is zero) to another. Although earlier writers had noted the European transition, the first comprehensive explanation of fertility change in the context of neuter theory was given by Notestein (1945).
1951 1980	Davis and Blake, 1956 Arthur and McNicoll, 1978 Becker and Tomes, 1979	- Social structure and fertility: an analytical framework. - Samuelson, population and intergenerational transfers - An equilibrium theory of the distribution of income and intergenerational mobility.
	Paul Ehrlich, 1968 Ehrlich and Ehrlich, 1990 Julian Simon, 1981	- Ecologists are interested in optimum population, but they sharply reject the gradually diminishing returns and continuous changes of classical economics and often attack radically the modern system of production and consumption. - Sustainable development - development without growth. - The economics of population growth
	Bucharest Conference	- Is there a population problem?
	The customarily used demographic neuter functions	- Arithmetic, exponential and geometric growth: $P_{t+1} = P_t(1 + r)$ $P_t = P_0 e^{rt}$ $P_t = P_0(1 + r)^t$
	R. May, 1975, 1976 Lee, 1974 Arthur and McNicoll, 1972 McNicoll, 1980, 1993b Greenhalgh, 1990	- Deterministic models with chaotic dynamics; simple mathematical models with very complicated dynamics. - The formal dynamics of controlled populations and the echo, the boom and the bust. - Optimal population policy. - Institutional determinants of fertility change. Institutional analysis of fertility. - Toward a political economy to fertility: anthropological contributions.

Source: Lucas (1994a: 20); Nazareth (1988: 13-60); Parson (1991:356-7); Becker and Tomes (1979); Keyfitz, 1972; Dupâquier and Dupâquier, 1985; Pearson, 1978; Smith and Keyfitz, 1977.

At the heart of the neuter debates on population has been the Malthusian relationships between population growth or density or fertility, on the one hand, and the rate of economic growth, the means of subsistence or environment, on the other. This matter has fascinated in the past and it seems it will continue to fascinate for many years to come.

> At issue here is a deep and thorny matter that has troubled the philosophy of science for the last several hundred years, the ascription of causality. Because the debate concerns fundamentals rather than technical questions, there is room for more than one point of view (Simon, 1989: 324)

This reference to Julian Simon seems appropriate here, for this author has become in the past two decades the most maverick voice countering the increasing antinatalist perception of the impact of population growth. Even when some authors have, in the last ten or so years, taken as McNicoll put it, 'an active complacency' (McNicoll, 1995a: 307) concerning the role of population growth, Simon dismissed the 'revisionist position' but exactly for the opposite reasons from those put forward by McNicoll.

At stake has been the conclusion particularly of the authoritative US National Academy of Sciences (NAS), which in 1986 concluded:

> [S]imple correlations between population growth and per capita income, although intriguing, ultimately provide little insight into the causal impact of a policy-driven decline in fertility. A scientific assessment of the impact requires that one identify the major mechanisms by which population growth is hypothesized to affect economic development; assess the evidence for each hypothesis; and, finally, synthesize the effect of the simultaneous operation of the mechanisms (National Research Council, in Simon, 1989: 323-324).

Simon countered this, among other studies, arguing that because the studies persuasively show an absence of association in their data, they incorrectly imply the absence of a negative causal relationship: 'In other words', Simon (1989: 325) asserted, 'the other writers point to what the studies do not show, whereas I point to what they do show'.

Since two-variable correlation studies do not reveal the forces that influence economic development, Simon maintained that the conclusion that 'neither variable is influencing the other' can still be persuasively refuted. He challenged researchers to build plausible scenarios

> in which one or more specified variables that have been omitted from the analysis would, if included, lead to a negative partial relationship between population growth and economic development. The variables must be named by the critic, and they must seem reasonable (Simon, 1989: 325).

Barlow, in a paper published in 1994, responded to this invitation, and proposed what he called 'lagged fertility ' as one omitted variable producing the result described by Simon. Perhaps somebody has already attempted to added a fourth omitted variable which will cancel Barlow's conclusion, based on his threefold variable model, that per capita income growth is negatively related to current population growth and positively related to lagged fertility. The merit of a neuter approach is exactly its flexibility to multiple possibilities which can be mutually exclusive but all internally coherent logically.

However aware one researcher may be about the crude or neuter measures, it is not up to the crude birth and death rates whether or not one researcher decides that they can be directly correlated with the rate of growth of per capita income. Nor is it up to the GDP per capita if demographers accepts the conventional view that this measure reflects the actual production of a given society. Even when data such as those included in Table 2.9.2 are placed in specific scenarios which are expected to depict plausible contingencies and mechanisms of demographic phenomena, the conclusions achieved can remain impersonal or neuter.

The literature is full of criticisms of the limitations revealed by the neuter approach, though critics usually focus their attention on specific manifestations of the overall nature of neuter demography: the reliance on impersonal and aggregate data (Greenhalgh and Li, 1995: 602); the 'explanatory amorphousness’ arising 'as a by-product of the eager endorsement of a statistical technology for empirical research in the social sciences' (Szreter, 1993: 684); or Wunsch’s (1984: 1-2) caricature comparing researchers in demography with a conjuror drawing out a rabbit from a top hat at the same time that they pretend to handle the hidden complexity of demographic relations.

In any case, the neuter approach does not stop producing new insights and scenarios of analysis. Perhaps one of the most recent versions of modern typology of population patterns not very different from that provided by Thompson, Notestein and Davis has been put forward in recent years by McNicoll (1991, 1993b, 1995b).[8] Instead of subscribing to the classical modernization theory, in 1993 McNicoll applied the approach on the institutional determinants of fertility he proposed in 1980 and identified five typical patterns of fertility transition: traditional capitalist (Latin America - Brazil); 'soft state' (Northern Asia, namely Bihar, Bangladesh); radical devolution (China); 'growth with equity' (other East Asia - Taiwan, South Korea); and 'lineage influence' (Sub-Saharan Africa). Contrary to many typologies of population patterns, the designations proposed by McNicoll try to stress the scale and intensity of social change rather than the orthodox geographical classification of major subregions. This is an important perspective, but McNicoll himself does not appear to be interested in distancing or even opposing his approach against classical and post-classical transition theories.[9]

The contemporary institutional approaches seem little different from that of the early 1930s sociological school called structural-functionalism, or functionalism for short.

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[8] See also Figure 1 in McNicoll (1991: 34): 'Total fertility rates of major third-world regions: estimates (1950-1985) and projections (1990-2025) - East & Southeast Aisa, Latin America, South Asia, Sub-Aaharan Africa, and West Asia & North Africa.

[9] Greenhalgh in a paper published in 1994 asserted that McNicoll's approach on the institutional determinants of fertility is 'very different' from classical and post-classical transition theories: 'although similar at a distance, appears highly idiosyncratic when viewed at close range', Greenhalgh (1994: 13) wrote, 'He argues that the pattern of reproductive change is shaped by the institutional endowments each society has inherited from its past'.

In 1964 Homans characterized this school in a suggestive paper called 'Bringing men back in':

> First, the school took its start from the study of norms, the statements the members of a group make about how they ought to behave, and indeed often do behave, in various circumstances. It was especially interested in the cluster of norms called a role and in the cluster of roles called an institution. It never tired of asserting that its concern was with institutionalized behavior, and that the unit of social analysis was not the acting individual but the role. The school did not ask why there should be roles at all. Second, the school was empirically interested in the interrelations of roles, the interrelations of institutions: this was the structural side of its work. It was the sort of thing the social anthropologists had been doing, showing how the institutions of a primitive society fitted together; and the sociologists extended the effort to advanced societies. They would point out, for instance, that the nuclear family rather than some form of extended kinship was characteristic of industrialized societies. But they were more interested in establishing *what* the interrelations of institutions were than in *why* there were so (Homans, 1964: 809-810).

Homans mentioned a third feature which may hold the key to understanding his neuter discourse about 'bringing men back in'. In his opinion the functionalist school was more interested in the consequences than in the causes of an institution, particularly in the consequences for a social system considered as a whole. Likewise, contemporary institutional approaches usually analyse society in general, specific social groups in particular, around a neuter 'model of man' (de Bruijn, 1993: 45; McNicoll, 1980: 449-450); to paraphrase Homans (1964: 811) they start from the existence of a particular institution and try to find out what difference the institution makes to the other aspects of social structure. Besides being neuter in the conceptual and methodological analysis, they rely on structure-oriented rather than process-oriented thinking.

The effort to overcome the structure-oriented approach in social science is not new (Anderson, Arrow and Pines, 1987; Arthur, 1989, 1990; Jantsch, 1975, 1980, 1981; Kellert, 1993; Loye and Eisler, 1987; Nallari, 1991). In demography, McNicoll (1993b: 4; 1995a: 314-315) acknowledges that 'institutions are *path-dependent*'; or that 'Nonlinearities, associated with local increasing returns and other self-reinforcing mechanisms in the economy and society, generate multiple equilibrium growth paths' (McNicoll, 1995a: 314).

In short, Table 2.9.3 leaves no doubt that neuter theories of population and family constitute a wealthy and powerful body of theory relevant to demography. The fact that they deal with men and women through the social institutions they create may be too simplistic and abstract for some purposes, but they are undoubtedly useful for others. Beyond that, the extent a neuter approach can in fact bring men and women to their analysis and remain neuter is doubtful.

10.

One-sex demography and fertility outputs

There is a crack in everything
That's how the light gets in
Leonard Cohen

The principle of separation between the sexes

The most successful body of analysis in the twentieth-century demography comprises the set of concepts, measures, methods and theories that can be called 'one-sex demography'. Together with the neuter demography they constitute the bulk of conventional demographic courses, textbooks and empirical research.

The one-sex demography relies on the second of the three-dimensional set of principles introduced in Chapter 8: the principle of separation of the sexes. This principle is consistent with the view that in reproduction there are not one but two main demographic natures: male and female. That is, the differentiation among individuals is set first of all by the category sex. Further, from the point of view of population structure the size, growth and development is no longer a function of an abstract time, but the age of individuals. These variables, sex and age, define population structure and explain why the demographic organism does not fall down.

For about two hundred years demographers have been increasingly aware that although neuter demography captures certain aspects of demographic reality, its scope of validity and reliability is limited. Neuter concepts, measures and models abstract from population composition, both its structure and organization: they offer first and quick approximations to population growth and development, but in general are unable to reveal the forces that influence them. One of such forces is population structure, which determines and is determined by the number of births, deaths and moves.

This chapter aims at demonstrating the assertion, in Chapter 6, that one-sex demography has become remarkably successful because of its adequate match between the conceptualization of demographic phenomena and the methodologies developed accordingly. Demographers have often entertained the thought that one-sex methods can be applied to either sex and that fertility is usually measured for women because it is easier than doing it for men. Technically speaking, it is true that there are several measures and models in demography that can be applicable to either sex, including the fertility rates, net reproduction rate, and stable population theory. However, the argument of convenience concerning the application of one-sex models to female population reflects a very superficial level of inquiry that has led to much misunderstanding.

This chapter shows that by taking the principle of separation between the sexes seriously Lotka's theory can be said to hold for its explicit and implicit assumptions. On these grounds, it can be argued that the comparisons of results derived from the application of one-sex models independently to both sexes are not consistent with the theoretical setting of the one-sex approach itself. Not only are the two sets of the population defined by sex hardly comparable with regard to their age distribution; but even more significant, males and females play distinctive roles in reproduction, which the one-sex approach is unfit to take into account.

As I have shown already in my overview on the evolution of the demographic concept of 'fertility', it took about two centuries for the necessary and sufficient conditions for the one-sex demography to blossom. The necessary condition corresponds to the recognition that in its fundamental way the structure of population can be reasonably described by focusing on females only, for the simple fact that they constitute the producing sex in demographic reproduction. This is not to imply that males are dispensable or unnecessary, but rather to assume that they exist in whatever numbers and behaviours are required to allow females to renew themselves and the whole population (Keyfitz, 1977b: 10). Besides that, one-sex methods do not exist for their own sake; rather they always presuppose clearly designated and reliably measured phenomena that are by their nature one-sex as well.

Once the second bifurcation in the development of the demographic concept of fertility output was attained and accepted by a large number of demographers, the conditions became sufficient for the development of one-sex methods. This occurred from the 1850s to the 1880s, the period of the nine international congresses of statistics. From then on, it did not take long for the development of specific models and theories around or including specific measures of fertility output. Curiously, if one compares this process with the development of mortality in the time of Graunt, it took almost the same time since the abstract concept of fertility became well established. After Graunt set the basic working concepts of mortality analysis, and especially his hypothetical distribution of mortality into age groups, it took about three decades until Halley outlined the first mathematical model of life tables. Likewise, after the first international congress of statistics in 1853 set the notion of fertility output and its data requirements it took also about three decades until Böckh outlined in 1884 the first fertility model to calculate the net reproduction rate. And then, the first three decades of the twentieth century became extraordinarily productive for demography, particularly in Europe, United

States and Australia. So remarkable were the first three decades for the development of demographic theory that some still believe that it was only then that fertility analysis was born. However, that was the time when the two most important components of demographic analysis, fertility and mortality, converged into an integrated and coherent model.

Convenience versus principles: why demography treats men as a dispensable sex

The one-sex methods have developed in formal, theoretical and empirical areas of demography somewhat independently from one another but sharing some common features, such as the principle that population can be subdivided into two main groups, males and females, though the latter are the ones potentially or actually exposed to 'risk' of having children. Secondly, population renewal and reproductivity are mainly concerned with births and these events are the derivative of the female population because only women give birth to children. Thirdly, the act or the process leading to demographic events such as births, or more generally, demographic outputs, can loosely be said to refer to the number of births actually produced by the population, its size, growth, and development.

The expression 'produced by the population' is loose in two ways: conceptually, because in that expression births are roughly related to the population in which not all members are at risk of giving birth or marrying; methodologically, demographers usually discuss this when they consider the numerical subtleties behind two important demographic concepts: demographic rates and demographic probability. The former is defined as a fraction in which the number of occurrences of a certain event (e.g. births, or even only daughters) in a certain period of time is divided by the mean population 'potentially at risk of the event' in the same period. The latter is the 'likelihood' or the risk that an event will occur in a given period of time; it is estimated as a fraction in which the number of the number of people who undergo a certain type of event at a given stage of their lives is divided by the population at risk of the same event.

In Chapter 6, I have mentioned that already Moheau as far back as 1778 asserted that 'for the reproduction of the species, the female sex is the one to which the State has its most obligations because it is the one that produces' (cited by Dupâquier and Dupâquier, 1985: 350). The authors of the *Histoire de la Démographie* used the authority of Moheau's remark to dismiss Quételet's attempt to gather data on fertility according to father's age and in the same way done in terms of mothers' age. But Moheau's remark can equally be used to counter the view that the relationship between births and the number of women in reproductive age is a simple matter of technical convenience. Although males constitute roughly half of the population and their numbers and behaviour are not irrelevant, there is little sense in pretending to consider the total set of contingencies on which births depend at the expense of the different roles of both sexes in demographic reproduction.

In choosing an axiom or a conceptual criterion Moheau's position seems rather more meaningful than any attempt to take refuge in arbitrary convenience. It is a position of principle which is scientifically rather more realistic than the position that everything in population depends on everything else; although the latter view is true in its fundamental way, even if it were possible to embrace every detail of all relationships in population it is still doubtful that one would then comprehend how and why population actually behaves and changes.

Körösi was one, if not the first, among the few demographers who have really taken pains to apply one-sex methods to monogenous fertility based not only on females but also on females independently. Although Körösi maintained that the bigenous birth-rates ideally should furnish a more reliable measure of fertility levels and trends, in the end he admitted that a more adequate measure of fertility is obtained by investigating not the fertility of the whole productive population, but for single ages of it.

As well, Knibbs more comprehensively discussed the monogenous partial tables of fertility in the wider context of a complete fertility table set in terms of the ages of females as well as males. Like Moheau, Knibbs's acceptance of the female-only approach was founded on the view that females constitute the 'producing sex' in the demographic system. It is unfortunate that the way Knibbs outlined the discussion on fertility placed in the broader context of demographic reproduction has never received any significant attention. To some extent, what happened to Graunt has happened to Knibbs what happened to Graunt. Just as Graunt's mathematical method of probability estimates was for too long overshadowed by Newton's powerful scientific method, Knibbs's approach on demographic reproduction was overshadowed by Lotka's self-renewal linear demographic approach.

Much of the descriptive demography taught currently in conventional courses, or even the explanatory and predictive parts of demography as well, is deeply rooted in the principle of separation of the sexes. This is true for most demographic analysis, from classical stable population theory to period and cohort fertility analysis, as well as models of nuptiality and reproductivity to population projections and forecasts.

However, the view that the application of the one-sex methods to either sex is irrelevant is correct perhaps only in some cases, but in others such a premise is obviously nonsense. For instance, this is clear evident if one compares the premises behind the application of the one-sex methodology in what is currently known as the 'intermediate variable of fertility' with regard to classical stable population theory. In the latter case, demographers are said to find it more convenient to apply one-sex models to the female component of population because they have a shorter reproductive life-span, and illegitimate births are more readily attributable to mothers (Lotka, 1939; Pollard, 1973: 23). Technical speaking, the same argument could be extended, for example, to Bongaarts's model of proximate determinants of fertility, but in general demographers do not even contemplate such a odd possibility. After all, at least implicitly demographers known that for one to clearly and reliably determine why and how fertility output changes the idea that one-sex fertility models can or should be equally applicable to either sex is meaningless and even misleading. However important men are to the path moving through reproductive stages such as sexual intercourse, conception, and gestation in childbearing, empirical demographers have little doubt that the measurement of fertility outputs, namely its levels and trends, can be better produced by concentrating consistently on females. Bongaarts’s model on 'proximate determinants of fertility' stands not on an undifferentiated one-sex model, but a female-only model of fertility outputs.

This distinction is not made explicitly, for instance by Bongaarts, because in general demographers take the one-sex approach applied to female-only population more-or-less for granted. The one-sex methodology is implicit in the title of Bongaarts’s paper published in 1991: 'Do reproductive intentions matter?'. Surely, this paper would been different if Bongaarts had formulated his question in a slightly different way; for instance, 'Do women's reproductive intentions matter?'; or 'Do only women's reproductive intentions matter?'. Of course, these alternative topics would turn out to be inadequate because of the data used in the analysis. But the fact that demographers now can ask a question such as ‘do reproductive intentions matter?’ without having to explain why they are only dealing with reproductive intensions of females is an interesting revelation about how the one-sex model has become thoroughly interiorized in fertility analysis. In the case just mentioned, Bongaarts accepted uncritically the data produced by DHS surveys and, presumably, expected that the female-only approach to fertility can be used to explain the fertility outcomes as adequately and successfully as it has been used to describe and measure the levels and trends of its outputs. It is interesting that demographers still think this way, particularly when one realizes that fertility outcomes are not just about counting the actual number of children born in a given period of time, a matter that can well be done by concentrating on one sex separately from the other. Yet, it seems extraordinary that most demographers still assume that even when they aspire to explain why and how fertility has changed in a given country by focusing on issues such as fertility preferences on contraceptive use, ideal family size and, and desire to continue childbearing, it is still acceptable to rely on one sex and ignore the contribution of the other.

The particular issue on when, why and how both sexes should be taken into consideration by demographers is discussed in Chapter 11; but the same issues need to be discussed here in terms of when, why and how it is reasonable to apply the one-sex methods to the female component of population only. So the problem is not so much that fertility research has been framed around superficial questions, but rather that such questions stand on a very superficial level of inquiry; indeed, often fertility issues are framed in such a way that they appear immune to any theoretical principle and, in such circumstances, nothing that is learnt elsewhere refutes or confirms whatever demographers do.

The assertion that one-sex methods can be applied to either sex once and for all is part of such a superficial level of inquiry, and the next sections discuss it with the objective to situate the emergence of the new research problem called the 'two-sex problem'.

Stable population theory as a source of diverse theoretical developments

Following the abstract demographic system outlined in Chapter 8, the either-or approach derived from the principle of separation of the sexes can be expressed formally as follows:

$$\bigcup S_d = \{\vec{f}_i \cup \vec{m}_j ; \vec{A}\vec{S}_l\}$$

where the demographic system S_d consists of either females (f) or males (m) of specific ages (i, j), respectively; the mathematical symbol $\cup$ (or) is meant to highlight the either-or nature imposed by the principle of separation between the sexes. The $\vec{A}\vec{S}_l$ denotes the age-sex reference frame based on the idea that population structure is mainly defined by the standard variables age and sex.

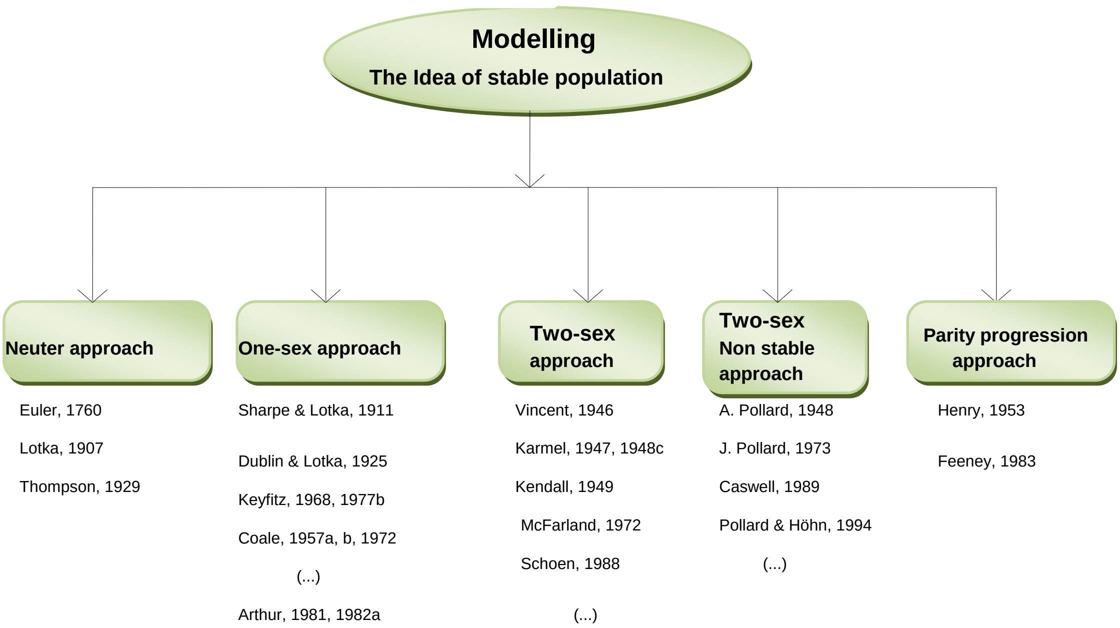
Historically the net reproduction rate proposed by Böckh in 1884 (in Kuczynski, 1935: 207) can be seen as the first concrete attempt to tacitly model fertility in conformity with the principle of separation of the sexes; behind the measure of net reproduction rate itself lies a table in which births are estimated on the basis of female children expected to born to a representative female child who throughout her life would be subject to current age-specific mortality and fertility rates. Of course, on the basis of the principle of separation of the sexes, births can be related independently to mothers or fathers as Körösi and Knibbs did with the male and female monogenous fertility (see also Kuczynski, 1932: 36-37; Keyfitz and Flieger, 1968: 642-666; Paget and Timaeus, 1994; Brouard, 1977).

In 1911, Bortkiewicz (cited in Kuczynski, 1935: 224-227) resumed and followed Euler's (1760) concept of stable population (Smith and Keyfitz, 1977: 76), but it was the work of Lotka in that same year that developed more systematically into the classical stable population theory. For about 15 years Lotka published a series of remarkable articles with Sharpe (1911), alone (1913, 1922), and with Dublin (1925) that established demography's 'central core of analytical development' (Notestein, 1950: 23).

Much of the controversy on the inconsistencies produced by the one-sex methods seems to have been caused by Lotka's ambivalence towards the applicability of the one-sex models to either sex. Since the techniques on the equations, parameters, relationships and applications of stable population theory are widely available in demographic literature, I will skim them and move immediately to the core of controversies that the development of stable population theory have motivated. For that, it seems important to avoid the conventional tendency to restrict the description of stable population theory to that approach associated with the name of Lotka. This is just one of several streams of theoretical developments closely related with the concept of stable population, and undoubtedly Lotka’s one-sex theory remains the central theory in the conventional teaching of stable population models.

The widespread tendency to speak of 'stable population theory' as a synonym of Lotka's pioneering approach is a fair compliment to its author because this has been the leitmotif, and perhaps the major source of inspiration, of all theoretical developments in twentieth-century demography aiming to study the dynamics of population structure. However, such a perspective does no full justice to the diversity of theoretical developments inspired by the idea of stable population throughout the twentieth century. Some of the alternative streams of research closely linked with the concept of stable population have even emerged as complements to, or perhaps against, Lotka's theory; they tackle similar issues and even claim to offer more adequate results. Figure 2.10.1 provides a classification of five important theoretical directions that can and should seen as being closely related to the development of stable population theory in the twentieth century.

Figure 2.10.1 Stable population as a source of diverse theoretical developments



The first direction corresponds to the one already mentioned in previous chapters; it dates back to Euler (1760) and attained prominence with Lotka's paper published in 1907. In Chapter 9, I have already explored the significance of this stream for the development of neuter approaches and, in particular, as a theoretical foundation for the classical demographic transition theory outlined by Thompson (1929). I will not discuss further this direction, though it can be added that the other four streams can be said to find in Lotka's 1907 paper a common theoretical basis; this paper outlined a neuter mathematical model of stable population which generalizes the modelling of long-term implications of constant mortality and a constant number of births each year by explicitly including a total birth rate.

The second stream became the most important stream of all and loomed from the neuter concept of stable population first in Sharpe and Lotka's paper published in 1911. Pollard considered this paper as the best symbol of 'the beginning of the subject of Population Mathematics' (Pollard, 1973: 23). Besides proposing a continuous-time model, perhaps even more important for this thesis is that in their paper of 1911 Sharpe and Lotka set the grounds for the development of the one-sex approach on stable population theory; they explicitly added a fertility schedule to the traditional mortality set. According to Smith and Keyfitz:

> Lotka's arguments were subject to mathematical criticism and did not gain universal acceptance until a rigorous mathematical proof that in the main they held for both discrete and continuous cases was given by William Feller ... Widespread application of the theory has come more recently, with the introduction of projection matrices and of modern computing equipment to facilitate the extraction of characteristic roots and vectors (Smith and Keyfitz, 1977: 76).

In the next sections this second stream is treated as the mainstream approach on stable population theory because it is the one widely taught and recognized in conventional demography as *the* stable population theory.

The third approach on stable population theory developed as an endeavour to replace the one-sex methodology in the mainstream approach set out by Lotka and his co-authors. This direction can be traced to two independent mathematical works: one from Paul Vincent, published in 1946, and the other from Karmel, first published in a paper of 1947 drawn from his Ph.D work that finished in 1948. Karmel’s papers published in 1947 and 1948 have been credited in the literature on the 'two-sex problem' as the fountainhead of the two-sex modelling. Indeed, just as Lotka’s approach turned into the symbol of *the* stable population theory, Karmel’s two-sex alternative to it has become the symbol of *the* two-sex problem. This is correct in that Karmel’s work emerged under the umbrella of stable population theory, aspiring to replace the one-sex model in which it was developed by Lotka. However, if one considers the two-sex approach in the sense proposed in this thesis it should become clear that Karmel’s thesis dealt with just part of a broader subject, in which fertility and mortality should be placed in the context of population composition as defined in Chapter 8.

While the mainstream one-sex approach on stable population and its two-sex alternative differ in the way they deal with the role of the sexes, at least two additional research directions should also be considered part of the development of stable population theory because of the alternatives that they offer. So the fourth direction can be traced to A. Pollard (1948) who extended Karmel's two-sex approach to relations between male and female rates in non stable populations. A fifth stream of research is based on the concept of parity cohorts. Although this concept can be traced at least to Henry's work on fertility of marriages earlier in the 1950s, it was in 1983 that Feeney published a paper called 'Population dynamics based on birth intervals and parity progression' which brought the relevance of parity progression for stable population theory into full view. Feeney’s paper proposes an interesting alternative to Lotka's theory set not around the role of age rather than sex; while it relies on a one-sex approach, the parity progression approach replaces age by parity and interval since previous birth.

This is not the adequate place to discuss each of the five streams I consider relevant in the development of stable population theory. It seems enough to stress that one cannot do full justice to, nor even comprehend adequately, the extraordinary role played by stable population theory in twentieth-century demography if such a theory is reduced to one of its streams. Indeed, one cannot even appreciate Lotka's own theory if the alternatives that it has inspired are not placed in the wider context of the development of stable population theory in its broad seam. In short, stable population theory has become central for the development of twentieth-century demography for at least four reasons: first, because it sets a systematic theory on population structure defined by age and sex and, in particular, a study of its growth and development through the correspondence between fertility and mortality. Secondly, such a correspondence has been clearly established and reliably measured mathematically in a variety of ways, such as in continuous- and discrete time models or in linear- and non-linear perspectives. Thirdly, the approaches on stable population theory have varied also according to the way the standard variables age and sex have been framed into the specific models; as Figure 2.10.1 illustrates, at least five distinct theoretical approaches can claim some inspiration as well as insight to the study of stable population theory. Fourthly, stable population has been very useful in estimating demographic measures on historical data and from incomplete or deficient data; even though data on fertility and mortality have improved since the times when stable population was almost the only adequate device to understand long-term demographic change, it would be misleading to think that the importance of its theoretical framework is fading away. After all, even when population projections are drawn from accurate empirical and mortality data based on data registration, the view that population will or should converge towards a stable or even stationary stage is in itself a legacy inherited from stable population theory.

Mainstream stable population theory: implicit and explicit assumptions

The transformation of the neuter stable theory into a one-sex model transformed the stable population theory from a natality to a fertility theory. Curiously, in 1991 Sharpe and Lotka proposed a one-sex stable theory and applied it to a male-only population only. Only in 1922 did Lotka prove the principle of ultimate stability on the basis of a female-only population, and from then on mainstream stable population theory has been set out almost exclusively in terms of female-only population. But the reason Lotka's one-sex theory became the mainstream is because it has been the one that has provided the first, if not the most reliable solution, for a variety of demographic puzzles such as the characteristic of renewal process, the parameters and principal relations in stable and stationary populations, intrinsic rate of natural increase, convergence of a population to stability, weak and strong ergodicity, the timing of fertility, the treatment of reproduction as a discontinuous process in work with very small populations, the mean age of childbearing and mean length of a generation, reproductive value, population momentum, and its application to a variety of demographic estimations from incomplete or deficient data in model mortality schedules, model fertility schedules and indirect techniques.

The demographic issues included in this already long list were raised and tackled first by Lotka's mainstream one-sex theory. Some of the prominent original works which contributed to this stream are the following: Arthur (1981, 1982a, 1984), Coale (1957a, 1957b, 1972), Dublin and Lotka (1925), Lotka (1939), Feller (1941), Fisher (1958), Keyfitz (1968, 1977b), Keyfitz and Flieger (1971), Lopez (1961), McCann (1973), Parlett (1970), Pollard (1973), Sharpe and Lotka (1911), and Thompson (1931). In addition to these works there are textbooks and collections of selected papers, such as those of Elizaga (1979), Kuczynski (1932, 1935), Newell (1986), Smith and Keyfitz (1977), Shryock and Siegel (1971), United Nations (1983), and Wunsch and Termote (1978).

With regard to the mainstream one-sex approach on stable population, perhaps the most important feature that needs consideration is the nature of its theoretical assumptions. In particular, I have asserted above that by taking the principle of separation between the sexes seriously Lotka's theory can be said to hold for its explicit and implicit assumptions. This position is at odds with the alleged inconsistent results that motivated the so-called 'two-sex problem'.

The alleged inconsistencies revealed by the one-sex approach of stable population can be attributed to Lotka's and his followers' lack of clarification of the full range of assumptions. To paraphrase Cohen and Stewart (1994: 186) the quality of mathematical conclusions is determined by a lot more than just the accuracy of the calculations.

> There are three types of mistakes. Errors made within the model are the *easy* type to stop. Harder are errors made in the explicit assumptions that lie behind the model. The hardest of all to stop are the implicit assumptions in the worldview that suggested the model (Cohen and Stewart, 1994: 186).

This is a very important distinction for an adequate discussion on whether the one-sex approach of stable population theory produces inconsistent results. For that purpose it seems useful to review how Lotka and his co-authors have set the one-sex model.

In 1911 Sharpe and Lotka resumed the issue addressed in 1907 in which the second of these two authors calculated the 'fixed' age-distribution under constant conditions established for its parameters. This time they where interested in showing whether the 'fixed' form, of age-distribution could also be regarded as a 'stable' distribution: 'that is, whether a given (isolated) population will spontaneously return to this 'fixed' age-distribution after a small displacement therefrom', proposed Sharpe and Lotka (1911: 98).

To answer this question Sharpe and Lotka started by establishing the equations for a more general problem, which they stated as follows:

> Given the age-distribution in an isolated population at any instant of time, the 'life curve' (life table), the rate of procreation at every age in life, and the ratio of male to female births, to find the age-distribution at any subsequent instant.
>
> 1. Let the number of males whose ages at time t lie between the limits a and $a+da$ be $F(a,t)da$, where F is an unknown function of a and t. Let $p(a)$ denote the probability at birth that a male shall reach the age a, so that $p(0)=1$. Further, let the male birth-rate (i.e. the total number of males born per unit of time) at time t be $B(t)$. Now the $F(a,t)da$ males

whose age at time *t* lies between *a* and *a+da* are the survivors of the *B(t-a)da* males born *a* units of time previously, during an internal of time *da*. Hence

$$F(a,t)da = B(t-a)\,p(a)da$$
$$F(a,t) = p(a)B(t-a)$$

2. Let the number of male births per unit time at time *t* due to the *F(a,t) da* males whose age lies between *a* and *a+da* be *F(a,t)β(a)da.* If γ is the age at which male reproduction ends, then evidently

$$B(t) = \int_0^{\gamma} F(a,t)\beta(a)da = \int_0^{\gamma} B(t-a)\,p(a)\beta(a)da,$$

Now in the quite general case *β(a)* will be a function of the age-distribution both of the males and females in the population, and also of the ratio of male births of female births (Sharpe and Lotka, in Smith and Keyfitz, 1977: 98).

The explicit assumptions underlying the equations in Lotka's model focus upon the amount of information needed to describe the long-term regime of unchanging schedules of fertility and mortality. However, in addition to the explicit assumptions provided in the previous quotation one can also identify the data necessary for the model to function. Since Sharpe and Lotka focused on male population the data and conditions were: the number of male population, in this case in England and Wales between 1871 and 1880; an invariable male life table; an invariable ratio of male to female births; an invariable rate per head of male procreation at every age in life.

Of course, these data make the stable population theory possible because there are several assumptions not immediately stated but undoubtedly necessary for the consistency of the model. In their paper, Sharpe and Lotka make just this remark with regard to their implicit assumptions: 'It may be noted that of course similar considerations apply to the females in the population' (Sharpe and Lotka, in Smith and Keyfitz, 1977: 100). While in 1911 Sharpe and Lotka had only discussed the convergence to stability in the case of small displacements in age distribution, in 1922 Lotka expanded the proof of stability to the case of large displacements.

Following his idea that the sex under consideration did not affect the conclusions drawn from the model Lotka wrote:

In a population of mixed sexes it is, of course, immaterial, numerically, to what parent each birth is credited. It will simplify the reasoning, however, if we think of each birth as credited to the female parent only (Lotka, 1922: 103)

In addition to this, Lotka (1922: 103) proposed to develop his argument 'on the supposition that *β(a)* is independent of the age distribution'. At the end of this article Lotka commented on the effect of variability in the form of the procreative factor, denoted *β(a)* and referring to the birth factor as the 'average number of births contributed per annum by a parent of age *a*', with changes in the coefficient of age distribution *c(t,a)*.

Some such variability undoubtedly exists owing to the influence of the ages of the male and female constituents of the population upon the frequency of matings ... Merely for purposes of defining *r,* we shall suppose that the function *β(a)* under the integral sign has that particular form which corresponds to the fixed age distribution (Lotka, 1922, in Smith and Keyfitz, 1977: 106).

Lotka was aware that his model contained certain ambiguities and he mentioned, in particular, the fact that a variable procreative factor $\beta(a)$ might lead to more than one real root for the natural rate of increase of the population r in his main renewal equation of stable population.

Lotka never discussed openly the tacit assumptions that lie behind his one-sex model; his most explicit position concerning the usefulness of the principle of separation between the sexes underlying his theory was that the one-sex model can be equally applied to either males and females. Even after Kuczynski (1932) illustrated, in a case discussed below, that the application of the one-sex model independently to males and females produced difference results, Lotka maintained that the option between one or another sex was irrelevant. In his most comprehensive work published in 1939 Lotka explained in chapter on 'relationships upon fertility':

> *Fertility and annual births* - It is convenient, at least for the time being, to treat our subject matter in application to one of the two sexes only. *For practical reasons we choose the population of feminine sex* (Lotka, 1939: 64) [emphasis added].

The last sentences stresses the core of Lotka's position, that is he applied the one-sex model to the female because this was more practical. This pragmatism is explained in a footnote added to the statement just cited in which Lotka, perhaps for the first time, enumerated the reasons why he found it more convenient to apply the one-sex model to the female rather than male: the reproductive period is more precisely defined among women than men; the maternal parenthood is more readily recognized, and illegitimate births are more readily attributable to mothers than fathers.

Throughout his book Lotka considered the actual fertility $m(a)$ independent of the proportion of the two sexes in the population; he considered this assumption a legitimate approximation in most of the cases:

> Finally, let us note again that if we have considered the function $m(a)$ as independent of the proportion of the two sexes in the population, it is a legitimate approximation in most of the cases because that proportion does not generally vary seriously. In extreme cases, however, these variations can play a more important role (Lotka, 1939: 89)

Still, in a chapter discussing the advantages and disadvantages on the indices and measures of natural increase, Lotka praised the merit of the net reproduction rates (denoted R_o) created by Böckh, but remarked: 'this measure is influenced by the mean length of a generation, and for this reason R_o calculated for the part of male population is higher than the one based on the female part' (Lotka, 1939: 102). Later, in a numerical example Lotka found the net reproduction rate based on female-only population equal to 1.166, and then he deduced a net reproduction rate for males equal to 1.194. Moreover, Lotka concluded, although in the long-run the male and female populations should have similar rates, for some periods there may be some discrepancies between the two rates of increase; so the approximate consistency among means of the age-specific fertility distribution, m = 1.260 calculated for males, the R_o

= 1.166 calculated for females, and the $R_0' = 1.194$ calculated for males, depends on the circumstances observed.

Kuczynski's experiment on the ineffable innocence of Lotka's implicit assumptions

Kuczynski, on different occasions (1932, 1935), justified the use of the one-sex approach to the female-only population differently from Lotka. In his book *Fertility and Reproduction* Kuczynski wrote:

> Total fertility includes births both of boys and of girls. For studies of the trend of fertility, it is advisable to restrict the investigation to the births of females, the potential future mothers. The best method, then, would be to relate the females borne by mothers of each specific year of age to the total number of women of that age (Kuczynski, 1932: 13).

In his book *The Measurement of Population Growth* published in 1935 Kuczynski also considered the calculation of net reproduction rates on the basis of female-only population more meaningful: 'Since we are concerned here with births only, it suffices to take into account the female population' (Kuczynski, 1935: 206).

Contrary to Lotka, Kuczynski did not insist on the applicability of the one-sex model to either sex. Kuczynski usually assumed that in measuring fertility and reproduction of a population, births should be related mainly to the female population only, perhaps for the same reason proposed by Knibbs and Moheau: the female component of population is the 'producing sex' in the demographic system.

Yet, in *Fertility and Reproduction* Kuczynski provides a case study on how to calculate the net reproduction rate for the total population which soon started to be used as the best illustration of the one-sex approach. The example was drawn from the French population in 1920-1923 and its results are summarized in Table 2.10.1.

In a sign of relaxation of his own views Kuczynski followed the ineffable innocence of Lotka's assumptions and applied independently to both sexes; he inclusively calculated crude birth rates for females and males separately. As Table 2.10.1 illustrates, the results of the net reproduction rates calculated independently for males of females appear rather different; this was Kuczynski's deduction:

> Both the birth rates and the net reproduction rates show a great discrepancy between females and males. How is this to be explained? It is due to the fact that, as a consequence of the World War, the men in the reproductive age of life were much less numerous than the women. More recently this disproportion between the two sexes in France has been reduced though the maturing of those males who were too young to serve in the war, and through immigration. As a consequence thereof, the difference between the number of children born to each 1,000 men and to each 1,000 women in reproductive age has decreased. And it will further decrease until all men born before 1901 will have passed the reproduction age, that is to say, until the balance between the two sexes in reproductive age which has been upset by the war is fully re-established (Kuczynski, 1932: 38).

However true, Kuczynski's explanation for the 'great discrepancy' between the results produced by the one-sex model applied independently to both sexes raises two important questions. One question is, where did Kuczynski get the explanation that the difference in results were caused by the consequences of the World War? Surely it was not from the amount of information put in, in the first place, from the starting axioms and rules of his model. This is the problem pointed out by Pagels (1989: 60) in a statement cited in Chapter 9: 'In mathematics one never gets out more information than one puts in the first place from the starting axioms and rules'.

To paraphrase Pagels, this suggests that a model specified by a certain amount of information should not be used to prove something about a system that requires a larger amount of information. This leads to the second question: is it correct to compare the results produced on the basis of one-sex models independently for females and males in a given population? Are the results produced by the one-sex model inconsistent, or is it the naive comparison of them that is inconsistent?

Table 2.10.1 Reproduction rate for both sexes, France, 1920-1923

	Female Population					Male Population				
Age Groups	Females March 6, 1921	Yearly Births 1920-23	Fertility rates	Females in stationary population	Births per 1000 female in stationary population	Males March 6, 1921	Yearly Births 1920-23	Fertility rates	Males in stationary population	Births per 1000 males in stationar population
15-19	1,719,248	41,267	24.00	4220.27	101.30	1,732,383	3,535	2.04	4145.64	8.46
20-24	1,641,524	215,751	131.43	4114.24	540.75	1,408,027	107,148	76.10	4029.09	306.61
25-29	1,554,521	238,381	153.35	3995.30	612.67	1,233,566	216,086	175.17	3898.99	682.99
30-34	1,514,556	162,291	107.15	3875.57	415.28	1,254,225	198,502	158.27	3773.98	597.30
35-39	1,498,813	96,032	64.07	3755.03	240.59	1,275,242	136,414	106.97	3637.55	389.11
40-44	1,442,321	34,785	24.12	3629.64	87.54	1,317,839	80,338	60.96	3479.99	212.15
45-49	1,332,892	3,056	2.29	3486.11	7.99	1,272,403	35,122	27.60	3296.94	91.01
50-54	-	-	-	-	-	1,133,217	14,418	12.70	3068.55	39.04
Total	10,703,875	791,563	506.42	27076.16	2006.12	10,626,902	791,563	619.81	29330.73	2326.66
				R_0	0.7923				R_0'	0.7508
	TFR	2.53					TFR	3.10		
	GRR	1.37					GRR	1.67		
	NRR	**0.977**					**NRR**	**1.194**		
			Boys	1053.3	1000	2053.3				
			Girls	949.4	1000	1949.4				
			NRR	**(total population) 1.085**						

Source: Kuczynski, 1932: 36-7

Kuczynski computed fertility assuming tacitly that each sex exists on its own; each sex takes care of and is independently responsible for providing its own descendants. Thus, he first applied the model to the female population assuming that women just produce female offspring (daughters of daughters of daughters). Then, Kuczynski computed 'a similar "fertility" table for males', this time assuming that men produce the male line of descendants (sons of sons of sons); so 'the son of a daughter of Elder Brewster is not taken as a descendant' (Keyfitz, 1977b: 10).

These rules illustrate the principle of separation of the sexes, the core principle in which the one-sex model stands. However, one cannot discuss adequately the results produced by the one-sex model without first considering the tacit assumptions that lie behind the model itself. First, the two sets of a given population defined by sex are different in their size, age distribution and reproductive role. The application of the one-sex model independently to both sexes tacitly assumes that males and females are not only different but separated. This means that the researcher's knowledge that the two sexes are part of the same population is irrelevant, numerically, for the consistency of the model.

Secondly, the validity of Kuczynski's comparison of the results for both sexes results is questionable. When two populations are different in their composition demographers developed a set of techniques commonly known as standardization. Kuczynski compared a crude birth rate of the female population, 18.9, with a crude birth rate of the males, 22.0, without controlling for the differences in the composition of the two populations. Kuczynski did the same with the net reproduction rates, though if he had first stripped off the differences in the age composition of the two populations his conclusion would probably be different: the standardized female and male net reproduction rates are 0.531 and 0.555, respectively.

So, is the alleged discrepancy statistically significant? Kuczynski did not contemplate this issue; nor did, it seems, any of the authors who have been using his example has the symbol of the inconsistencies produced by the one-sex approach. To give just one example, Pollard in 1973 interpreted Kuczynski's results as follows:

> The use of a one-sex model with the female component of the population would predict a continually decreasing population for France whilst the same model applied to the male component would predict a continually increasing population (Pollard, 1973: 82).

This is an inadequate interpretation that has nourished the debates on the 'two-sex problem' for too long. In particular, this inference from Pollard does not do justice to the one-sex model, mainly because it conceals some confusion about how facts and theory interact with social reality, or more accurately, how negative evidence provided by a given theory interacts with the scope of its own assumptions. As Walker and Cohen put it:

> a conditional theory is not false within its scope ...'true' theories cannot generate negative evidence within their scope ... a theoretical formulation is expected to be true: (1) If scope limitations are satisfied, inconsistent evidence falsifies the conditional formulation; (2) if scope limitations are not satisfied, all observation statements are consistent with either the truth or falsity of the formulation and hence, are irrelevant to it, and (3) if a formulation is true, negative evidence implies that some scope limitation is unsatisfied (Walker and Cohen, 1985: 288).

Indeed, the discrepancy between male and female results should be expected from the assumptions and conditions in which stable population theory is outlined. If the population is divided into two populations defined by their sexes and such populations differ in terms of size, age distribution, and their generation reproductive behaviour, any immediate similarity between their results is pure accident.

In any case, before moving on from the Kuczynski's case study it seems important to pay some attention his own final remarks. First, Kuczynski remarked that the net reproduction rate of the total population 'is not very significant and should, in any case, be used with great caution'; secondly, with regard to the application of the one-sex model to male population he concluded:

> It may be useful in connection with certain studies on differential fertility to include the males but for any general study of the balance of birth and deaths it seems preferable to relate births and deaths to the female population only (Kuczynski, 1932: 38).

Apparently, Kuczynski never returned in later works to this experiment. This can be interpreted as a rejection of the ineffable innocence of Lotka's argument of convenience regarding the usual application of the one-sex model to female-only population and a reliance on his own explanation.

Coming to terms with the assumptions of the one-sex approach on stable population

In general, following Lotka and his collaborators, the authors who have contributed to the development of the mainstream one-sex model in stable population theory have made no significant effort to overcome Lotka's failure to provide a better justification for it. Some can well argue that the mainstream one-sex approach on stable population theory hardly needs to be defended, mainly because Lotka's one-sex theory has survived scrutiny. However, this is a bad excuse for those who since Lotka have shied away from any attempt to provide a convincing theoretical justification for the explicit and implicit assumptions that lie behind their models. As Cohen and Stewart (1994: 186) put it, 'Impeccable mathematics can produce nonsense if it is based on nonsensical assumptions. "Garbage in, garbage out', as the computer scientists say'.

Whether Lotka's assumptions, explicit and implicit, are meaningful, his followers in the mainstream approach on stable population theory have not been convincing enough. Already late in 1940s Karmel (1948c: 51) complained that 'the reasons when given have always been stated baldly without explanation'. Hajnal also distanced himself from what he called 'reasons of convenience'; he maintained that the maternal indicators are founded on physiological reason and dismissed the 'reasons of convenience' as follows: 'Arguments of this kind, of course, provide no reason why computations based on women should be a better guide to the truth than those based on men' (Hajnal, 1948a: 355).

This was the time when the mounting dissatisfaction with the one-sex fertility and reproductivity measure were setting the grounds for the alternative research developed against Lotka's one-sex approach in the study of stable population. Such directions, illustrated in Figure 2.10.1, have gathered their own momentum but remained more or less at the margin of conventional demography. Contemporary courses and textbooks still do not mention the 'two-sex problem'. In more advanced treatments of stable population most authors proceed as if the controversy on the allegedly inconsistent results produced by the one-sex model had never occurred. This is apparent, for instance, in one of the most important contributions on 'why a population converges to stability' proposed some ten years ago by Arthur. In 1981 and 1982 Arthur outlined an elegant and self-contained proof of the convergence of the age structure based on the one-sex approach.

> Central to both mathematical demography and its sister subject, population biology is a single, fundamental theorem: If the reproductive and the survival age-patterns of a population remain unchanged over time, its age composition, no matter what its initial shape, will converge over time to a fixed and persistent form. In brief, when demographic behavior remains unchanged, the population, it is said, converges to stability. This is the Strong Ergodic Theorem of Demography (Arthur, 1981: 557).

Then, Arthur added: 'It is this theorem that makes *stable population theory* possible'. Of course, implicit in this possibility is the unspoken assumption that the central theorem of strong ergodicity can be adequately demonstrated on the basis of the one-sex model.[10]

Yet, among demographers who have contributed to the development of the mainstream approach on stable population theory, at least Keyfitz and Coale have sometimes countered the view that the one-sex method produces inconsistent results. On several occasions Keyfitz has maintained that the one-sex model exists not so much for convenience, as Lotka suggested, but perhaps because this is the best 'device' which 'gives clear answers to questions otherwise indeterminate'. The data gathered in Table 2.10.2 were extracted from Keyfitz and Flieger's impressive book called *World Population: An Analysis of Vital Data*.

As the authors explain, most of the tables in this book are compiled on the assumption of female dominance,

> which is to say the births by age of mother provided female age-specific birth rates, and these were applied to the female population for projections, direct standardization, intrinsic rates, and other purposes. The results may be thought of as essentially a one-sex calculation; the female population is first projected and then a number of male births introduced at each stage in the ratio of male to female births in the year of the data (Keyfitz and Flieger, 1968: 641).

Keyfitz and Flieger's (1968) book seems to be the only one where there is some mention of the question of standardization of the results calculated independently for both sexes from the same population.

> The only point at which there is even a suggestion of relating births to fathers is in the indirect standardization. The results are a crude approximation to what would be obtained with a proper two-sex calculation (Keyfitz and Flieger, 1968: 641).

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[10] The fact that Arthur found it not necessary to explicate the tacit assumptions that lie behind his one-sex model is somewhat ironic and curious. On the one hand, Arthur enjoys the reputation of maverick, among conventional economists (Casti, 1994: 41), mainly because of his rejection of the neoclassical wisdom that 'negative feedbacks' lead to stability and equilibrium in the supply and demand in market prices. Arthur has maintained that this is not at all the way the real economy works; rather Arthur has claimed that 'positive feedbacks' from the supply and demand relationships lead to unstable price equilibria (Arthur, 1989, 1990; Casti, 1994: 41). On the other hand, while Arthur stands currently among the most respected scientists in the field of complexity and dynamical systems, in demography he has remained in the mainstream and left the maverick approaches for others. The curious aspect of this is that some of the maverick two-sex alternatives longing to replace classical stable population theory have turned to Arthur's recent papers support despite the fact that they deal with the one-sex models as given. For instance, Pollak wrote in 1990:

> Demography's two-sex problem is a fundamental anomaly that can be resolved only by replacing classical stable population theory with a model that recognizes that the observed rates for both females and males are in desiquilibrium (Pollak, 1990: 401).

Pollak has attempted to replace the classical stable population theory for which Arthur offered a new proof which he even uses in his article. After all, it is striking that in a theory considered 'ill suited ... because it is a "one-sex" theory', Pollak (1990: 399) still provides so much inspiration and insight for those who believe its anomalies can be solved by a two-sex model.

Table 2.10.2 Male dominant vesrus female dominant period calculations

	Male	Female	Male	Female	Male	Female	Male	Female
USA	**1959-61**		**1962**		**1963**		**1964**	
GRR	1.963	1.717	1.896	1.696	1.838	1.624	1.777	1.567
NRR	1.837	1.66	1.779	1.634	1.724	1.564	1.666	1.51
TFR	3.833	3.528	3.705	3.474	3.585	3.333	3.473	3.208
Generation	29.067	26.11	29.074	26.076	29.153	26.17	29.222	26.25
Sex ratio	104.947	105.468	104.795	104.795	105.272	105.272	104.719	104.719
Chile							1964	
GRR							2.751	2.18
NRR							2.167	1.824
TFR							5.416	4.43
Generation							32.839	28.553
Sex ratio							103.235	103235
nidad & Tobago	**1956-58**							
GRR	3.063	2.643						
NRR	2.665	2.43						
TFR	6.006	5.402						
Generation	32.09	26.864						
Sex ratio	104.11	104.405						
Cyprus	**1956-58**							
GRR	1.916	1.7						
NRR	1.777	1.602						
TFR	3.746	3.481						
Generation	27.602	27.993						
Sex ratio	104.734	104.734						
Hungary							**1964**	**1965**
GRR							0.99	0.876
NRR							0.916	0.833
TFR							1.913	1.808
Generation							29.794	25.756
Sex ratio							107.244	106.516
Norway					**1963**			
GRR					1.454	1.419		
NRR					1.384	1.383		
TFR					2.843	2.905		
Generation					31.204	27.595		
Sex ratio					104.676	104.676		
U.K.			**1960-62**					
GRR			1.406	1.347				
NRR			1.336	1.305				
TFR			2.731	2.777				
Generation			30.314	27.253				
Sex ratio			106.101	106.101				

Source: Keyfitz and Flieger, 1968.

Table 2.10.3 provides a summary of the standardized figures for three countries with United States in 1964 as a standard. Indirect standardization, in which the age distributions of the given country are applied to the age distribution, is signified by a grey shade in rows for males females. Keyfitz and Flieger were wise enough to point out the differences in the results without declaring them inconsistent.

> The comparison of male- and female-dominant results is affected at many points of our printout by any tendency for one of the sexes to increase even slightly more rapidly than the other. For the United States in 1964 the intrinsic rate is 15.70 for females and 17.48 for males, a difference of 1.70, all per 1000 population. At the end of 102.5 years the female rate would multiply the population by 5 times its starting value, and the male rate by 6 times (Keyfitz and Flieger, 1968: 642).

Table 2.10.3 Standardized population, United States 1964

	Female dominant			Male dominant		
	Birth	Death	Increase	Birth	Death	Increase
1. Observed population						
Both sexes	21.05	9.4	11.65	21.05	9.4	17.48
Males	21.92	10.83	11.09			
Females	20.2	8.01	12.19			
Intrinsic rate	23.48	7.78	15.7	24.78	7.31	17.48
2. Standardized rates ('1000)						
England and Wales 1961						
Both sexes	20.60	11.31	9.08	22.32	11.31	11.02
Males	23.92	12.16	11.76	23.59	12.23	11.36
Females	19.30	10.44	8.86	19.65	10.59	9.06
United States 1960						
Both sexes	20.78	9.29	11.49	21.44	9.29	12.15
Males	22.21	10.84	11.37	22.27	10.85	11.42
Females	20.2	7.78	12.22	19.93	7.77	12.16
Mexico 1960						
Both sexes	25.05	5	20.05	22.88	5	17.88
Males	21.03	8.63	12.4	23.46	6.09	17.37
Females	24.41	3.91	20.5	22.71	5.54	17.17

(shaded) The age distribution of the given country applied to the age distribution of the given country

Source: Keyfitz and Flieger, 1968

Keyfitz (1977b), in his *Applied Mathematical Demography,* illustrated his position with regard to the use of the one-sex model as compared with the two-sex model with an example on 'the descendants of the Pilgrim Fathers'. Keyfitz considers that a treatment of both sexes simultaneously introduces great uncertainties; with his example on the Pilgrim Fathers Keyfitz attempted 'to illustrate the indeterminacy of a two-sex model'.

> This is not to imply that the other sex is not necessary, but rather to suppose that it exists in whatever numbers are required to produce the growth in the sex being followed (Keyfitz, 1977b: 10).

Keyfitz concluded his example on the Pilgrim Fathers as follows:

> That the one-sex problem gives the simple and unique answer of 50,000 on our assumptions, whereas the corresponding two-sex problem leaves us in the range of 50,000 to 50 million, is only one aspect of the difficulty. Another is the effect on marriages and births of adding a number of males to a population, as against the (presumably greater) effect of adding the same number of females. Hunting female rabbits affects reproduction more than hunting male rabbits - how much more depends on how actively the remaining males get around. Satisfactory answers to such questions are not easily found. They cannot be obtained without facts or assumptions regarding individual behavior of a more detailed kind than demography ordinarily introduces ... The fact that the one-sex model gives simple answers to difficult questions, and that under a

> considerable range of circumstances these answers are realistic, makes it a positive achievement (Keyfitz, 1977b: 11-12).

Still Keyfitz, in a 1982 article published in the *International Encyclopedia of Population*, commented on the 'two-sex problem' not as a euphemism of the one-sex problem but as the difficulties of the two-sex modelling itself.

> The contemporary period has reached beyond Lotka in several directions. It has gone further with cohorts as well as with time periods; it has tackled the two-sex problem (although that seems to be beyond any simple solution) ... Acceptance of dominance avoids the essential difficulty of the two-sex problem, on which there is a large literature. Any linear model that takes account of both parents runs into difficulties when the sex ratio departs substantially from unity; if births depend on the mean number of men and women, then if one sex drops to zero the births are reduced only by half where they should drop to zero. Yet nonlinear models seem impossible to handle mathematically. Aside from technical difficulties, the number of offspring depends on behavior that is not embraced by any mathematics using presently available. Number of offspring of any species, including the human species, usually depends more on the number of females than on the number of males, but how much more is determined by how active the males are (Keyfitz, 1982: 438, 441).

In turn, in 1972 Coale published an important mathematical investigation on *The Growth and Structure of Human Populations*. In a chapter discussing 'some uses and limitations of the stable population' Coale defended 'the hypothetical nature of stable populations':

> The stable population, as noted earlier, is the population that is established by a prolonged regime of unchanging schedules of fertility and mortality. Since actual schedules always change, the stable population must be regarded as hypothetical: It is the population that *would* result if specified schedules of fertility and mortality were to persist. Perhaps the most useful view of the concept is as a particularly elaborate set of implications of any pair of schedules (Coale, 1972: 51).

In other words, this explanation counters the unspoken naive belief in a model independent reality. Furthermore, Coale addressed the allegedly inconsistent results produced by stable population and raised the possibility 'that the use of the term "true rate of increase" and the general tone of the discussion gave an inappropriate flavor of prediction to the characteristics of the stable population' (Coale, 1972: 52).

> It is always unlikely, and often logically impossible, for specific schedules of fertility and mortality to remain unchanged for a long time. However, it would not be wise to conclude that the concept of a stable population is useless. The proper conclusion is that the stable population should rarely (if ever) be interpreted as a prediction. A reading of 60 mph on a speedometer means that the automobile bearing it would travel 60 miles in an hour if velocity were held constant. Because cars usually travel at varying speeds, a reading of 60 would only rarely be a valid prediction; nevertheless, the speedometer is a useful instrument, and so is the stable population if properly used.
>
> Demographers and actuaries have long been aware of the hypothetical nature of the *stationary* population, which, hypothetical or not, is a valuable framework of analysis. The knowledgeable user never makes the mistake of thinking that the expectation of life at birth in a 'period' life table is the mean age at death of any actual population. It is the average age at death only in the hypothetical population defined by the mortality schedule (Coale, 1972: 52).

With regard particularly to 'male and female stable population' Coale considered the application of the one-sex model to both sexes independently. 'However', Coale (1972: 55) remarked, 'the two stable populations ... are not necessarily the same, and in fact normally differ, sometimes substantially'. Coale attributed the basic difference to

> the different intrinsic rates of increase ... associated with the current fertility and mortality of the two sexes, a difference originating in the fact that the balance of the sexes in the reproductive ages is often not the same in the actual population as in either of the two stable populations (Coale, 1972: 55).

In short, Coale focused on differences in the 'balance of the sexes' in the two stable populations derived from the one-sex model and those in the actual population: 'an imbalance of the sexes must be offset by an equal imbalance of fertility' (Coale, 1972: 55). In addition to this, Coale considered that the balance in the reproductive ages in the actual population and the stable population have very different sources: the former depends on the recent history, often including different gains or losses of one sex or the other through migration, and episodic sex-selective mortality, especially as a result of war; the latter depends on the current sex ratio at birth, on the relative survival rates, and on the rate of increase of the stable population.

However, Coale (1972: 56) insisted, a difference between the male and female intrinsic rates of increase 'need not be paradoxical, but merely as an indication of an imbalance of the sexes at parental ages as compared to the balance inherent in the current mortality schedules'. Coale finished his remarks commenting that the concept of the stable population is self-contradictory. In the end, Coale concluded that whatever the computations and alternatives one may propose for the one-sex model it cannot be based on observed schedules.

> It would be necessary to make assumptions about the influence of the age and sex distribution of fertility. That there is such an influence no one can doubt, but by its nature it cannot be observed directly as can fertility and mortality schedules. Hence a stable population derived from the fertility of both sexes is *not* a population inherent in observed fertility and mortality schedules, and is therefore different in concept from the stable population as we have defined it in this chapter (Coale, 1972: 58).

Vincent and Karmel: the fountainhead of the two-sex problem

The third stream of approach on stable population is the result of the mounting dissatisfaction with the estimate of fertility and reproductivity as well as stable population theory on the basis of one-sex methods. That is, as Karmel wrote in his Ph.D thesis ‘the conflict between male and female measures’ based on what he called the female and male systems set to describe independently from one another the future behaviour of a given population:

> There is no reason why the two systems should yield equal male and female true rates of increase ... and in practice these values turn out to be different and often quite different.

> Consequently the estimate of the reproductive potential of the population based on the female part of the population will be in conflict with the estimate based on the male part of the population. This is then the conflict between the male and female measures of reproductivity and it can be referred to as the 'male-female conflict' (Karmel, 1948c: 30).

After stating the conflict Karmel enumerated seven questions which he discussed in his thesis; the first two of those questions are of interest at this stage to the content of this chapter. First, 'What has been the attitude of demographers to this conflict in the past?'. Second, 'Is there any reason for preferring a female to a male system or vice versa and hence avoiding the conflict?'. Karmel dedicated Chapter 3 of his Ph.D thesis to the first question, and Chapter 4 to the second question.

The reason I have not discussed the issues raised by Karmel before should already be apparent. That is, if one believes that there is no way to describe, to say nothing of explaining and predicting, population behaviour independent of theory; and if one also accepts that a theory is not false within the scope of conditions set by its fundamental principles, such conditions should be applied consistently. Although Karmel himself admitted there is no reason why the two systems should yield equal male and female rates of increase, he regarded this contradictory in line of the widespread belief that the application of the one-sex system to females is for convenience only.

In setting the research problem for his thesis Karmel started by setting up the 'fundamental question of demography':

> *At what rate can a particular population at a particular time be said to be reproducing itself, given the fertility and mortality conditions obtaining in the particular population at the particular time under consideration?* It should be noted that this question excludes the influence of migration and hence refers to a closed population. What we require, therefore, is to establish a system or model which will describe the future behaviour of a population subject to certain given fertility and mortality conditions. From this behaviour we could then estimate the reproductivity potential of the population under consideration (Karmel, 1948c: 1).

After reviewing Lotka's theory of the stable population and setting his own research problem as the 'conflict between male and female measures of reproductivity', Karmel traced the origin of this conflict and for that classified the matter into main groups: the 'direct recognition of the conflict', and the 'indirect recognition via nuptiality'. In the first group Karmel started by inferring that Lotka was 'aware of the possibility of the existence of a male-female conflict' (Karmel, 1948c: 34). Especially since 1922, when Lotka started to apply the one-sex model to females only, 'he assumes that the female fertility conditions are independent of the sex age distribution of the population, although sometimes he recognizes that this may not be true' (Karmel, 1948c: 36). And, at least on two occasions (1931 and 1939) Lotka estimated the true rate of increase from the male part of the population and 'noted that somewhat different values for the true rate will be obtained in general from calculations on the male and female parts of the population' (Karmel, 1948c: 36). 'However', Karmel adds, 'it is clear that Lotka does not attach much importance to this conflict. He pays virtually no attention to it in all his practical computations' (Karmel, 1948c: 37). Karmel expresses his puzzlement towards Lotka's reluctance to even discuss the matter theoretically, 'which has been Lotka's main contribution to demographic science' (Karmel, 1948c: 37).

In addition to Lotka, Karmel mentioned other authors who directly recognized the male-female conflict: Fishe, Kuczynski, Husson, Rich, and Tieze. But

> It was not until 1941 that a paper directed entirely to the problem of the male-female conflict appeared. In that year R. J. Myers published his paper, 'The validity and significance of male net reproduction rates' (Karmel, 1948c: 39-40).

And then

> In 1946 P. Vincent's paper 'De la mesure du taux intrinsèque d' accroissement naturel dans les populations monogames' appeared. Like Myers' work, this was directed primarily to the examination of the male-female conflict, but Vincent's treatment of the problem showed much more insight than Myers' and he clearly realised the important implications of the conflict. He pointed out that there are no good logical reasons for preferring the use of rates based on the female part of the population to the use of rates based on the male part when the population is monogamous and that in fact calculations of the true rate of increase based on males and females often give very different results (Karmel, 1948c: 42).

It is important to notice that this review appeared in Karmel's Ph.D thesis submitted in 1948, that is a year after he published his first article on this matter in *Population Studies*, without being aware of Vincent's paper; so in his thesis Karmel also referred to the works from Hajnal, Hajnal and Hopkin, and the from A. Pollard which was somewhat motivated by his own work.

With regard to the 'indirect recognition via nuptiality' Karmel mentioned the works from Yule, Bortkiewicz, Connor, Somogyi, Depoid, Honey, Quensel, Hyrenius, and Hajnal. Karmel considered that these and many other demographers had at some time or another recognized the conflict between male and female measures of reproductivity in one of its forms. Yet, Karmel remarked,

> In view of the widespread recognition of the conflict, it is more than a little strange that there has been no real attempt to justify the use of female measures or to show that the conflict is of no practical importance. Both Lotka and Kuczynski, to whom more than anyone else is due the modern development of demographic science, were aware of the conflict; but carried on their researches as if it did not exist. It is difficult to explain this by saying that data concerning the ages of the male parents have not been available for the calculation of male measures, for such data have been available for some countries and in any case where the data are not available substitute methods for the calculation of the male measures could easily have been used. Neither can it be explained by saying that when actual cases of conflicting male and female true rates of increase have been noted the magnitude of the conflict has been unimportant, for quite the contrary has been the case ... The conclusion may seem to be that demographers have ignored the conflict simply because to recognise it fully would have cast doubt on the validity of the techniques which they were developing and using. But to draw this conclusion would not be altogether fair, for workers like Lotka and Kuczynski do seem to have believed that female measures were fundamental in a way that male ones were not, a belief which will be examined in the next chapter (Karmel, 1948c: 49-50).

In Chapter 4 of his thesis Karmel discussed the 'reasons given by demographers for the use of female measures of reproductivity' which are summarized in Box 2.10.1. 'If it could be shown that male systems describing the future behaviours of a population were meaningless or unrealistic', so Karmel started his Chapter 4,

> then the male measures derived from them could be regarded as without significance and female systems and female measures derived from them could be regarded as correct and hence no conflict would arise. Demographers have given various reasons for using female measures to the exclusion of males ones. These reasons when given have always been stated quite baldly without explanation and are set out and considered in turn below (Karmel, 1948c: 51).

Box 2.10.1 Reasons given by demographers for the use of female measures of reproductivity

1. *Reproductivity is concerned with the production of offspring and this is a function of the female sex* - This reason is given by Kuczynski and he appears to rely on it solely. He says, for example:

> Since we are concerned here with births only, it suffices to take into account the female population (Kuczynski, 1928: 42, cited in Karmel, 1948c: 51).

> and

> Total fertility includes births both of boys and of girls. For studies of the trend of fertility, it is advisable to restrict the investigation to the births of females, the potential future mothers. (Kuczynski, 1932: 13).

This argument must also be in the minds of those who believe that the female measures of reproductivity are in some ways more fundamental than male measures. For example, Rich says:

> Too much significance should not be placed on the male rate; it seemed that the fertility rates of males were less fundamental than those of females (Rich, 1934: 73)

2. *The female reproductive period has more definite limits and has an earlier termination than the male one* - Lotka seems to rely mainly on this reason. Thus he says:

> the computation of the true rate of natural increase is most conveniently conducted on the basis of the female population (mothers and daughters), because the reproductive period of women is shorter and more sharply defined than that of men (Lotka and Dublin, 1936: 247)

3. *The ages of mothers are more readily available than the ages of fathers and in the case of illegitimate births the latter age are generally not known* - Lotka has referred to this as a reason on one occasion.

4. *There is no need to take males into account because it can be assumed that male births run parallel to female births* - This argument was explicitly stated by E. C. Thodes (s.c.):

> we need only concern ourselves with the changes in the number of females assuming that the number of males will run parallel with that of females. It is also to be found implicitly in a number of places: Dublin and Lotka, Rich, Wicksell, Linder, Glass, Population and Movements and Policies.

Another way of putting this reason is to say that the theory of stable population 'holds separately for each sex and it is convenient to restrict one's attention to the females' (Lotka, 1927: 158).

Karmel, 1948c: 51-56

As Karmel mentioned each of the above examples he exposed their weak theoretical argumentation, though time and again he appealed to the authority of commonsense such as in the following case:

> It seems extraordinary that Kuczynski and his followers were satisfied with such a reason for excluding male measures from consideration and yet the above statements are to be found in a number of standard works on demography. It certainly must have seemed peculiar to any layman reading the works, for if female births are potential future mothers are not male births potential future fathers? Every birth must have two parents. What seems to have been behind this very weak reason is the idea that the females do represent the limiting factor in population growth, although this has never been explicitly stated as a reason for using female measures (Karmel, 1948c: 52).

Karmel admitted that there is ‘a more or less definite upper limit to female fertility ... No such definite upper limit exists for males. The argument might then run:

> the birth-giving capacity of a population is limited rather by the number of females than by the number of males - a relative shortage of females will more obviously reduce the birth rate than a similar shortage of males, i. e. , the birth rate will be a function of the number of females and to a much lesser extent of the number of males. Now this may be all very well in an animal population or in one with complete sexual promiscuity, but in human populations in which monogamy is practised this argument breaks down - a relative shortage of males will reduce the birth rate by reducing the proportion of females who can marry in the same way as a similar shortage of females, so that the birth rate must be regarded as a function of the numbers of both males and females. This argument must also be in the minds of those who believe that the female measures of reproductivity are in some ways more fundamental than male measures (Karmel, 1948c: 52-53).

In short, Karmel found that on closer examination none of the reasons which demographers have given for using female measures have

> any elaboration as if they were obvious ... none of them is found to be adequate and they really only amount to saying that female measures are used because it seems sensible to estimate reproductivity in terms of females or because it is convenient to do so. Nevertheless it is evident that behind the reasons lies the idea that female measures are in some way more fundamental than male ones ... Thus, not only have demographers put forward reasons for adhering to female measures which are unconvincing, but they have themselves on occasions proved to be unconvinced by making use of male measures (Karmel, 1948c: 59).

At this stage of his thesis Karmel had clearly established the theoretical grounds for the new endeavour aiming to tackle the fundamental question mentioned above in a rather different way from the mainstream one-sex method. I return to Karmel’s thesis in Chapter 11, and thus the review of his arguments should be enough to give a picture of the origin of the so-called ‘two-sex problem’.

With regard to the fourth and fifth streams important for the development of stable population theory there is no space to discuss them here in the same detail as those already considered. Indeed, this is not even necessary for the purpose of this chapter. There are, however, at least two features in the remaining two streams of theories on stable population related to the important contributions from A. Pollard (1948) and Feeney (1983) that deserve to be stressed. First, regardless of the specific assumptions underlying both approaches, like the previous three they deal with what I have already defined as the description and measurement of demographic outputs. Secondly, all these approaches recognize the importance of demographic concepts such as ‘fertility’ and ‘reproductivity’ and despite their sharp criticisms of the mainstream approach neither rejected its usefulness and validity completely.

On the contrary, all the authors who have come up with some alternative models to that first set out mainly by Lotka have expressed their great appreciation of pioneering work. Beyond that, it would be cynical to dismiss the validity of their alternatives on the grounds that they have been inspired and make much use of the theory they long to replace. Curiously, Feeney's parity progression model provides a complete, formal alternative even to conventional age-based approaches to the study of fertility and population growth; he did this after having himself attempted to tackle the matter from a two-sex perspective but considering age. One should not be surprised if someone proposes a two-sex alternative to Feeney's one-sex parity progression model and then arrives at a result somewhere between those provided by Lotka's and Feeney's models.[11] Whether demographers will ever get agree on a single and widely-accepted model to study population dynamics remains to be seen. But, without doubt, even if demographers were to agree now on a single model and universal model for them to study issues on demographic outputs, such as population stability and the level and trends of fertility and reproductivity, the core question addressed by this thesis would remain untouched: why and how a certain rate has happened in a particular population at a particular time. This is an explanatory issue that goes beyond the scope of the tools developed so far in demography to describe and measure the structure of population.

Diversity and wealth of one-sex demography: why woman is the indispensable sex

As in previous chapters with regard to neuter demography, Chapter 10 has given a brief characterization of one-sex demography and its validity within demography in general. I have concentrated the discussion on the part of one-sex demography that has been more controversial among demographers: the one-sex approach on stable population theory. In summing up, I have challenged the widespread argument in demography that the one-sex approach can be applied to either sex and that fertility is usually measured for women because it is more convenient, or it is easier than doing it for men.

Technically speaking, it is true that the one-sex model can be applied to either sex. However, the argument of convenience reflects a very superficial level of inquiry and has led to much misunderstanding. By taking the principle of separation between the sexes seriously Lotka's theory can be said to hold for its explicit and implicit assumptions. This leads to the conclusion that the attempts to compare results derived from the application of one-sex models independently to both sexes are not consistent with the theoretical setting of the one-sex approach. Not only are the two sets of the population defined by sex hardly comparable with regard to their age distribution; but even more significantly, males and females play distinctive roles in reproduction, which the one-sex approach is unfit take into account.

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[11] In his numerical comparison Feeney arrived for United States in 1970 at a stable growth rate of 0.62% on the basis of the Lotka model and 0.57% on the basis of the parity progression model; in the case of Costa Rica in 1963 he estimated a stable growth rate of 3.66% on the basis of the Lotka model and 3.44% on the basis of the parity progression model.

Here, the distinction between demographic outputs and demographic outcomes becomes crucial. The former refers to demographic issues concerning the description of population size, growth and structure; or in the case of fertility, its magnitude and direction. The latter refers to the explanation of important clusters of practices, attitudes and knowledge of both sexes which can be said to determine how and why changes in demographic output have happened. All demographic outputs have demographic outcomes, but not all outcomes result in outputs. When the objective is to study demographic outputs, as has been generally the case in conventional demography, the one-sex approach provides adequate results; and behind this success is the fundamental idea that only the component of population exposed to 'risk' of having children can produce demographic outputs.

Perhaps the most fatal weakness in setting up the conditions of stable population theory is related to the significance of the principle of separation of the sexes. However important the social condition may be, it is remarkable that demographers find it so difficult to admit that the crucial basis of separation of the sexes is that the power of reproduction is immediately determined by females, or as Knibbs call them, the 'producing sex'. Contrary to the assumption of unchanged fertility and mortality into the future, the nature of sexual reproduction is not a hypothetical condition but a characteristic of human population. To argue that this is incorrect because in reality the sexes do not exist on their own, nor do they take care of and be independently responsible for providing their own descendants, entails some confusion between the purposes of descriptive and explanatory areas of demography.

The twentieth century can well be seen as the century of the one-sex demographic theory; but even more important it has been the century in which the producing sex has assumed widespread recognition in demography for its importance in understanding population structure. Overall, demographers have treated women as the indispensable sex for a systematic study of population structure because the load of population composition is determined primarily by the producing sex, and modified to a greater or lesser extent by males. However obvious this observation may appear to commonsense, without the one-sex approach there would not be any adequate way to demonstrate it adequately.

These two developments, the rise of the one-sex approach and the widespread feminization of demographic theory in the study of population reproduction, survival and movement, have become the source of significant breakthroughs in the history of demographic theory during the twentieth century. However, to move into any detailed discussion on other expressions of one-sex demography would take this thesis too far afield. Instead I finish this chapter with two summary tables. Table 2.10.4 provides some measures of fertility, mortality, and reproduction, which fall in this thesis into the designation one-sex demography. Like in the case of Table 2.9.2 in Chapter 9 the data refer to Asia Pacific Region only.

Table 2.10.4 One-sex measures of fertility, mortality, and reproduction female Asian Pacific population (a)

Country	Years	Reproduction rates		Mean	Expectati	Vital rates of female population				
				Age of	of life at	reproduction, birth, death				
		Gross	Net	Fertility	birth	intrinsic			crude	
						r	b	d	b	d
Hong Kong	1956-59	2.31	-	-	-	-	-	-	-	-
	1960	2.49	-	-	-	-	-	-	-	-
	1961-63	2.47	-	-	-	-	-	-	-	5.3
	1969	1.81	1.74	29.5	I 73.4	19.1	26.0	6.9	20.3	4.3
	1970-74	1.63	1.58	29.2	74.4	15.8	23.4	7.5	19.1	4.6
	1975	1.20	1.17	29.0	L 75.0	5.5	16.4	10.9	17.7	4.5
	1976	1.24	1.21	28.8	L 75.0	6.6	17.1	10.5	17.3	4.9
	1977	1.17	1.14	28.6	L 75.0	4.4	15.8	11.4	17.4	4.9
Japan	1930-4	2.32	1.62	30.4	48.2	16.3	33.8	17.4	-	-
	1935-39	2.08	1.49	30.6	49.8	13.3	30.2	17.0	-	-
	1947-49	2.13	1.73	30.2	57.8	18.5	30.8	12.3	32	11.9
	1950-54	1.46	1.30	29.4	65.1	9.1	21.0	12.0	22.5	8.8
	1955-59	1.04	0.96	28.5	68.6	-1.3	13.9	15.2	17.3	7.2
	1960-64	0.96	0.91	27.8	71.5	-3.3	12.3	15.6	16.4	6.6
	1965-69	0.98	0.95	27.8	73.9	-2.1	12.5	14.6	16.9	6.1
	1970-74	1.03	1.01	27.6	75.8	0.3	13.4	13.1	18.1	5.9
	1975	0.93	0.92	27.4	L 76.3	-3.3	11.5	14.8	16.2	5.7
	1976	0.89	0.88	27.4	77.4	-4.8	10.6	15.4	15.5	5.7
	1977	0.87	0.86	27.5	L 77.4	-5.5	10.3	15.8	14.8	5.5
Malaysia, Peninsula	1966-69	2.54	2.29	29.4	66.3	29.0	36.3	7.3	35.1	6.6
	1970-74	2.34	2.10	29.5	68.9	25.7	33.3	7.6	31.5	5.8
	1975	2.10	1.92	29.4	70.8	22.5	30.0	7.5	29.8	5.3
	1976	2.07	1.90	29.4	71.4	22.3	29.6	7.3	30.0	5.2
Singapore	1963	2.63	2.39	-	K 68.4	-	-	-	34	4.8
	1964	2.52	2.29	-	-	-	-	-	32.4	4.8
	1965-69	2.01	1.89	29.3	70.4	22.2	29.2	7.0	26.3	4.5
	1970-74	1.39	1.31	28.7	70.1	9.5	20.0	10.5	21.9	4.4
	1975	1.02	0.96	28.3	L 70.0	-1.6	13.4	15.1	17.5	4.1
	1976	1.03	1.00	28.1	73.9	0.0	13.5	13.6	18.4	4.3
	1977	0.89	0.86	28.1	L 73.9	-5.2	11.0	16.2	16.3	4.4
Sri Lanka	1965	2.36	2.09	29.4	L 64.4	25.6	34.1	8.5	33.9	7.8
	1966	2.31	2.09	29.5	I 66.5	25.6	33.3	77.7	33.0	7.9
	1968	2.28	2.07	29.4	L 66.9	25.4	33.0	7.6	32.7	7.3
Taiwan /10	1955-59	3.01	2.67	30.3	65.0	33.3	40.7	7.4	42.8	7.6
	1960-64	2.62	2.4	29.5	K 67.5	30.3	37.0	6.7	37.0	5.9
	1965-69	2.15	2.02	28.1	69.9	25.3	32.1	6.6	30.0	4.8
	1970-74	1.67	1.59	27.3	71.8	17.3	25.0	7.8	25.3	4.1
	1975	1.34	1.29	26.6	73.0	9.7	19.5	9.8	22.8	3.9
	1976	1.49	1.44	26.5	73.1	13.9	22.3	8.4	26.4	4.0
	1977	1.31	1.26	26.3	73.1	8.9	18.9	10.0	24.0	4.0
	1978	1.31	1.26	26.3	73.5	8.9	18.8	9.9	24.5	3.9
	1979	1.28	1.24	26.2	73.0	8.2	18.5	10.3	24.6	3.9
	1975-79	1.35	1.30	26.4	73.1	10	19.6	9.6	24.5	3.9
Australia /1,20	1930-34	1.11	1.01	28.8	66.9	0.4	15.2	14.8	17.4	7.9
	1935-39	1.06	0.98	28.4	67.9	-0.9	14.3	15.2	17.0	8.5
	1946-49	1.46	1.38	28.3	70.8	11.5	21.2	9.7	22.8	8.7
	1950-54	1.53	1.46	28.1	72.1	13.7	22.4	8.7	22.7	8.3
	1955-59	1.64	1.57	27.9	72.8	16.5	24.2	7.7	22.3	7.8
	1960-64	1.64	1.59	27.5	74.0	17	24.3	7.3	21.7	7.8
	1965-69	1.41	1.37	27.3	74.2	11.5	20.4	8.9	19.3	7.9
	1970-74	1.31	1.28	26.9	74.6	9.1	18.7	9.6	19.5	7.7
	1975	1.08	1.06	26.7	75.9	2.1	14.3	12.2	16.5	7.0
	1976	1.01	0.99	26.8	L 75.9	-0.4	13.0	13.4	16.0	7.2
	1977	0.99	0.97	26.9	L 75.9	-1.3	12.5	13.8	15.6	6.9
	1978	0.96	0.95	27.0	77.2	-2	12.0	14.0	15.4	-
New Zealand	1930-34	1.10	1.01	29.6	68.0	0.4	14.9	14.6	-	-
	1935-39	1.07	0.99	29.2	68.7	-0.3	14.4	14.7	-	-
	1946-49	1.64	1.56	29.0	71.5	15.5	23.8	8.3	24.9	8.4
	1950-54	1.69	1.62	28.3	72.7	17.3	24.8	7.5	23.9	8.3
	1955-59	1.89	1.82	27.9	73.3	21.9	28.2	6.3	24.3	8.1
	1960-64	1.93	1.87	27.6	73.9	23.1	29.0	5.9	25.0	8.0
	1965-69	1.63	1.58	27.0	74.5	17.3	24.4	7.1	22.0	7.8
	1970-74	1.41	1.37	26.4	74.6	12.2	20.7	8.6	20.6	7.7
	1975	1.14	1.11	26.2	L 74.6	4.0	15.6	11.6	17.9	7.3
	1976	1.10	1.07	26.2	L 74.6	2.4	14.7	12.3	16.9	7.3
	1977	1.09	1.06	26.3	75.5	2.2	14.5	12.2	16.9	7.4
	1978	1.02	1.00	26.4	L 75.5	-0.1	13.2	13.2	15.9	7.1

Notes to Table: K Values of exp. of life at birth taken from Keyfitz and Flieger (1968)

L Latest exp. of life at birth is used to compute NRR and intrinsic rates

I Interpolated

Source: Population Index', Vol. 47, Nº 1 (january) 1981: 402-414.

(a) This table has been drawn from Francisco, 1

In turn, Table 2.10.5 sets out a brief chronological overview of population theories consistent with the principle of separation of the sexes and, thus, part of the one-sex demography. The table includes a range from approaches drawn from commonsense to some of the most recent one-sex population theories such as Arthur's proof of the ergodic theorems of demography and feminist theories.

Table 2.10.5 Chronology of the earlier sources and contemporary one-sex population theory

Dates	Main Authors	Emphasis on
Legends religious & common-sense views	Kenya: the Masai Australian Aboriginal Mozambique - Tsonga myth	- Before the physiological causes of conception were known, men thought that maternity resulted from the direct insertion of the child into woman's womb ... children were not conceived by their fathers ... Man has no part in creation (Badinter, 1989: 24). - In the beginning, men and women formed two separate tribes who lived apart. Each tribe was independent of the other and they met only fortuitously in the forests for lovemaking. The children born of these unions stayed with their mothers (Badinter, 1989: 12-13) - Most often men and women live apart from each other, either as single individuals, or as all-male and all-female groups (White, 1975) - The first man is named 'the one who brings a glowing cinder', and the first woman, 'the one who grinds vegetables' (Junod, 1975:32).
Bronze Age	Middle East and West	- Goddess either becomes a subordinate wife, and then disappears from the divine scene altogether, or becomes masculinized and driven out by the male god (Badinter, 1989: 61-2). The minimal patriarchal system can be recognized when fathers exchange their daughters for daughters-in law (or brothers exchange their sisters for wives), with or without the consent of the parties concerned; they were bought and sold, and were the property of their husbands. The main characteristic of the patriarchal society in its most absolute form is the strict control of female sexuality (Badinter, 1989:.59).
6000 3000	Sumerians Egypt Babylon, Mesopotamia to Anatolia and Syria Indo-European	- Men gradually began to demand the right to participate in the various tasks and functions that had previously been the prerogative of women. This collaboration led to the future dispossession of women. - The birth of the new notion of the couple. Gradually, it came to be realized that it takes two to procreate, two to produce (Badinter, 1989:41-2). - The divine couple made its appearance. Osiris the spirit of grain and water married to Isis, the great goddess of universal fertility. This marriage symbolizes the union of the water (the Nile) and the earth. - The agrarian gods took on a male aspect - Tendency towards the formation of couples of the Jupiter-Juno type, namely matrimonial unions resembling monogamy.
	Ancient Greece Plato Aristotle Hippocrates	- First elaborated theories of polarization of female and male. Greek philosophical justification of man's superiority in procreation was translated into 'form' and 'matter'. The male transmits the form and his semen is the source of life. The female only transmits the matter because she waits passively to be engendered and merely nourishes the foetus. Females are fallen or failed males (Badinter, 1989: 70-1; Thomlinson, 1976: 167-168; Cadden 1993: 14, 23-4).
17th to 5th century BC. 1st century AD	Nomadic Jews, Athenians New Zealand Maori Bible - *Genesis* New Testament	- The myth of the creation of the world is found in these patriarchal societies (Badinter, 1989: 67). In less than 1000 years, Brahma, Jehovah, Zeus and Jupiter imposed themselves on believers as the fathers of humanity, and reduced mothers to the status of minors; as if men had invented God, to make it easier to impose paternal power (Badinter, 1989: 60). - The Testaments' history depicts the rise of the androcentric approach, specially in the replacement of a goddess with a god. It introduces us into the religious world of the cult of 'god the father' (Badinter, 1989: 64-5). - The cult of God-the-Father substituted for that of the Mother-Goddess.

Middle Ages 11th to 14th	Constantine the African, Hildegard of Bingen, and William of Conches	- Explored the roles of males and females in generation and the place of sexual pleasure, though did not give rise to the dominance of a single theoretical model. 'Classical medicine had entertained two models of sex determination: one depending on the uterine environment and the other on the seed (Cadden, 1993: 54-55; 62-63). Differences between males and females in general and between men and women in particular were, according to medieval opinion, natural: they were understood to constitute defining principles inherited essentially in individuals, and they formed part of the larger order, the plan or logic of the world' (Cadden, 1993: 188).
17th century	John Usher, or Ussher, archbishop of Armagh, and John Lightfoot Laqueurs	- In the year 4004 BC., at precisely nine o'clock on the morning of October 23, "God created man in his own image, in the image of God created He him; male and female created He them.' The year, the day, and the Creation, unspecified in the Bible itself, were calculated by two seventeenth-century scholars' (Tannahill, 1980: 2). - Before the 18th century male and female were in various ways regarded as manifestations of a unified substratum (Cadden, 1993:3).
Vivum omne ex ovo	Descartes William Harvey C. Linnaeus A. van Leeuwenhoek	- Descartes accepted the theory that both sexes emitted semen in coitus and compared the chemistry of reproduction to that of brewing: 'The semina of the two sexes mingle and act as yeast, each on the other'. The biseminal theory was attacked in 1651 by William Harvey, who founded *ovism*, the doctrine that the female element is decisive in procreation. Carolus Linnaeus summed up Harvey's thesis in an epigram: '*Vivum omne ex ovo*' (everything living comes from the egg). The Dutch lens-grinder Anton van Leeuwenhoek put semen under his invention, the microscope, and became the first man to see the small swimming creatures which he called spermatozoa - a blow from which the ovists never recovered. Yet it was over two centuries before biologists were able to produce microscopic evidence of the fertilization process in human beings (Thomlinson, 1976:167-8).
1900 to 1949	Lotka, Sharpe and Dublin Wicksell, 1931 W. Feller, 1941	- First outline of the explicit mathematical one-sex model, applied originally to the male population alone. Its underlying assumption is the complete independence of sexes in population growth. - 'Nuptiality, fertility, and reproductivity'. - 'On the integral equation of renewal theory'.
1950 to 1969	Henry, 1953 Davis & Blake, 1956 Coale, 1957b Coale-Demeny,1966 Lopez, 1961 Becker, 1960 Easterlin, 1969 Bongaarts, 1978	- 'Theoretical basis of measures of natural fertility' (in Smith & Keyfitz, 1977: 373-382) - 'Social structure and fertility: an analytic framework' - the model of the intermediate variables of fertility. - A new method for calculating Lotka's r - the intrinsic rate of growth in a stable population. - 'Regional model life tables and stable population' - 'Weak ergodicity'. - 'An economic analysis of fecundity'. - 'Towards a socio-economic theory of fertility ...' - 'A framework for analyzing the proximate determinants of fertility'
1970	B. Parlett, 1970 Caldwell,976a, Caldwell, 1980 Easterlin, 1975, 1978 Handwerker, 1989 Coale, 1972 Coale-Trussell, 1974 Arthur, 1981, 1982a WFS and DHS Mason, 1986, 1995 Federici et al, 1993 Greenhalgh, 1994; Greenhalgh & Li, 1995	- 'Ergodic properties of populations I: the one sex model.' - 'Towards a restatement of demographic transition theory'; 'The wealth flows theory of fertility decline'. - 'An economic framework of fertility analysis'; 'The economics and sociology of fertility'. - 'Women's power and social revolution: an anthropological critique of demographic transition - 'The growth and structure of human populations'. - 'Model fertility schedules: variations in the age structure of childbearing in human populations. - 'Why a population converges to stability' - a proof of the ergodic theorems of demography based on one-sex model'. - World Fertility Surveys; Demographic Helath Surveys. - 'The status of women: conceptual and methodological issues in demographic studies'. - 'Gender and demographic change: what do we know?'. - 'Women's position and demographic change: selected papers'. - 'Anthropological contributions to fertility theory'; 'Engendering reproductive policy and practice in peasant China: for a feminist demography of reproduction'.

Source: Arthur, 1981, 1982a; Badinter, 1989; Cadden, 1993; Davis and Blake, 1956; Junot, 1975; Smith and Keyfitz, 1977; Tannahill, 1980; Thomlinson, 1976; White, 1975.

11.

Two-sex demography and demographic outcomes

> The end rules the method (Bacon, 1875: 254).

> The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is - insofar as it is thinkable at all - primitive and muddled. (Einstein, 1949).

Two-sex demography: moving from separation to complementarity

The two-sex demography relies on the third of the three-dimensional set of principles that has already been discussed extensively in this thesis: the complementarity between the sexes. Just as with the separation of the sexes in one-sex demography, the principle of complementarity recognizes that in reproduction there are two human natures, rather than only one as is the case in the neuter approach: male and female. The difference, though, is that from the point of view of the complementarity principle the two demographic natures are meant to interact with one another, rather than stand on their own. It is in this context that the distinction between the concept of 'demographic output' and 'demographic outcome' becomes indispensable for an adequate consideration of a two-sex approach in demographic theory in general.

In previous chapters, particularly in Chapter 8, I have maintained that the representation of the subject matter of demography arranged within a sphere with a descriptive-hard mathematical core and a softer surrounding explanatory theory cannot adequately accommodate a two-sex approach. Nor is it the contention of this thesis that a clear-cut dividing line should be drawn between a mathematical core, assumed to describe what has happen to population, and a non-mathematical softer surrounding, expected to explain the causation and the mechanisms of demographic change.

In Chapter 10 I maintained that one-sex demography offers the best body of demographic analysis for the scientific study of the structure of population. The feasibility, usefulness and validity of the one-sex approaches in demography can be well understood when they are assessed within the scope of scientific study of the structure of population. The one-sex demography has achieved a remarkable match between its conceptual theory and its increasingly elegant and sophisticated methods and models.

Most of the questions concerning the measurement and description of population in terms of its size, growth, development of structure, and fertility levels and trends can be settled with sufficient precision by accepting the principle of separation between the sexes. Moreover, one should not trivialize this principle by simply assuming that the sex chosen to calculate the demographic measures is irrelevant; or that the one-sex approach is usually applied to the female component of population for reasons of convenience; or even that since every birth must have two parents, both sexes contribute equally to reproductivity and therefore they should always be taken into account (Karmel, 1948c: ii, 52). After all, demographic analysis will benefit more in recognizing that the female-population is the 'producing sex' (Knibbs, 1917: 235) within the demographic system, than insisting that because both sexes are necessary they should be taken into account at all times and for all purposes.

Following the proposed reconceptualization of ‘population composition’ from a two-sex perspective the demographic structure of population by sex and age became just one of two important building-blocks within the demographic system. The second building-block, called demographic organization, corresponds to the functional relationships surrounding the structure of population and has already been defined according to the categories gender and generation (see Chapter 8).

Just as in the case of the study of population structure, the scientific study of the organization of population needs proper and systematic analytical tools. The problem, though, is that the concepts and measures which have produced reasonable results on the basis of neuter and one-sex methods are not necessarily applicable to phenomena in which the organization of population cannot be abstracted.

So far the demographic concepts which embody specific aspects, features and relations have not been classified in the way proposed here. Neuter concepts, such as crude rates, have been mixed with one-sex concepts, such as total fertility and net reproduction rate. Moreover, one can also find a third array of concepts that embody features that involve the interaction and complementarity between the sexes; this includes the concepts discussed in Part I, and several concerning the organization aspects of population, including nuptiality, education, labour force participation, migration, value of children, demand for children, ideal family size, desired number of children, magnitude of marriage attraction, marriage squeeze, marriage market, and intergenerational wealth flows.

Concepts of this third type are by their nature potentially two-sex concepts; demographers have been developing them somewhat haphazardly, aiming at explaining why and how the magnitude and direction of demographic events have or have not changed in a given population. Regardless of the differences and degree of specification found in these concepts, to date they have generally been applied to methodologies that are adequate to study demographic phenomena as outputs.

It is at this stage that the one-sex methods have broken down; not because they have been built upon inconsistent assumptions and inadequate methodologies. Instead, the one-sex demographic methods and theories have been effective and sufficiently precise when investigators need to strip off or control for aspects that make it difficult to capture the essential characteristics and relations in a population. The strength of one-sex demography in dealing with the sexes separately may become its own weakness if one tries to use it to handle the complementarity between the sexes. Yet, when the investigator wants to dig deeper into the explanation for the occurrence of a certain event in a population, the study needs to shift from separation to complementarity. The mechanisms and causes of change are part of a notion of population composition in which structure and functional organization are parts of the same whole.

Thus an explanatory demography becomes a complex process somewhat different from a descriptive demography. Explanation entails two main aspects. On the one hand, to understand how and why something happens as opposed to describing what has happened; this depends on the nature of phenomena under analysis, particularly whether they involve historical events, manipulable events or scientific processes (Cook et al., 1994: 17-18). On the other hand, from a methodological point of view explanation entails the need to accept the idea that both sexes should matter; in this case a clearly designated and reliable phenomenon depends on modelling interactions, combinations, interdependence, causal mechanisms, and generative processes between both sexes.

In previous chapters I have referred to the conditions which are necessary and sufficient for a two-sex demography as of two types: conceptual and methodological. While the latter refer to the development of two-sex methods and models adequate to tackle research problems of a two-sex nature, the former are concerned with the development of adequate working concepts and measures to operationalize the explanation of demographic outcomes. This final chapter of the thesis provides a detailed discussion on these two types of conditions.

The principal objective of this chapter is to discuss the important aspects needed to transform the conceptual theory outlined in the previous chapters into a comprehensive two-sex research design. For that the concept of demographic outcome, the core working concept derived from the principle of complementarity between the sexes, is compared with the concept of demographic outputs. Secondly, the preliminary definition of two-sex demography given in the Introduction is elaborated. Thirdly, in closing this thesis a set of brief sections discuss important aspects of the two main components of the definition of two-sex demography. Two particular areas in the twentieth-century literature directly relevant to the development of a two-sex demography are discussed: first, the literature that for about half a century has explicitly aimed at developing two-sex models in association with the so-called 'two-sex problem'; secondly, the literature which during the same period has identified and attempted to explain the causes and mechanisms of demographic change; this literature corresponds mostly to what has been become known as the 'determinants of fertility'. As a background for this final discussion Appendix B provides a selective bibliography prepared in chronological order.

Demographic outputs versus demographic outcomes

The dualism between macro and micro approaches stands as a persistent challenge to contemporary demographic thought. In recent times, this issue has provoked too much anxiety, if not an increasing sensitivity, among demographers. Time and again yet another analytical framework stresses the need to interact the macro-aggregate with the micro-individual levels of analysis. Several authors already referred in previous chapters have discussed the macro-micro dualism (i.e., Davis and Blake, 1956; Caldwell, 1985b; Cleland, Hobcraft and Dinesen,, 1985; Coleman and Schofield, 1986; de Bruijn, 1993; Burch, 1994, 1995; Greenhalgh, 1994). To mention a new reference, Smith in 1989 discussed the macro-micro dualism as part of his proposal to integrate theory and research on the institutional determinants of fertility.

> This article has attempted to (a) draw out the *macro* nature of salient conceptual discussions of the institutional determinants of fertility, (b) argue the need to judge theories of fertility with respect to their ability to explain fertility variation as observed at the *individual* level, and (c) outline a general approach by which relevant macro causes and micro outcomes might be integrated and investigated empirically (Smith, 1989: 182).

However, some critical remarks made earlier in the 1980s seem to remain valid today. For instance, McNicoll wrote in 1980 about the wide agreement in the field of demography concerning the lack of an adequate theory of fertility:

> Everyone knows why fertility falls in the course of economic development ... Yet despite this knowledge, it is widely agreed that we do not have an adequate *theory* of fertility, if by theory we mean a coherent body of analyses linking a characterization of society and economy, aggregation or local, to *individual* fertility decisions and outcomes, able to withstand scrutiny against the empirical record (the latter test presumably assuring some predictive value) (McNicoll, 1980: 441; also cited in Greenhalgh, 1994: 16; Smith, 1989: 171).

Still in 1980 Mackensen wrote about the little that has been done to integrate the levels of research and systematic explanation:

> we too often disregard of [sic.] the conceptual conditions of different levels of aggregation. It is a well established fact in methodology that we have to carefully keep to the aggregate level of argumentation, and each of us knows well enough about that. In population research, nevertheless, we often find reasoning that seem[s] to imply that the problems of understanding reproductive behaviour can be solved on one level alone. We have done too little, I mean, to interconnect the levels of research and explanation systematically, particularly on the explanation side (Mackensen, 1980: 15-16).

Throughout this thesis I have avoided the conventional dualism between micro and macro, or even better tried to tackle it, in a different way. To illustrate my departure from orthodoxy I reproduce in Table 2.11.1 the framework sketched by Mackensen (1980) in the paper just mentioned. Mackensen considered three, rather than the conventional two levels (see Smith, 1989) of national aggregation: MACRO level, for the total population; MESO level, for the patterns of age groups, cohorts, social strata, and local and regional subdivisions; MICRO level, for the individual families and individual persons.

Table 2.11.1 A scheme of frame for explanations of fertility

Level of Argument And of empirical Aggregation	**EXPLANANDUM** **resulting phenomena** **POPULATION** **DEMOGRAPHIC EFFECT**		**REGENERATIVE STRUCTURE**		**EXPLANANS** **determining factors** **SOCIETY** **CULTURE**		**SOCIAL STRUCTURE**
MACRO level total population society	General frequency age rate of fertility TFR NRR	←	General frequency age rate of marriage intercourse divorce death		General system change of values norms laws **ideal family size**	←	General structure change of society economy policy
	↑		↓		↓↑		↓↑
MESO level homogeneous partial populations with similar conditions and behaviour	specific frequency age rate of fertility	←	specific frequency age rate of marriage intercourse divorce death		specific values norms aspirations expectations social prestige identification	←	specific social economic political relations categories reference groups kinship system
	↑		↓		↓↑		↓↑
MICRO level social action and interaction	individual family size family cycle birth order **real family size**	←	individual regenerative behaviour birth control **expected family size**	←	individual preferences life style evaluations decisions **wanted family size**	←	individual options living conditions possibilities alternatives monetary costs and benefits

Source: Mackensen, 1980: 17

Mackensen outlined her framework hoping that most of the concurrent theories of fertility, if not all, could be located somewhere in it. She described the characteristics of the two building-blocks in the framework, the explaining or determining factors and the explained or resulting phenomena. In particular, in explaining why 'there is just one arrow from explaining to explained phenomena, that leads to behaviour' (Mackensen, 1980: 18), she pointed out that already Davis and Blake (1956) had stressed this in their model listing the factors affecting fertility.

Mackensen discussed the various theoretical approaches to fertility in terms of their location in the different levels of aggregation. But this reasoning seems to have become somewhat tautological, if not even obsolete. As Mackensen (1983: 16) admitted, 'it is pretty obvious that we could - or even must - work with more than three levels'. The departure of this thesis from this orthodoxy is to argue that regardless of the number of levels of aggregation, the dualism between macro and micro approaches stands as a persistent dilemma for demographers (see McNicoll, 1980, 1992, 1993b; Caldwell, 1985b; Smith, 1989; Burch, 1994, 1995). On the one hand, ‘A focus on macro determinants of fertility does not imply that fertility outcomes are determined at the institutional level’ (Smith, 1989: 171). But on the other hand, it seems that a shift of emphasis to a micro-demographic approach does not mean that demographers should renounce 'the aggregate level of argumentation' (Mackensen, 1980: 16). Mackensen touched on this dilemma, saying:

> We more and more abandon theories on a very high aggregate level, and increasingly work with concepts of individual behaviour. With this tendency, we ... [lose] a lot of potentials we previously had in demography: prognostical power, coincidence of social structure with individual behaviour, uniformity of theory and empirical research. We have become more refined - and more ... [disatisfied] with what we do [sic.] (Mackensen, 1980: 20).

Something seems to be still missing in this debate, and this seems to be the working concepts without which one can hardly avoid proposing *ad hoc* interconnections between levels of research. The concepts *demographic output* and *demographic outcome* emerged in this context as providing the analytical linkages among the horizontal and vertical flows in a scheme, such as the one in Table 2.11.1. They are the corollary of the distinction proposed in this thesis between different epistemological purposes in demographic analysis (i.e., descriptive, explanatory, predictive); different theoretical principles (i.e., differentiation-separation-complementarity); and different methodological frameworks (i.e., neuter-one-and-two-sex) .

The choice of the terms 'output' and 'outcome' is far from arbitrary; since they are widely used in commonsense language it seems important to explain why and how they can be expected to serve as explanatory resources in demographic theory construction.

In ordinary language and in general dictionaries the terms 'output' and 'outcome' share at least one common meaning, that is 'result'. Likewise they are also frequently used in scientific language, including in demographic literature. Box 2.11.1 illustrates this with twenty small extracts from texts dealing exclusively with demographic issues.

Although the concepts 'output' and 'outcome', are frequently used interchangeably as synonyms of 'result' in the broad sense, they tend to be applied in relatively different situations. People are more prone to use the term 'output' in reference to tangible results, or perhaps the quantity or amount of things actually produced. This is particularly so with material production, but also with the computing information derived from certain input data; by extension, one can also speak of output in demographic reproduction as to the number of births actually produced by the 'producing sex', to use Knibbs's expression.

Box 2.11.1 The use of the terms 'output' and 'outcome' in scientific literature relevant to demography

Only italics have been added to all sentences.

1. POTENTIAL OUTPUT OF CHILDREN, C_n: On the production side of fertility determination, the key analytical concept is the *potential output* of children - the number of surviving children a household would have if fertility were not deliberately limited (Easterlin, 1975: 55).

2. The theory presented in this article can be characterized best as interactionism and is a variant of social psychology. Specifically, the theory claims that fertility is an *outcome* of two broad social processes within the family unit ... socially constructed attitudes of household members ... social exchanges transpiring between husband and wife ... (p. 301-302) ... Family decision making in general and the demand for children in particular are regarded as *outcomes* of the unique combination of individual volition and social interaction found within the family unit. It is claimed that the husband and wife have needs, expectations, and preferences as to family size and related child rearing practices (which are reflected in their attitudes) and that these variables influence and are influenced by the particular social exchanges occurring between the spouses. Fertility is posited to be a direct function of these attitudes and exchanges, subject to the economic and social constraints discussed earlier (p. 306) (Bagozzi and Van Loo, 1978).

3. Utilizing the approach originated by Blood and Wolfe (1960), power in fertility surveys is typically measured through questions on *the outcome of decision-making* (who makes the final decision) on various issues relating to family life, e.g., finances, leisure time activities, and child-rearing (p.1) ... In discussing *power outcomes* in decision-making, we can distinguish between passive and active decision-making (p. 6) ... *Perceptions of fertility decision-making outcomes* (p. 13) ... *The interrelationship between bases of power and fertility decision-making outcomes* (p. 15) ... *Alternative outcomes: unilateral/surreptitious and open fertility regulation* (p. 19) (Hollerbach, 1980).

4. *Fertility decision outcomes*, of course, are complicated not only by the idiosyncratic factor noted for exclusion above, but more significantly by the typical jointness of the decision making and hence by *dependence of the outcome upon* the relative power of those involved. For analytical purposes, there are advantages in conceptually restricting this jointness to the case of a married couple (p. 451) ... Whether marriage decisions are made by the members of a couple themselves or by their parents is clearly a potentially important distinction for *fertility outcomes* (pp. 451-452) ... These insights can only benefit the understanding of *fertility settings and outcomes* (p. 459) (McNicoll, 1980).

5. In an effort to anchor the analysis to a solid base, Becker makes use of a simple device first proposed by Cairncross. The family is seen as a production unit. It transforms the 'inputs' of its time, efforts, luck, and other endowments into the 'outputs of self-esteem, health, leisure enjoyment, children, and other valid things (p. 394) ... The general reader, and economists too, will be annoyed at the unfortunate choice of terminology: 'low-quality men' (p. 66) ... 'inferior' men (less preferred in the marriage market) (p. 72) ... the 'quality of children' (their level of education) (p. 93) ... '*marital output*' (the joys of family life (p. 397) (Arthur, 1982b).

6. Sometimes events occur during the life of a project that tend either to increase or to decrease *the expected outcomes* of the project. (Fisher et al., 1983: 19).

7. How, within a given demographic model, would arbitrary changes in its age-and time-specific schedules alter *particular output* variables that interest us? (Arthur, 1984: 109).

8. As the *outcome* beyond the second birth seems so heavily conditioned by the time of the second birth, the age at achievement becomes a very major determinant of the likely *average fertility outcome* (Hobcraft, 1985:82).

9. To quote Smith once again: 'There is no place within this system for a fertility analysis that supposes households or individuals within households had a determinant and determinable set of preferences for children, which they rationally assessed so as *to produce optimal outcomes* (Cleland and Wilson, 1987: 15).
10. The total differential is a generally applicable mathematical technique for decomposing changes; for example, it has been used in economics to relate *changes in total output* to changes in each of the factors of production (Schoen, 1988: 122).

11. A focus on macro determinants of fertility does not imply that *fertility outcomes* are determined at the institutional level (p. 171) ... (c) outline a general approach by which relevant macro causes and *micro outcomes* might be integrated and investigated empirically (p. 182) (Smith, 1989).

12. This is essentially the conclusion of the World Bank report in 1986. Any worldwide comparison made today would indeed produce this *statistical outcome* (Lesthaeghe, 1989: 1).

13. All of the prospective studies reported significant effects of husband's desires on *couple fertility behavior or outcomes* ... A 'marginal' interpretation of husband's effects does not adequately represent theoretical relationships between two individual desires and a *single joint fertility outcome* (p. 579) ... A few analyses of data collected during the 1970s demonstrated that disagreeing couples were not 'average' in their *contraceptive behavior or fertility outcome* (p. 580) ... Since desired fertility is endogenous to elements of social and family structure that *affect fertility outcomes*, we first estimate the total effects of desires on the birth rate (p. 582) ...The findings do support previous investigators' assumption that wives' and husbands' desires are additive in their effects on fertility. Conflicting number desires resulted in 'average' birth rates. This means we could account for the same *fertility outcomes* with a simpler set of indicators, representing wife's desire and husband's desire (p. 586) (Thomson, McDonald, and Bumpass, 1990).
14. *Demography* has published its share of analysis based on the WFS and other surveys. Few articles have appeared in our journal in recent years that have not used *computer output* somewhere, and many could not have been written at all without the computer (Keyfitz, 1993b: 534).

15. Thus, the typical hypothesis that relates women's status to fertility or mortality posits the existence of an interaction between women's status and socio-economic conditions *in affecting demographic outcomes.* (p. 6) ... Part II: Women's position as an *outcome of demographic change* (p. 242) (Federici, Mason and Scogner, 1993).
16. The perspective on fertility I have set out in this essay can be illustrated by a discussion of recent world experience of fertility trends. I am not concerned with comprehensive description, but with a brief, selective presentation of *differences in structure and outcome* (McNicoll, 1993b: 15).
17. In Easterlin's model social and economic modernisation and other basic determinants are seen as affecting *reproductive outcomes* by operating through the following three mediating variables (p. 438) ... *Easterlin's framework has one outcome* (the number of living children) and three determinants (demand, supply, and regulation cost), but there is no convenient equation that relates dependent to independent factors (p. 441) ... (Bongaarts, 1993b: 438).

18. Demographic variants such as Penn Handwerker's (1986b, 1986c, 1989) seek parsimonious explanations of *demographic outcomes*, in particular the level of fertility (p. 4) ... Although we are nowhere near a comprehensive understanding of how gender shapes reproduction in different times and spaces, recent anthropological and historical research suggests some elements of a truly gendered understanding of *fertility dynamics and outcomes* (p. 30) (Greenhalgh, 1994).
19. When the number of variables in the system becomes large, when the relationship[s] are non-linear, when there are interactions and feedbacks, and when the system is dynamic - it becomes awkward if not impossible to express the theory in everyday language, and virtually impossible to 'eyeball' the implications of the system, or *to infer outcomes* from specified inputs (Burch, 1995: 24-25).
20. What kind of study design might allow one to demonstrate this? First, to permit analysis of demographic change, both the causal agents and the *demographic outcome* would have to be measured over successive cohorts (Mason, 1995: 3).

Although the examples included in Box 2.11.1 have been selected without pretende that they are comprehensive or statistically representative, it seems possible to conjecture that the term 'output' is less used than the term 'outcome' in demographic literature. Even the five examples in Box 2.11.1 (paragraphs 1, 5, 7, 10 and 14) were rather difficult to find. In part, this may be because demographers seem to find the metaphorical association between childbearing and material production morally counterintuitive. Another reason may be the lack of adequate discussion on this, and in particular on why the analogy between the two types of production is meaningful, if not morally at least theoretically.

In turn, the term 'outcome' is customarily reserved for situations in which something tangible or not has consequences and influences both the amount of outputs produced and other activities in people's life. This seems to be the reason the term 'outcome' is perhaps used more often than the term 'output'; not surprisingly, as the examples in Box 2.11.1, show the term outcome is likely to be found in studies of 'institutional determinants of fertility'.

Of course, on the basis of commonsense one cannot draw a clear-cut distinction between output, outcome, and results. But the attempt to impute a certain theoretical role to specific concepts is part of the process of theory construction. In particular, the very fact that only women can give birth or 'produce' children should have a theoretical appeal for demographers. While every birth needs two parents, the demographic evens designated 'births' are the derivative of and depend directly on the female-only population.

To some extend this dismisses the theoretical meaning of the assertion that the one-sex method can equally be applied to male-only population and without much scientific purpose. Mainstream demography drawing upon the one-sex approach usually treats the idea that fertility and reproductivity measures can be computed on the basis of either sex as trivial generalization. In turn, the debates around the 'two-sex problem' have been unconvincing in their challenge of the mainstream view. In part this is because the importance of the two sexes in the 'two-sex problem' is taken for granted; another reason is that the applicability of the one-sex models independently to both sexes has become a technical rather than substantive question.

Much of the research on the 'two-sex problem' seems to have derived from a basic but important misunderstanding between the two main levels of demographic analysis considered in this thesis. On the one hand, not enough attention has been dedicated to the conceptual and methodological implications of the distinction between description and explanation. On the other hand, there is the confusion between the *necessity* of both sexes and the *nature* of the contribution of each sex to demographic reproduction.

The recognition of the female-population as the 'producing sex' takes into consideration the differences in the nature of the reproductive role of both sexes, rather than that only females are necessary to demographic reproduction. In particular, demographers recognize that because only females of a certain age are at risk of giving birth to children they should measure and describe the levels and trends of fertility outputs as a function of female-population. Yet, although men and some women do not give birth to children, the outcome of their activities (i.e., practices, attitudes and knowledge) can have consequences and influence demographic events in a variety of ways. This means not all reproductive activities produce outputs, and thus not all demographic outcomes are demographic outputs. Likewise, while only women of reproductive age have the potential to produce demographic output, all demographic activities from both sexes are likely to have demographic outcomes.

The recognition of this distinction should have important implications for research design. In particular, when demographers aim to measure and explain the causes and mechanisms of demographic change no sex can *a priori* be considered eligible to represent, theoretical and statistically, the whole population. This view can hardly be found in most fertility surveys and debates on the determinants of fertility.

Most fertility surveys do not distinguish fertility output from fertility outcome, and for this reason their research designs do not take into consideration the important differences as to their methodological and analytical requirements. One can accept that it makes sense to focus on the female component of the population to study fertility, but in this case fertility is treated as an output (i.e., population size, level and trends of fertility). But if a survey is aimed at explaining the causes and mechanisms of fertility change and family life cycles associated with parenthood, then one should wonder, 'do reproductive intentions matter?', 'Do values matter?' These questions were borrowed from two different papers, the former by Bongaarts (1991) and the latter by Lesthaeghe and Moors (1994). Both papers conclude that reproductive intensions and value orientations do matter, but particularly the general title of Bongaarts's paper is not consistent with its content.

Neither of these two papers recognizes the need to distinguish between demographic outputs and demographic outcomes in the way proposed in this thesis. However, because Lesthaeghe and Moors (1994) focused on surveys which have data on both sexes they were able to conclude something as to the 'gender-specific validity' of the theories they assessed:

> The final conclusion is that the results of the European Values Survey of 1990 of the four countries are largely in agreement with what is found in the American cross-sections and panels: value orientations do matter. Furthermore, the two economic theories considered here seem to have a gender-specific validity only: it is indeed easier to trace the Easterlin effects in the results for males and the Becker effects in the results for females (Lesthaeghe and Moors, 1994: 15).

In turn, Bongaarts's paper concludes that reproductive intentions matter, but strictly speaking the paper tries to demonstrate that only female reproductive intentions matter. Starting from its conceptual framework and moving to the discussion on the research design, the consistency of reproductive measures, the data and the conclusion, the author expects that the reader should accept that if there is any limitation in the way the reproductive intentions are studied is because of the data. But since no comment is provided in this regard, one is led to conclude that fertility preferences of married couples in the 18 DHS countries taken into consideration are exclusively determined by women's ability to control reproduction.

This is a remarkable assumption, both from the point of view of the currently fashionable views put forward by feminist-demographers but also from the thrust of the relatively old debate within demography itself, the 'two-sex problem' in the perspective first discussed by by Vincent (1946) and Karmel (1948c: 59-75; 1948d: 361-364) some fifty years ago. Although these two authors were mainly concerned with the effect of the relative number of available partners, their concern seems even more pertinent when the question in debate is, 'do reproductive intentions matter?'. I return to Vincent's and Karmel's views below, but one can already anticipate their argument to say that Bongaarts's paper takes for granted one of the two alternative assumptions they discussed: either the populations in the 18 DHS countries studies by Bongaarts are assumed to be completely sexually promiscuous, or option to focus on females only implies that they are perfectly marriage dominant. Karmel questioned the validity of conventional one-sex net reproduction rates maintaining that the only reasonable way to accept them would be to have a justified reason to believe that females are perfectly or nearly marriage dominant.

Ntozi (1993), in a book called *The Role of Men in Determining Fertility*, follow the procedures used when fertility is studied in terms of women only, but implicitly assuming that males rather than females are perfectly marriage dominant. Ntozi commented on the limitations of his data as follows:

> The original objectives of the study were to study the determinants of fertility in Ankole - mostly by examining the responses from women. The men's questionnaire was included to collect information that would merely complement the data about women. However, after the analysis of the women-related data and the initial tabulation of the men-related one, it was found that the data about men could be analyzed separately. The study would not only serve to supplement and complement the women's responses; it would also provide an independent report of the men's role in fertility in the area. While most of the data, especially on factors of fertility, would satisfy the last approach, not enough information was collected to enable to measure and analyze some aspects of levels, patterns, trends, and differentials in men's fertility in the area (Ntozi, 1993: 11).

In this case the one-sex approach has been applied to males as one would do with female-only population, and thus the issue of fertility outcome in the sense proposed here is not even contemplated. But still more striking is that Ntozi, like Bongaarts (1991) and Lesthaeghe and Moors (1994), found no need to even contemplate the central issues in the 'two-sex problem', namely the allegedly inconsistent results produced by the one-sex approach when applied to men independently from women (Pollard and Höhn, 1994: 204; Pollard, 1995).

In short, a demographic outcome can be defined as the cluster of practices, attitudes and knowledge of both sexes on which the explanation of the causes and mechanisms of demographic change depend. This concept of demographic outcome has been derived from the principle of complementarity between the sexes in the process of the search for the reason of using a two-sex approach. Together with the concept of demographic output, the notion of demographic outcome can be used both to conceptualize specific demographic issues in theoretical terms only, and to establish the stages in specific operational research designs. In this regard, these two concepts should become more that 'operational definitions for key variables and terms', as defined by Fisher et al. (1983: 15-18). They should be seen as key operational definitions and guide investigators in the overall process of operations research design, including the identification and definition of two-sex research problems, the justification of the study, objectives, hypotheses, operational definition of specific variables, sampling, data collection, and the analysis of data.

A more detailed definition of the two-sex demography

Following the sketch of the abstract demographic system outlined in Chapter 8, the two-sex approach derived from the principle of complementarity between the sexes can be expressed formally as follows:

$$\bigcap S_d = \{ \vec{f}_i \cap \vec{m}_j ; I\vec{G}\vec{G}_l \}$$

where $\bigcap$ denotes the *both-and* nature of the demographic system S_d or the 'intersection' relationships between the females (*f*) and (*m*) of i-th and j-th categories (i.e.) respectively. The dimension $I\vec{G}\vec{G}_l$ denotes the intergender-generational relationships as part of the content of population organization.

In the Introduction of this thesis the two-sex demography was briefly defined as the analytical body of demography aiming to explain, rather than describe only, the changes in the magnitude, direction and pace of size, territorial distribution, and composition of population, as well as the components of such changes from the point of view of the complementarity between the sexes. Following the outline of the two-sex conceptual theory provided in the previous chapters the preliminary schematic definition given in the Introduction, that is Two-sex demography = {{conceptual setting} + {2-sex methodology}}, can now be elaborated further:

Two-sex demography = {{IGG approach + demographic outcome} + 2-sex methodology}

The conceptual setting is now detailed in terms of the intergender-generational conceptual theory and the two-sex methodological framework aiming to explain essential demographic events and relations. The conceptual theory is the theory of why and when (causation) both sexes should matter to demography. The methodological framework is the theory of how (mechanisms) research on two-sex problems does or should proceed; it includes the rules and procedures on how both sexes might be combined or interacted in specific models and methods applied to particular research issues.

In short, the two-sex demography is a coherent seam of a conceptual theory and methodological framework that studies the set of contingencies and mechanisms on which the explanation of the magnitude, direction and pace of demographic outputs depend. In this context, the concept of demographic outcome constitutes the key to interconnect the levels of research and explanation systematically in two ways: the interconnection between the descriptive and explanatory areas of demographic analysis, and the main levels of reproductive behaviour.

The remaining sections of the thesis discuss the literature directly relevant to the two-sex demography, in terms of its conceptual and methodological needs. In this regard two research areas which have for too long developed separately from one another need to be brought to the discussion: the literature on the 'two-sex problem', on the one hand, and the literature on the 'determinants of fertility', on the other. This is the reason the bibliography contained in Appendix B does not separate the two types of literature.

Contemporary conventional demographic literature offers many reviews of the literature on the two-sex problem and the determinants of fertility. However, when one reads the former one gets the impression that the two-sex problem is a technicality without relevance for the study of fertility determinants; in turn, when one reviews the existing theories on determinants of fertility it seems that the fertility and reproduction are determined by everything but the interaction and other relations of complementarity between the sexes. In the remaining sections of the thesis I am concerned not in providing a comprehensive alternative review of an integrated literature comprising both areas, but in demonstrating that their separation is self-defeating and untenable. This view is consistent with the definition of two-sex demography seen as a comprehensive seam between a gender-generation conceptual theory and two-sex methodological frameworks.

The 'two-sex problem' and the 'determinants of fertility': one single origin

When one traces the literature on the 'two-sex problem' and the 'determinants of fertility' it becomes clear that their split occurred early in the 1950s. The conjecture that this slip was a sign of progress and further specialization is only partially true. The motivation of the studies known as 'two-sex problem' and 'determinants of fertility' is the same, and this is clear when one browses through the titles of the articles included in Appendix B.

For instance, the purpose and motivation of Pollard's (1948) paper entitled 'The measurement of reproductivity' are not different from those by Bhusham and Hill (1995) called 'The measurement and interpretation of desired fertility': to measure accurately fertility, reproductivity and population growth. But while the former proposes a two-sex model aiming to improve the measurement of a phenomenon which has been settled with sufficient precision by one-sex methods, the latter discusses the measurement of fertility preferences without even wondering whether this is a one or two-sex research problem.

I start the chronological bibliography with Knibbs's *Mathematical Theory of Population* in line with my discussion in Chapter 7. Although Knibbs did not suggest any direction concerning the bifurcation of the concept of fertility outputs and fertility, his *Mathematical Theory of Population* provides an adequate conceptualization of demographic reproduction. Rather than considering nuptiality of no 'particular interest to demographers', as Newell (1988) suggested, Knibbs moved from natality to fertility not directly but through nuptiality. This seems to be the most correct approach of demographic reproduction, not to dismiss the usefulness and validity of neuter and one-sex perspectives but to put them in their wider context.

Figure 2.11.1 is inspired by a simple scheme stressing Knibbs's construction of demographic reasoning discussed in Chapter 7. Knibbs did not elaborate on when, why and how what he called 'complete table of fertility' as opposed to 'partial table of fertility' should be used (see Table 1.7.4), but here lies the crux of the one-sex versus two-sex approaches.

Figure 2.11.1 From natality to fertility through nuptiality

$$\left|\begin{matrix}\text{DEMOGRPAHIC}\\ \text{REPRODUCTION}\end{matrix}\right\rangle \to (\text{Natality}) \to (\text{Nuptiality})\begin{cases}\left\{\begin{matrix}\text{One - sex}\\ \text{fertility}\\ \text{analysis}\end{matrix}\right\} \to \text{Fertility output}\\ \\ \left\{\begin{matrix}\text{Two - sex}\\ \text{fertility}\\ \text{analysis}\end{matrix}\right\} \to \text{Fertility outcome}\end{cases}$$

Knibbs's sketch of demographic reproduction was overshadowed by the widespread recognition of Lotka's mainstream stable population theory. While in the 1930s demographers where still hesitant as to the applicability of Lotka's theory, its transformation from a continuous to discrete time approach by Feller (1941), Bernardelli (1941), Lewis (1942) and Leslie (1945) converted the one-sex stable population theory into the mainstream demographic theory of the twentieth century.

Ironically, it was also in the 1940s that the consistency of Lotka's theory started to be questioned. As I have discussed in Chapter 10, Lotka (1939; Lotka and Dublin, 1936) and Kuczynski (1932) had acknowledged the different results when the existing one-sex fertility and reproductivity measures were applied calculated independently for females and males. However, this only started to be treated as a theoretical deficiency or problem by Tietze (1939), Myers (1941), Vincent (1946), Pollard (1948), and more comprehensively and fully Karmel (1947, 1948a, b, c, d, e).

In Appendix B, following Knibbs's work of 1917 I stress two classification drawn from Karmel's (1948) Ph.D thesis for three objectives. First, Karmel's review of the literature reflects accurately the development of demographic literature leading to his own work. Secondly, to my knowledge Karmel's Ph.D thesis was never cited even in the area of the 'two-sex problem'.[1] This is interesting, first because of the importance of Karmel's work for the development of formal demography and even the controversy that his theory provoked late in the 1940s; secondly, the parts of Karmel's work that have been acknowledged and extensively cited are his three articles published in 1947 and 1948; but his PhD thesis submitted in October of 1948 contains his most comprehensive and lengthy treatment of the 'conflict between male and female measures'.

But there is an even stronger reason why I provided Karmel's own classification of the literature on the 'attitude of demographers to the male-female conflict' in Appendix B. He classified the literature in two groups: the 'direct recognition of the conflict' (Karmel, 1948c: 34-44), and the 'indirect recognition via nuptiality' (Karmel, 1948c: 44-50). The direct recognition of the 'male-female conflict' refers to the argument that fertility and reproductive measures applied independently to both sexes produced inconsistent results. The indirect recognition of the male-female conflict refers to the literature recognizing the importance of nuptiality for the analysis of fertility and reproduction. In other words, the literature included in Appendix B under the heading 'Indirect recognition of the male-conflict via nuptiality' focused on issues since the 1950s become part of the study of determinants of fertility.[2]

Searching for **la 'tendance profonde'** ***in monogamic populations***

Conventional literature correctly credits three authors as the forerunners of the two-sex problem: the French demographer P. Vincent, for his paper published in 1946 called 'De la mesure du taux intrinsèque d'accroissement naturel dans les populations monogames'; the Australian economist-demographer P. Karmel, for his three articles published in 1947 and 1948; and the Australian mathematical demographer A. Pollard, for his article published in 1948, 'The measurement of reproductivity'.

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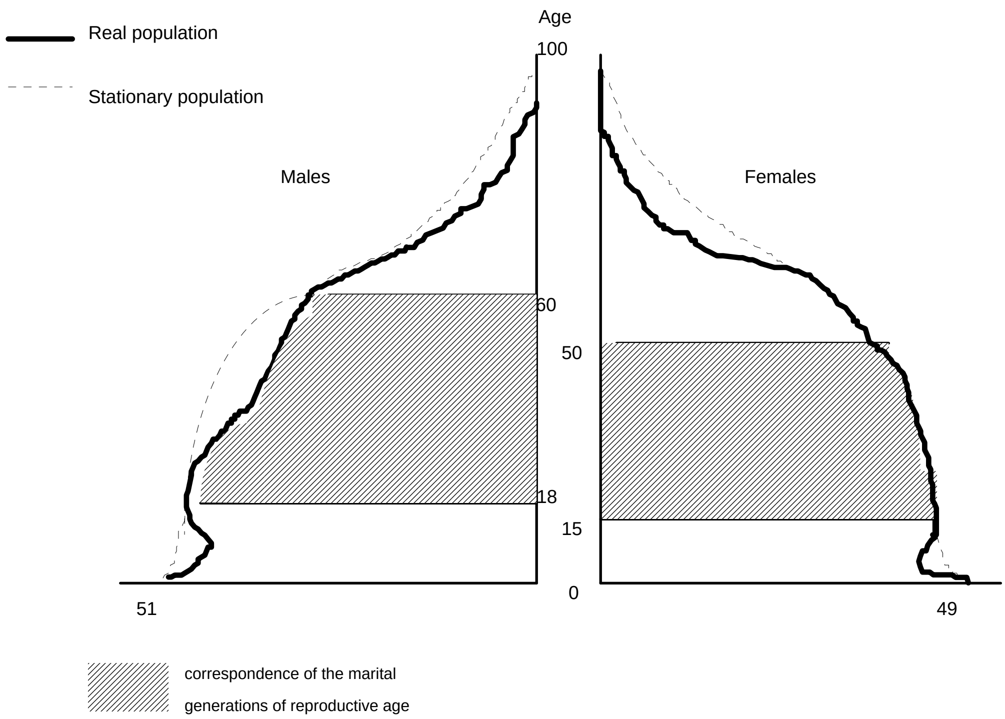
[1] I was advised of the existence of this thesis by Karmel himself, when I approached him personally after learning that he was living in the same city where I was working on this thesis. Professor Karmel kindly lent me his own copy of his thesis and allowed me to use it in this work.

[2] On this Appendix B, one of the examiners considered it 'an excellent idea which should help orient the reader lost in the main text'. However, the examiner added: 'its chronology would be most useful if its users could devote nearly as much time to the study of two-sex demography as was devoted to that study by its compiler'.

These three works were developed simultaneously and independently from one another, and represent the culmination of the mounting dissatisfaction with the one-sex approaches and measures on fertility and reproduction. Vincent (1946) questioned the reasoning for preferring the use of rates based on the female part of the population to the use of rates based on the male part when the population is monogamous. Figure 2.11.2 reproduces one of Vincent's (1946: 706) graphical illustrations of the male-female conflict, particular the dependence of fertility conditions of the two sexes on the distribution of population by sex and age.

Vincent maintained that the populations studied by demographers are strongly monogamous, which means that the two sexes play a similar enough role in procreation and are in no way independent. The fertility functions of the two sexes are tied by nuptiality, which depends itself on the age distribution of the marriageable population of each sex. This distribution in an actual population often is not comparable with what it would be in stable population; but the '*tendance profonde*' under investigation should be a function of both sexes' schedules leading to a single intrinsic rate.

Figure 2.11.2: Vincent's scheme on fertility conditions for both sexes



Source: Vincent, 1946: 706

Why has Karmel's Ph.D thesis been ignored, and why does this matter?

Karmel's widely acknowledged articles are the three papers published in 1947 and 1948 in the first volume of *Population Studies*. Karmel's first article, 'The relations between male and female reproduction rates', set the research problem that has ever since provided the motivation for the search for an adequate and widely-accepted two-sex model for the measurement of reproductivity:

> Although reproduction rates are usually calculated in terms of maternities, it is well established that they can also be calculated in terms of paternities. Hence it is possible to calculate for a given region at a given time both male and female gross and net reproduction rates. Frequently, such male and female rates give contradictory results, in that they do not lie on the same side of unity. More often they give results, which, although not so obviously paradoxical, are nevertheless inconsistent in that they yield very different stable rates of growth for the two sexes (Karmel, 1947: 249)

The fact that this and the other two articles were published, and thus made available to a wide public, seems to be the only valid reason for the emphasis they have received as opposed to his Ph.D thesis submitted in October 1948.[3] Since no author seems to have ever claimed that Karmel's published papers dispensed any additional remarks to his Ph.D thesis there is no point in rejecting such a conjecture. Nor is the idea that Karmel's thesis was never published a valid argument for not mentioning it, particularly when at least three other unpublished Ph.D thesises dedicated to the two-sex problem have been included in the literature on the 'two-sex problem. I refer particular to the doctoral thesises by McFarland (1971), Feeney (1972), and Wijewickrema (1980), to which I return below.

So why has Karmel's Ph.D been completely ignored, and why does this matter? The answer to this question seems important here because it reinforces my assertion about the weak level of inquiry of the debates on the two-sex problem. When demographers do not ignore the issue raised by Vincent and Karmel, which is concerned with the fundamental basis of demographic measurement, few have discussed its theoretical substance adequately. Perhaps the best and most substantiated review of Karmel's reasoning appeared immediately after his first article, Hajnal's 'comments on Mr Karmel's (1947) paper.

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[3] Karmel's thesis entitled 'The measurement of reproductivity in relation to the conflict between male and female measures' comprises the following twelve chapters: 1. Theoretical structure of measures of reproductivity. 2. Conflict between male and female measures of reproductivity. 3. Attitude of demographers to the male-female conflict. 4. Male versus female systems for measuring reproductivity. 5. Conditions for consistency between male and female systems. 6. Relations between simple male and female measures of reproductivity in a stable population. 7. Relations between male and female nuptiality conditions in a stable population. 8. Tabulations of relations between male and female nuptiality conditions in a stable population. 9. Relations between nuptiality controlled male and female measures of reproductivity in a stable population. 10. An arithmetical illustration. 11. Sources and magnitudes of conflicts between male and female measures of reproductivity in actual populations. 12. Resolution of the conflict.

> I do not think, however, that the majority of those who have used the customary indices based exclusively on females would regard the case against them as made out because there is a conflict between the results obtained when these methods of computation are applied both to the male and to the female section of the population. The existence of this conflict has in fact been widely recognized by many of those who have always used indices based on females (including Dr Kuczynski and Dr Lotka). The reason for using maternal rates, none the less, have never to my knowledge been fully stated (perhaps because they were never clearly thought out by any one). But I believe the maternal rates were felt by many to be more 'fundamental' (Hajnal, 1948a: 354)

In some ways, this statement acknowledges the superficial level of discussion concerning the reason many demographers considered maternity rates more 'fundamental' than those which could be calculated on the basis of a male-only population. In Chapter 10 I drew attention to Hajnal's (1948a: 355) rejection of the 'reasons of convenience for working on maternal rather than paternal rates'. However, Hajnal himself did not elaborate on what might be called 'reasons of principle'; he spoke of 'physiological' reasons, presumably assuming that physiological reasons are by principle more fundamental than, say, social, moral, economic, and ideational.

Hajnal did take the matter far enough and like Karmel (1948c, 1948d), frequently appealed to the authority of commonsense, which is apparent in the use of terms such as: 'present custom', 'customary indices', 'passing from the actual to the stable population', 'obvious physiological reasons'. It happens that in science, as in other aspects of social life, customs do not occur by accident and without purpose. But throughout his work, including the thesis, Karmel (1948c) often ridiculed the one-sex measurement and in particular the reasons given by Lotka and Kuczynski for excluding male measures from consideration in a number of standard demographic works (see Box 2.10.1):

> It certainly must have seemed peculiar to any layman reading the works, for if female births are potential future mothers are not male births potential future fathers? Every birth must have two parents. What seems to have been behind this very reason is the idea that the female do represent the limiting factor in population growth, although this has never been explicitly stated as a reason for using female measures (Karmel, 1948c: 52).

Karmel has never attempted to draw any relevant theoretical principle from Knibbs's idea that females are the 'producing sex' within the demographic system. So Karmel's and Hajnal's rejection of 'reasons of convenience' for calculating female rather than male reproductive measures have been portrayed, if not ridiculed, as unrealistic and theoretically unsubstantiated.

Hajnal's (1948a: 355) following remark exposes another weakness in Karmel's theoretical reasoning: 'the argument against the customary reproduction rates seems to me to be solidly founded only if marriage is brought explicitly into the discussion' (Hajnal, 1948a: 355). Karmel's Ph.D thesis (1948c: 59-75; see also Karmel, 1948e) is the work where one can find his best discussion concerning the 'conditions under which either male or female systems are valid'.

Hajnal's remark as to when the argument against the customary reproduction rates is 'solidly founded' was necessary because Karmel's published papers are ambiguous with regard to when and why both sexes should matter. However, following Hajnal's comments the issue has never been taken seriously, and those who have attempted to develop two-sex models usually take Karmel's statement of the problem as given.

There is still another interesting passage in Hajnal's (1948a) comment, where he wondered whether Karmel's two-sex method of finding a true rate of natural increase could add any new insight to that already attained on the basis of the one-sex methods.

> Such a method of finding a true rate of natural increase is perhaps desirable on grounds of logic, but even if it could be found and if it were simple enough in computation to be practicable, it is highly doubtful if under present circumstances its use would be justified (for example, the problem of constructing fertility rates to represent long-run trends is in too fluid a state). As a preliminary step it is very important to know how much difference the whole problem may make to the value of *r* (or the reproduction rate) and correspondingly to the general assessment of the demographic situation. This question can be answered by studying how the nuptiality conditions of men must be related to those of women in stable populations with various rates of increase. This procedure enables one to investigate to what extent the nuptiality conditions of a given time and place may in fact be inconsistent (Hajnal, 1948a: 356).

Hajnal raises here another issue which has never been adequately explored in the debate on the two-sex problem; this concerns a broader problem which Berlinski (1976) discussed in general in his discussion 'On systems analysis':

> it is not always necessary to subject an analytically intractable system to simulation in order to understand it qualitatively; correspondingly, qualitative insights are at greater depth than partially quantitative results. The moral: look to systems for which a qualitative analysis is possible (Berlinski, 1976: 83-84).

Karmel overlooked this issue in his 'rejoinder to Mr Hajnal's comments', and thus missed a good opportunity to place his own debate on the limitations of one-sex methods on a more solid theoretical footing. In this way, Karmel himself seems to have contributed to the neglect of his most important work, the Ph.D thesis. This in itself does not explain the neglect of the issues that Karmel addressed more fully in his thesis.

In any case, there are at least three main reasons which justified Karmel's Ph.D thesis receiving even more attention than his published articles. First, Karmel submitted his thesis in October 1948, after having had the opportunity to debate extensively the content of his three published articles. Secondly, at least Hajnal's (1948a) comments seem to have helped Karmel to clarify and correct some of his arguments. This was particularly acknowledged by Karmel, in his 'Rejoinder to Mr Hajnal's comment', with regard to the term 'net fertility of marriage'. Thirdly, Karmel (1948e: 329-337) was among the visitors invited for a special sessional meeting of the Institute of Actuaries held in Cambridge to discuss A. Pollard's (1948) paper, 'The measurement of reproductivity'. This Institute published Pollard's papers followed by a detailed 'Abstract of the discussion' and Karmel's (1984e: 329-337) written notes prepared for the occasion.

In short, these three reasons seem to make it clear that the absence of Karmel's Ph.D thesis from the literature on the two-sex problem is a striking flaw. As it is his most comprehensive, lengthy, elegant and latest work, there is no good reason for the absence of Karmel's Ph.D thesis from the literature on the two-sex problem.

'The measurement of reproductivity' and subject of fertility in 1940s

In 1948 Pollard was awarded the *Rhodes Prize* by the Institute of Actuaries for his paper called 'The measurement of reproductivity', apparently written three years earlier (see Bailey, 1948: 322). This paper proposed another two-sex model for the interdependence between male and female rates in non-stable populations; the model integrates fertility functions for daughters to fathers and sons to mothers, and then the two-sex intrinsic rate of natural increase becomes an arithmetic mean of both sexes' intrinsic rates weighted by the respective generation lengths.

Following Pollard's award of the *Rhodes Prize* the Journal of the Institute of Actuaries organized a sessional meeting to discuss his paper. A brief review of this discussion drawn from the 'Abstract of the Discussion' published in the *Journal of the Institute of Actuaries* (1948: 319-337) seems interest here. In particular, it is interesting to notice how in late 1940s mathematical demographers searching for 'two-sex' measures of population growth, reproductivity and fertility did not appear to be dealing with something totally strange from what interested the empirical demographers of the time.

Among the scholars who participated were Pollard, members of the Institute and visitors: M.A.H. Rowell (the President), P.R. Cox, W.G. Bailey, H.O. Worger, P.H. Karmel (visitor), W.A.B. Hopkin (visitor), Prof. R.A. Fisher FRS, H.W. Haycocks, W. Perks.

Pollard, in introducing his paper, explained its background, particularly that it was largely inspired by two papers that appeared in an Australian journal, one by Karmel (1944) and the other by Clark and Dyne (1946). Then, Cox opened the discussion pointing out that Pollard’s paper dealt with a very interesting topic and a suitable subject for actuaries to examine, though unfortunately it was one not often raised at Institute meetings. A reconsideration of the means of measurement of reproductivity was especially opportune at the present time, because the study of fertility had been making big strides recently and ideas about it were undergoing some changes.The author's contribution was therefore particularly welcome. It covered a wide field and raised some interesting technical questions. Section 5 consisted of an ingenious development of Lotka's mathematical formulae of the stable population in an attempt to eliminate inconsistencies between the sexes. There were some elegant formulae at this point, especially on page 307, which could not fail to appeal to the mathematically minded.

The subject of fertility was suitable for actuaries to discuss because their training gave them the power to get down to fundamentals in demographic matters and it was very important at the beginning of the discusion to consider what was really fundamental in fertility. In order to emphasize the essentials in fertility, certain significant facts were worth noting. First, there had been a big decline in fertility since the last century, due to the use of contraception. Secondly, there had been a rise, probably followed by a fall, in differential fertility, i. e. the extent to which various social classes of the population differed from each other in their fertility experience. That indicated, apparently, an increasing use of contraception, starting with the upper classes and proceeding downwards throughout the whole of the population. It was known that illegitimate births were small relative to the total, and that infertility was probably not a large factor. Cox deduced, therefore, that a very large part of fertility consisted of a process of family building according to the plans which married couples made as to the families which they wanted. Fertility and of course reproduction were in fact dependent on human volition. In consequence, one should seek to study fertility principally through the five following essential factors: family size; time at which the marriage of the parents took place; ages of parents at marriage; time at which the birth of the child took place; and possibly also the social class of the parents.

Examining Pollard's paper with these points in mind, Cox observed that it was necessary to distinguish fertility and reproduction; age attained played a very large part in the analysis, but this variable was not one of the fundamental factors. There was also the generation effect, and while the duration of marriage was not fundamental to fertility it went hand in hand with family size. With regard to the possibility of a joint rate of increase judging by recent researches, particularly those of Karmel, it seemed doubtful whether it was possible to reconcile male and female fertility functions based on attained age alone. However, Pollard's core equation for an unified rate of increase struck, as Karmel suggested, at the root of the Lotka mathematical system, because it meant that the population could not tend towards a stable state with constant fertility rates for both sexes. This threw considerable doubt on the worth of the reproduction rate, and the recent researches which had been made in thus subject had brought out the big difference between male and female rates and the great difficulty of reconciling them. It did not seem that the difficulty was really circumvented by combining male and female rates in a double integral.

Bailey (1948: 322-323) followed Cox saying that Pollard was entitled to claim that if use could be made of his model he was justified in putting it forward. However he had been disappointed to find that the author had been content to follow the lead of the population mathematicians and confine his attention to that side of the subject. The trouble about demography was that so far it had been under the domination of the population mathematicians, whereas normally one would expect the mathematician to be a servant of the investigator. The cause of that was lack of data, particularly on exposuse to risk. The result had been, in his view, the production of models the main purpose of which appeared to be to give the maximum scope for the peculiar technique of the population mathematician, without any particular regard to the purpose of the models or even to their utility. In fact, he did not think that he had ever seen stated the purpose behind the model or the index derived.

Bailey then discussed the two orthodox interpretations of the net reproduction rate. The first was that it showed the rate at which women now in childbearing ages were replacing themselves in the next generation. The second, if used, speculated on mortality and fertility. Yet, an attempt to adjust the net reproduction rate to fit in with Karmel's or Clark and Dyne's formulae and introduce the factor of marriage, objected to the nature of the reproduction index. Like Cox, Bailey distinguished reproduction and fertility; but he would like to see, if models were being constructed, some attempt to construct a model which not only held good at infinity, but would provide some information regarding the period when the population was passing through a stage resulting from a lack of balance amongst the sexes. He would like also to see provision made in such a model for the hypothetical effect of a change in marriage rates resulting from a change in that balance, and of a change in fertility rates as affected by change in the marriage rates. It seemed to him that the dynamic conception of the change in population was considerably more important than what the condition of the population might be expected to be if it ever became stable.

One of the reasons for the veneration of the net reproduction rate, Bailey continued, was that it appeared to give a criterion against which it was possible to measure the results of current fertility. Actually it was a very poor criterion. Pollard had taken the view that if the net reproduction rate fell below 1, then the population was bound to fall, whereas it was known that that was not true; it was possible to put up for a considerable period with a net reproduction rate below 1 without in the end suffering a fall in population. Yet as a fertility measure the net reproduction rate could perhaps be further improved by the introduction of birth parities, though that was open to the objection that it neglected generational effects. He though that an index of current fertility was needed for studying correlations. After all, not very much would be known about population until a study was made of the correlation between fertility rates and economic or other factors.

The discussion proceeded from Worger to the visitors, Karmel and Hopkin, and other members of the Institute mentioned above. Several issues were raised, and it is enough to list only the main points to complete the picture of the debate: the increasing interest of governments in the reproduction and fertility rates; the possibility that some governments would try to increase the number of births and, for that, encourage earlier marriages; that many people merely planned to have a fixed number of children; the problems of assuming an indefinite flexibility in the sex ratio in a method attempting to reconcile the male and female reproduction rates; the need to discover which was in fact the dominant sex in matters of marriage; the need to distinguish carefully between mere index numbers of fertility and the more elaborate process of constructing population models; that fertility was a function of other things besides age, including duration of marriage, birth order; the need to study reproductivity changes in generations.

The end of a remarkable decade and the beginning of an unexpected split

Still before ending the 1940s Stolnitz and Ryder (1949) reviewed the 'recent discussion of the net reproduction rate', namely the methodological background of its simple synthetic form, proposed refinements of the synthetic approach as opposed to real cohort reproductivity. 'By 1940', Stolnitz and Ryder (1949: 116) wrote, 'the synthetic net reproduction rate, customarily female, was the single measure most often used in projective and comparative analyses of reproductivity'. However, at the beginning of a new decade the traditional role of the net reproduction rate had been called into serious question. This indicator was not likely to continue as unqualified in the future as it had been in the past; each of its major properties, namely its synthetic nature, dependence on female experience, and the use of rates specific for age only, had been strongly criticized. Thus, in view of the existing criticisms, 'the traditional net rate cannot be expected to retain a central analytical position in future demographic research, at least for Western nations' (Stolnitz and Ryder, 1949: 124).

Stolnitz and Ryder (1949: 124) found it premature to particularize probable future developments because recent discussion had 'not yet crystallized into a positive confirmation of methods needed in the study of reproductivity'. In any case, they considered that the literature of the past few years was broadly suggestive of new directions of methodological emphasis and research interest and accurately predicted some particular areas in need of improvement:

> Future research is likely to be centrally focused on the structure and timing of individual family formation and completion. The shift of emphasis to a micro-demographic point of view is implied by the recent stress on the importance of refining vital statistics measures by the introduction of additional controls ... Events of the past few years have reaffirmed the long recognized need for the development and testing of dynamic models of demographic response to changing social, economic, and psychological climates. Population study has developed no conceptual framework for investigating short-run variations in marital and childbearing patterns. In addition, theoretical consideration of long-run, as distinguished from the short-run, aspects of population change is likely to be an increasingly important prerequisite to the refinement of future empirical research. The proliferation of emphasis on generation reproductivity is symptomatic of growing research interest in this direction. A promising avenue of future investigation is the analysis of rates pooling the reproductive experience of successive real male and female age or marriage cohorts.
>
> Research in these directions will require substantial extension of theoretical and methodological frontiers. Such advance will undoubtedly enable more efficient exploitation of available empirical data (Stolnitz and Ryder, 1949: 124-125).

Following the burst provoked by Vincent, Karmel, and Pollard, the 1940s closed with a remarkable new area securing its own place in the field of mathematical demography: two-sex modelling. Perhaps the last important contribution of the 1940s was the one by Kendall who attempted to model the marriage process.

Davis and Blake's model of 'intermediate variables of fertility': Karmel's anticipation

Although the research on the 'two-sex problem' was born at the same time and with similar concerns as the study of 'determinant of fertility' in the 1950s, they developed in different directions like two brothers fostered by different families.

There is a point in this analogy. For some the estrangement between the two fields was a reasonable solution for what Bailey called, in his discussion of Pollard's (1948) paper described above, 'the trouble about demography': that demography had been under the domination of the population mathematicians (Bailey, 1948: 322). Others would have perhaps thought that the 'determinants of fertility' come from a purely sociological stream of demography. In Chapter 9 I drew attention to a similar misconception of the origin of demographic transition theory. But most probably Thompson (1929) would also be surprised with the current alienation of demographic transition from Lotka's mathematical system of population.

The subject of two-sex models lay dormant during the 1950s and so it remained until the mid-1960s; the only contributions during a decade and half were those of Yntema (1952) and Goodman (1953). In turn, the modelling process of determinants of fertility in terms of a systematic framework of 'intermediate fertility variables' was born in the mid-1950s from two main sources: the sociological article by Davis and Blake (1956), ‘Social structure and fertility: an analytical framework’, and the more mathematical work by Henry (1953, 1957, 1961).

One of the negative aspects of having isolated the two-sex problem from any relevant theoretical principle as far as the determinants of fertility are concerned is that demographers have been deprived of some other interesting aspects of Karmel's endeavour 'to replace the customary one-sex dimensional methods of measurement by two-sex dimensional methods' (Karmel, 1948c: ii).

Even though his Ph.D thesis has received no attention in the literature, at least Karmel's (1948d) 'rejoinder to Mr Hajnal's comments' contains what seems to be an interesting anticipation of Davis and Blake's model of 'intermediate variables'. Table 2.11.2 presents side-by-side Karmel's sketch of his debate upon the 'condition under which either male or female systems are valid' and the intermediate variables as listed by Davis and Blake.

Since the reasons for using reproduction rates based on the female part of the population to the exclusion of the male part had never been fully stated, Karmel found it worth while setting out the implicit assumptions involved when population replacement is measured by indices referring to only one of the two sexes.

> If the rate of replacement of a population is measured by the net reproduction rate or the true rate of increase of one sex, this implies that that rate is unaffected by the relative numbers and age distribution of the other sex. The level of these rates depends on the probabilities of surviving to each year of age and the probabilities of producing a birth at each year of age, but the latter must be regarded as being compounded of three elements, namely, the frequency of sexual intercourse, the probability that intercourse will lead to a conception, and the probability that conception will result in a live birth (Karmel, 1948d: 361).

Table 2.11.2 The intermediate variables: Karmel and Davis & Blake

Karmel's sketch of the 'condition under which either male and female systems are valid'	**The intermediate variables as listed by Davis and Blake**
f the female-only system is valid to measure reproductivity, this means that the males must be unimportant as a factor affecting fertility, the functions entering into the system are independent of the relative number of males in the population and their distribution. The one-sex system applied independently to both sexes to measure reproductivity is determined by two fundamental functions, survivorship rates $\lambda(x)$ and fertility rates $\beta(x)$, but the latter must be regarded as being compounded of three elements, namely, • the frequency of sexual *intercourse*, • the probability that intercourse will lead to a *conception*, • the probability that conception will result in a *live birth*. Thus for female systems to be valid, i. e., for the use of female measures of reproductivity to be valid, it is necessary that: (i) the probability of females surviving to each year of age - $l_F(x)$; (ii) the frequency of sexual intercourse at each age of female - $i_F(x)$; (iii) the probability that intercourse will lead to conception at each age of female - $c_F(x)$; (iv) the probability that conception will result in a live birth at each age of female - $g_F(x)$, are all independent of the relative number of males in the population and of their age distribution. A similar model of conditions for male measures can be outlined (i) $l_M(y)$, (ii) $i_M(y)$, (iii) $c_M(y)$, (iv) $g_M(y)$. Moreover, it is important to consider the dependence of $c_M(y)$, $c_F(x)$, $g_M(y)$, $g_F(x)$ on the opposite sex, for the following three cases: (a) complete sexual promiscuity; (b) females perfectly marriage dominant; (c) males perfectly marriage dominant. Karmel, 1948c: 59 -75; 1948d: 361-364.	The process of reproduction involves three necessary steps sufficiently obvious to be generally recognized in human culture: (1) intercourse, (2) conception, and (3) gestation and parturition. In analyzing cultural influences on fertility, one may well start with the factors directly connected with these three steps. Such factors would be those through which, and only through which, cultural conditions *can* affect fertility ... by way of convenience, they can be called the 'intermediate variables as follows I. *Intercourse variables* (factors affecting exposure to intercourse) A. Formation and dissolution of unions in the reproductive period. 1. Age of entry into sexual unions. 2. Permanent celibacy: proportion of women never entering sexual unions. 3. Amount of reproductive period spent after or between unions. a. When unions are broken by divorce, separation, or desertion. b. When unions are broken by death of husband. B. Exposure to intercourse within unions 4. Voluntary abstinence 5. Involuntary abstinence (from impotence, illness, unavoidable but temporary separation) 6. Coital frequency (excluding periods of abstinence) II. *Conception variables* (Factors affecting exposure to conception) 7. Fecundity or infecundity, as affected by involuntary causes 8. Use or nonuse of contraception 9. Fecundity or infecundity, as affected by voluntary causes (sterilization, subincision, medical treatment, etc.) III. *Gestation variables* (factors affecting gestation and successful parturition) 10. Foetal mortality from involuntary causes 11. Foetal mortality from voluntary causes. Davis and Blake, 1956: 211-212

While Davis and Blake aimed at setting forth an analytical framework for the comparative sociology of fertility, Karmel was concerned with an issue which at some stage needed the consideration of those who became interested in developing Davis and Blake's model. Put generally, the issue raised by Karmel was, under which conditions is it correct to study fertility on the basis of female-only population, and in particular to assume that the frequency of sexual intercourse at each age of females could be assumed independent of the relative number and age of distribution of males?

Two alternative answers were provided by Karmel to this question. First, female's sexual intercourse could be regarded as independent of males if there were complete sexual promiscuity in the population; or as Vincent (1946: 709) put it, a 'complete sharing of females'. Davis and Blake were not concerned with the sociology of fertility of hypothetical complete sexual promiscuity, but with populations in which monogamous marriage or similar institutions are the rule. Secondly, the frequency of females' sexual intercourse would be independent of the relative number of males and their age distribution if the females were 'perfectly marriage dominant'.[4]

In this case, $c_F(x)$ and $g_F(x)$ will be independent of the relative number and age distribution, because the age distribution of married couples will be determined independently of the male sex, and once this is determined the distribution of the age of sexual partners of females aged x is given. Hence in such a population $i_F(x)$, $c_F(x)$ and $g_F(x)$ will be independent of the male sex, and the use of measurement and analysis of fertility and reproduction on the basis of the female-only population would be valid.

Since Davis and Blake set their model aiming to study the organizational rather than structural conditions influencing fertility change, it is striking that the issues raised by Karmel did not come into the discussion; particularly when specific operational designs for fertility analysis have been discussed.

Karmel found it curious that demographers tacitly assume in their studies of reproduction and fertility a perfect marriage dominance on the part of the females.

> ... since it seems more sensible on a priori grounds to assume that the dominance is with the males. Since the male is the breadwinner, then for economic reasons the initiative in marriage must lie largely with him. This view has in fact been taken by a number of demographers in discussions involving questions of nuptiality, and at least one, Connor, advocated the use of what was a male measure of reproductivity precisely for this reason. But in general, although demographers, if asked which sex was likely to be marriage dominant, would probably have suggested the male sex, they have used measures of reproductivity implying dominance of the female sex. They have probably done this because the usual measures of reproductivity are not

[4] By the female being perfectly marriage dominant Karmel meant that the proportions in which females aged x are married and the distribution of those married females according to the age of their husband are determined entirely from the female side without any dependence on the numbers and age distribution of the males available for marriage (Karmel, 1948c: 62). Karmel also considered the case of 'males perfectly marriage dominant', but despite some rare exceptions the determinants of fertility are generally focused on females only rather than on males.

> nuptially controlled and hence nuptiality has not entered explicitly into the discussion (Karmel, 1948c: 67-68).[5]

But Davis and Blake meant to bring nuptiality into the discussion on the sociology of fertility. This should mean that neither sex can be perfectly marriage dominant, and Karmel detailed the reasons and implications resulting from that with regard to the sex ratio and the balance of the sexes.

When the two-sex modelling became a curiosity for mathematicians

The isolation of the study of determinants of fertility from a possible two-sex approach intensified in the 1950s and 1960s in several ways: first, by ignoring the debates on the 'two-sex problem' in the 1940s which raised not just technical but substantial questions, both in conceptual and methodological terms; secondly, during the 1950s and 1960s the measurement and interpretation of fertility preferences become an increasingly relevant research issue for demographers.

Although the distinction proposed here between fertility output and fertility outcome was not explicitly elaborated, the third bifurcation depicted in Figures 1.6.1 and 1.6.3 between such concepts was happening when people considered the distinction between 'fertility' and 'fertility desires' or 'ideal family size'. Why did demographers simply assume that females not only give birth to children alone, but construct their desired fertility independent of males? Despite the precedent with regard to the descriptive measures of reproductivity and fertility, which Karmel (1948d), Hajnal (1948a) and others tried to overcome, once again demographers simply took the female-sex population as given at the level of explanatory research.

The following description illustrates well the shift to and increasingly reliance on the one-sex approach in empirical research on fertility in the United States:

> Over the past 50 years, three major U. S. fertility surveys directly measured husband's fertility desires: the 1941 Indianopolis Fertility Survey ... the 1957 Princeton Fertility Survey ... and the 1975 Value of Children Survey ... Only the Princeton Surveys also collected longitudinal data to estimate effects of spouses' desires on births ... In the 1970s, several local surveys collected wife-husband data on desires for the next child, along with data on fertility behavior and outcomes for the following year or two ... All of the prospective studies reported significant effects of husband's desires on couple fertility behavior or outcomes. Despite these findings, national fertility surveys continue to rely exclusively on female respondents, not only for data on contraceptive behavior and births but also to measure fertility desires. In the past, married women were asked about their husband's fertility desires, but the two most recent National Surveys of Family

[5] This statement deserves to be contrasted with the following indictment of demography's androcentrism:

> While the other social sciences have been thoroughly interrogated and to varying extents transformed by feminist scholars, demography has taken only the first step of adding women to the equation ... The more fundamental tasks - of challenging basic assumptions and reformulating central concepts - remain largely undone (Greenhalgh and Li, 1995: 602).

Growth omitted even these proxy measures of husband's desired fertility (Thomson et al., 1990: 579).

In the theoretical area earlier study of determinants of fertility was hardly the prerogative of the one-sex approach only, and important neuter approaches were proposed in the 1950s and 1960s.

The bibliography provided in Appendix B mentions several works which became seminal in their own right, including those by Becker (1960, 1965), Freedman (1961), Hill, Stycos and Back (1959), and United Nations (1953). In 1968 Tien reviewed these works, with the exception of Becker's, and identified three main models for the comparative analysis of fertility behaviour: the institutional, interactional, and normative models. First, the Davis and Blake model focused upon institutional mechanisms in society and the 'intermediate variables' that link these mechanisms to fertility. Second, Hill et al. (1959) used the nuclear family as a planning and decision-making association to develop the interactional frame of reference in studying family planning in Puerto Rico. And, third, Freedman (1961) proposed a 'normative' model using elements from both institutional and interactional models. After reviewing these three models, Tien maintained that ways in which fertility change may be initiated or quickened lie in areas other than institutional change, and that it is the demographic, technological, institutional, and information variables that are of substantive consequence in the comparative sociology of fertility.

‘Can there be a marriage function?’

In the mid-1960s research on two-sex modelling was resumed, and since the article by Keyfitz (1965), 'On the interaction of population', the investigation gathered its own momentum. Indeed, it has never been abandoned as in the 1950s and half of the 1960s.

The central issue under investigation in this period is posed by the title of this section, which is a paraphrase of the title of Parlett's paper published in 1972. Rather than reviewing the core papers published in the 1960s and 1970s, I refer to the reviews given by McFarland (1972) Parlett (1972) and Pollard (1973, 1995). Here I prefer to draw attention to three doctoral dissertations which, together with Karmel's thesis, provide the most comprehensive and lenghty debates concerning the 'two-sex problem': McFarland (1971), Feeney (1972), and Wijewickrema (1980).

The Ph.D thesises by McFarland (1971) and Feeney (1972) tackle the issues of marriage functions. McFarland set his debate on 'the problem of the sexes' in the context of the 'mathematics of the family'.

> Marriage, per se, is of only secondary importance in a model designed for population projection. But it is crucial for a complete understanding of the entire demographic process, and is absolutely essential for an adequate treatment of the Problem of the Sexes. For this reason it deserves consideration in its own right (McFarland, 1971: 11-12).

As the title of McFarland's thesis suggests he dedicated his work to the development of 'A model of the marriage market, with implications for two-sex population growth'. However, it was in a paper published in the following year that McFarland (1972) provided the best synthesis of his own approach, if not the overall debate on 'Alternative marriage models' to date. MacFarland defined a marriage function as

> a function for predicting the numbers of marriages which will occur during a unit of time, between males in particular categories and females in particular categories, from knowledge of the numbers available in the various categories. More specifically, let $\mathrm{M_i}$ and $\mathrm{F_j}$ denote the number of available males and females in the i-th and j-th categories respectively, and let $\underline{\mathrm{M}}$ and $\underline{\mathrm{F}}$ denote the vectors of which they are elements. Then a marriage function is a function $\mathrm{C_{ij}}(\underline{M},\underline{F})$, whose arguments are i, the male category; j, the female category; and the numbers available in each of the categories. The problem, then, is to determine the form that such a function should take; i. e., to determine how the changes in availability would affect the frequencies of marriage between particular pairs of categories (McFarland, 1972: 94).

Then McFarland outlined a set of seven axioms to which the marriage function $\mathrm{C_{ij}}(\underline{M},\underline{F})$ *ought* to conform (see Box 2.11.2). This set of axioms constituted perhaps the best synthesis of the overall investigation since Knibbs's work of 1917 aiming to develop an adequate and widely accepted setting for the construction of marriage functions; recent works on the subject, such as those by Pollard (1995), Pollard and Höhn (1994) and Schoen (1977, 1988), seem to confirm the comprehensiveness of MacFarland’s set of axioms.

Box 2.11.2 McFarland's set of axioms for a marriage function

Axiom 1: The marriage function should be defined for all vectors $\underline{M}$ and $\underline{F}$ whose elements are non-negative integers.

Axiom 2: It should necessarily yield $C_{ij}(\underline{M},\underline{F}) \geq 0$, for i, j, and for all choices of $\underline{M}$ and $\underline{F}$ (The number of marriages occurring cannot be negative).

Axiom 3: It should necessarily yield $\sum_i C_{ij} \geq F_j$ and likewise for the opposite sex, $\sum_j C_{ij} \leq M_i$. That is, the number of marriages cannot exceed the numbers of available females and males.

Axiom 4: The number of marriages should depend heavily on the ages of the males and females, and thus the categories, denoted i and j, should classify then by age (perhaps also cross-classifying by some additional variables) ... at the very minimum, the population should be classified by age, sex, and marital status.

Axiom 5: C_{ij} should be a nondecreasing function of M_i, as it varies with the other arguments held constant. Furthermore, for each particular set of fixed values of the other arguments, there exists an internal of values for M_i over which C_{ij} is a strictly increasing function. And likewise with the sexes interchanged. In other words, an increase in availability can never decrease the number of marriages, and if the category in question was initially scarce relative to demand, an increase in availability will actually increase the number of marriages.

Axiom 6: C_{ij} should be a nonincreasing (and, over some intervals, strictly decreasing) function of M_h, for $h \neq i$. And likewise with the sexes interchanged. This axiom incorporates competition.

Axiom 7: The closer of two categories will have the larger negative effect on the marriages of the focal category: $\rho(i,h) < \rho(i,k)$ implies $\left|C_{ij}(M_h+1) - C_{ij}(M_h)\right| \geq \left|C_{ij}(M_k+1) - C_{ij}(M_k)\right|$, with strict inequality unless both sides are zero; and likewise with the sexes interchanged. This corresponds to the substitution axiom.

(McFarland, 1972: 97-100)

Feeney's (1972) Ph.D thesis is entitled 'Marriage rates and population growth: the two-sex problem in demography'. This thesis searches for a marriage function in the aggregate population dynamics defined by sex and age; its overall purpose

> is to develop the analysis of population growth trends through the discovery of empirical regularities and the generation of empirically refutable propositions concerning past, present, and future population phenomena (Feeney, 1972: 15).

Feeney added:

> The explicit introduction of marriage into the study of population dynamics gives rise to a host of questions and problems which remain conveniently beneath the surface in the classical theory based on aggregate age schedules of fertility and mortality. What is the level of nonmarital fertility? How important are fertility differentials by duration of marriage and age of husband? What is the role of widowhood, divorce, and remarriage? How important is differential mortality by marital status? (Feeney, 1972: 17).

Feeney's thesis does not make it clear why and when demographers can overlook marriage from their analysis of aggregate population dynamics. In his Chapter 2 on 'Marriage and sex distribution' Feeney (1972: 18) considered the dual dependence of the incidence of marriage on what he called the 'availability conditions' in the population and the 'marriage habits' of the society. Feeney (1972: 20) traced the failure to deal with this dual dependence 'to the failure of a very basic demographic concept - the demographic rate.' In both cases marriage rates fail to accommodate the dual dependence of the incidence of marriage for the same reason: 'They do not represent only marriage habits, they reflect both marriage habits and availability conditions'. So, according to Feeney, there was a need to create 'some concept which represents marriage habits only and which may therefore play in the study of marriage the role played in the study of mortality by mortality rates' (Feeney, 1972: 20).

Hereafter Feeney systematically explored three main issues. The first was the structure of the class of several mathematical functions and their specific criteria likely to express the dependence of numbers of marriages in a population on the numbers of males and females available for marriages. Secondly, he defined a class of systems of population dynamics which incorporate the process of marriage and in which male and female marriage rates vary in response to change in the numbers and age distributions of marriageable males and females. The third issue was the degree to which these systems exhibit several basic properties established by mainstream stable population theory. Finally, he presented some exploratory computations based on United States data.

Feeney's two-sex model has inspired more recent work. In particular, during the 1980s Pollak (1986, 1987, 1990) has developed what he calls a Birth Matrix-Mating Rule (BMMR). In his paper of 1990 Pollak recognized the similarity between his structure and one outlined by Feeney; the difference, though, is that Pollak was trying to work on a 'satisfactory proof of the existence of equilibrium' not provided by Feeney's model. More recently, Pollard (1995: 17; Pollard and Höhn, 1994: 205-206)) has pointed out that few of the marriage models of the 1970s met all the requirements; in the case of Pollak (1990), Pollard maintains, among his the five axioms only three (axioms 1, 2, and 3) obey McFarland's setting given in Box 2.11.2. Moreover, Pollard remarked on Pollak's model:

> His comment that it is really requirement (3) that makes the two-sex models non-linear is interesting. Pollack's omission of any competition requirements akin to (6) and (7), however, appears strange, at least to this author, who also has certain

> reservations when the economic concept of rational person is applied to demographic events, marriage in particular (Pollard, 1995: 18).

To my knowledge the latest Ph.D thesis dedicated to the 'two-sex problem' is the one by Wijewickrema (1980), called 'Weak ergodicity and the two-sex problem in demography'. This thesis focuses on the nuptiality dimension of two-sex population dynamics with the objective of generalizing the debate on stable population theory to the context of the weak ergodicity.

In the past the alternatives for the one-sex model of stable population theory took as reference Lotka's theory; in its fundamental way Lotka's theory established what in the late 1950s became known as the theorem of strong ergodicity (Coale, 1957a, 1972; Lopez, 1961; McFarland, 1969). The strong ergodicity in Lotka's stable population theory, particularly the idea that the age structure approaches a stable limiting shape, was then placed in the context of a more general theorem called weak ergodicity. 'That is, the age distribution of a closed population tends to "forget" its past shape and to be determined exclusively by the recent history of fertility and mortality' (Lopez, 1961, in Smith and Keyfitz, 1977: 256).

The same inconsistencies and unreality attributed to the theorem of strong ergodicity can be raised with regard to the theorem of weak ergodicity. As Wijewickrema (1980: 5) put it,

> the unrealistic nature of the stable model and its extensions .... they all deal with populations composed of one sex' ... What causes difficulties is not the banal fact that a human population is essentially composed of two sexes and that both sexes should find a place in any process of explanation. There is nothing intrinsically wrong in trying to study a whole in terms of its parts: and the study of one sex, as isolated *formaliter* from the other, is what hits the eye as a first possibility (Wijewickrema, 1980: 5).

Contrary to what happened to mortality, Wijewickrema added, fertility cannot enter submissively into a one-sex scheme of population dynamics. Wijewickrema did not try to be convincing in attempting to explain why that is so. In particular, his justification for the widespread use of the one-sex approach illustrates the superficiality of the argument, which I have already discussed with regard to other works:

> If past demographic reflection and performance has been linked almost entirely to one-sex approaches, it may be attributed in large measure to difficulties arising both from non-availability of adequate data, and the inherent complexities of a two-sex approach. One can very justifiably say that the ease and clarity which accompany one-sex problem formulation have had a hypnotic effect on demographic thinking (Wijewickrema, 1980: 5-6)

Wijewickrema constructed 'a population projection engine' inspired by Henry's (1972) marriage model known as the 'panmictic circles'. This model establishes that people meet and marry and pass through several stages or overlapping 'circles' throughout the process towards marriage. These overlapping circles are considered ‘panmictic’ in the sense that within each circle marriage takes place without regard to age (Henry, 1972;

Wijewickrema, 1980: 48-56; Schoen, 1988: 129-130). After studying the effect of different fertility hypotheses on nuptiality parameters under constant mortality and absence of migration Wijewickrema concluded: first, that current sex-age-marital status structure is independent of sex-age-marital status structure experienced in the remote past, that is, he replicated and confirmed the theorem of weak ergodicity demonstrated by Lopez (1961) on the basis of the one-sex model; secondly, that the results are independent both of the particular two-sex marriage model used and the particular conditions under which the experiments are carried out.

'Modelling the interaction between the sexes': a reconceptualization?

It would be misleading to conclude this overview of the literature on the 'two-sex problem' implying that this field is still moved by the same maverick expectations set forth about half century ago: 'to replace the customary one-dimensional methods of measurement by two-dimensional methods' (Karmel, 1948c: ii).

Over the past five decades Lotka's stable population theory has not only survived scrutiny, but continued to be the source of new theoretical developments; both within its own one-sex principal methodological framework, and in terms of alternatives such as those based on two-sex models. Some, if only a few, continue to insist that the one-sex demographic approach is a fundamental *anomaly*:

> In the terminology of Thomas Kuhn (1970), demographers have generally regarded the two-sex problem as a *puzzle* rather than as a fundamental *anomaly* whose resolution might require recasting the *paradigm* ... Demography's two-sex problem is a fundamental anomaly that can be resolved only by replacing classical stable population theory with a model that recognizes that the observed rates for both females and males are in disequilibrium (Pollak, 1990: 401).

However, contrary to the expectations raised by Kuhnian philosophy the dominant one-sex demography is not dying out, nor is it likely to fade away. This seems to be the reason most authors who have become interested in the 'two-sex problem' had to reconsider their views as to whether and why the one-sex demography needed to be rejected. Without considering those who simply abandoned the topic altogether soon after tackling it, a variety of attitudes can be found among those who have remained directly or indirectly interested in the 'two-sex problem'. Some continue to see the 'two-sex problem' as a purely technical problem, one 'that seems to be beyond any simple solution' (Keyfitz, 1982: 437). Others continue to believe that the search for widely-accepted two-sex models is worthwhile, but they become rather more conciliatory as to the usefulness and validity of one-sex demography. For instance, as Pollard and Höhn (1994: 204) put it, 'For many demographic purposes, the usual one-sex approach applied to the female component of population provides reasonable results'.

Pollard's attitude towards the 'two-sex problem' contrasts with Pollak's more radical stance, but it is also different from Schoen's (1988) revisionist picture of the two-sex problem. In a book published in 1988, *Modeling Multigroup Population*, Schoen avoided the famous assertion that the one-sex approach produces 'inconsistent' results.

> Attempts to simultaneously reflect the experience of both males and females have confronted the 'two-sex problem', the fact that the observed behavioral rates of males and females cannot both be consistently incorporated into either life table or stable population models (Schoen, 1981: 201).

This book is divided into three parts, and follows the path of conventional demographic analysis: mortality-fertility-marriage and other states (see, for instance, Newell, 1988). Schoen's revisionism is not so much in his somewhat uncritical reference to the one-sex and two-sex models. Instead, Schoen assumes the one-sex approach as the standard and deals with two-sex models as its extension rather than a theoretical alternative.

> Following customary practice, the stable population will be considered a female-only population, though the following discussion would be equally valid for a male-only population. (Part III of this book will consider the extension to 'two-sex' models) (Schoen, 1988: 38).

Part I of Schoen's (1988) book focuses on the classical mortality model, the decrement-only life table concerned with the survivorship of a birth cohort, and its associated stable population; Part II examines multistate models, or also increment-decrement or 'combined' models, such as marital status, labour force behaviour, and interregional migration. And then Part III, called 'Two-sex populations models', generalizes the one-sex to two-sex models in five chapers, which provide perhaps the most recent and comprehensive overview of the 'two-sex' models within the scope of the 'two-sex problem'.[6]

The difference between Pollard's and Schoen's definitions of the 'two-sex problem' are subtle. They have both stressed in their recent work that the one-sex models used by demographers are unable to handle 'the necessary interaction between the sexes' (Pollard, 1995: 1). Or, in Schoen's (1988: 121) words, 'The two-sex problem is the

[6] Chapter 6, 'The interaction between the sexes', reviews the alternative solutions for the 'two-sex problem' and introduces the concept of 'magnitude of attraction' between males and females independently of population composition. Chapter 7, 'Two-sex marriage models' discusses six types of two-sex marriage models: two-sex nuptiality life table (TWONUP), marriage squeeze model (MSQUEEZE), two-sex marital status life table (TWIMSLT), two-sex marital status stable population (TWIMSSP), two-sex nuptiality multistate life tables (TWONID), two-sex nuptiality multistate stable population (MIDSQZ). Chapter 8, 'The marriage squeeze', examines the effects on marriage behaviour produced by an imbalance between the numbers of males and females. Chapter 9, 'Two-sex population models', examines two types of two-sex fertility models, the two-sex fertility stable population (TWOGRO) and the two-sex fertility multistate stable population (TWOFIDS); as well it explores the 'birth squeeze', that is the fertility counterpart of the marriage squeeze. Chapter 10, 'Models of interacting populations', examines nuptiality and fertility in populations composed of interacting ethnic or racial subgroups; for intergroup marriage, it proposes a composition-independent measure, applies two-sex nuptiality models, and considers the influence of compositional factors.

inability of conventional population models to capture the changes in nuptiality and fertility rates that are produced by changes in population composition'.

Both definitions continue to use the term 'two-sex problem' as a euphemism for 'one-sex problem'. Moreover, the two definitions do not refer to the conceptual and methodological difficulties of the two-sex modelling itself, namely the need to identify and define research problems that can only be adequately explained in terms of the interaction between the sexes. Most demographers do not use the conventional one-sex population models with the expectation that they capture the changes in population composition. On the contrary, conventional one-sex population models have been developed with the objective of stripping off part of the effects of population composition. But as I have demonstrated in previous chapters the reconceptualization of the term 'population composition' is in itself a requirement for an adequate development of a two-sex demography.

However, the recent works produced by Schoen and Pollard contain explicit insights for a reconceptualization of the objective consistent with the view of a two-sex demography discussed in this thesis. Both authors have become persistent in defining the development of two-sex models in terms of what they call 'Interaction between the sexes' (Schoen, 1988; Pollard and Höhn, 1994); or as Pollard (1995) put it in one of his latest papers, 'Modelling the interaction between the sexes'.

After maintaining that the 'harmonic mean approach' has properties that make it a more adequate solution to the two-sex problem several other alternatives, Schoen draws the following conclusion:

> Experience has thus shown that axioms and empirical evidence are unable to contribute more than general guidance. In choosing a solution, there is no escaping the necessity for using theoretical and conceptual criteria. It is not enough to have a procedure that reconciles male and females rates. The method must also have a rationale that relates it to how and why marriage and fertility rates respond to changes in population composition (Schoen, 1988: 131).

This is a positive conclusion, mainly because Schoen concedes that realism is not something given once and for all, nor is it something that demographers can achieve independent of theory.

In turn, Pollard (1995: 25) remarked in his recent paper that ‘the modelling issues raised by the two-sex problem are likely to remain until such time as a realistic two-sex model gains wide acceptance’. The important issue, though, is the one discussed in this thesis: when, why and how should the interaction between the sexes matter to demography?

Although Pollard has never discussed the issue of two-sex modelling in this terms, on different occasions he has pointed out some directions which go beyond the orthodox desire of replacing the one-sex models altogether, or that the two-sex models should be developed only to improve results settled with sufficient precision by the one-sex approach. Even more significantly, Pollard proposed with Höhn in 1994:

> Whilst the one-sex approach provides reasonable answers to many demographic questions, there are important issues where account must be taken of the interaction between the sexes and the relative number of available partners. We instance two here: population projection by family status, which is so central to planning in social security, and the analysis of nuptiality. In these situations, a more sophisticated two-sex model is essential ... There are other less obvious applications. The extent to which members of a particular migrant group marry outside their own group, for example, is often used as an indicator of assimilation into the local population. Simple proportions are usually calculated, but these can be misleading in situations where, for example, there are few potential partners from the migrant's own group and few marriages take place. Actual marriages for the various groups should really be compared with expected numbers of marriages under a realistic two-sex model with parameters the same across groups (Pollard and Höhn. 1994: 204).

Bongaarts's model of proximate determinants: a model for fertility output

The 1970s and 1980s were remarkably productive for the study of determinants of fertility, but just for neuter and one-sex approaches. Several new research directions were developed; original analytical frameworks and working concepts have been proposed and tested empirically.

Following Davis and Blake's analytical framework the study of fertility determinants has been formulated in two interrelated but relatively different areas of research: on the one hand, the proximate determinants, in the perspective developed mainly by Bongaarts (1978, 1991, 1993a, 1993b; Bongaarts and Menken, 1983); on the other hand, the institutional determinants referring to the economic, socio-cultural, material, and political contextual factors which affect fertility behaviour (McNicoll, 1975, 1978, 1984, 1993b; Greenhalgh, 1990; Johansson, 1991).

Bongaarts's model is perhaps the most refined and elegant model for the study of the determinants of fertility seen as an output. For purposes of empirical implementation Bongaarts collapsed the list of eleven intermediate variables proposed by Davis and Blake into eight (Bongaarts, 1978), or more recently six (Bongaarts, 1993a: 11): (1)age at marriage; (2) duration of postpartum infecundability due to breastfeeding and abstinence; (3) frequency of intercourse; (4) age at onset of fertility; (5) intra-uterine mortality; (6) biological risk of conception failure. Bongaarts called the first three variables 'behavioural factors', and the other three 'biological factors'. But following Davis and Blake's analytical framework the model of 'proximate determinants of fertility' is aimed at modelling the three main stages determining that a woman has a live birth; these three stages are classified in three categories: intercourse variables, conception variables, and gestation variables.

The model of proximate determinants can be classified as a one-sex model, for both its conceptual and methodological conditions. Usually both Bongaarts and his followers or critics take for granted that the model of proximate determinants should be applied to the female component of the population. Authors are generally silent as to the assumptions for choosing the female population only; but one can conjecture that they have no intention of implying either that population is sexually promiscuous or that females are perfectly marriage dominant. Instead, the model of proximate determinants seems to be a valid and useful measure and describes the levels and trends of fertility outputs. Since the fertility output of a population is a function of and depends basically on the producing sex, one can accept that the model of proximate determinants is applied to the female sex only.

Unfortunately, there has been a misuse of the model of proximate determinants, particularly when it has been applied to study demographic issues which are the outcome of both sexes. To mention just two examples, I refer Bongaarts's (1991a) paper called 'Do reproductive intentions matter?', which has already been commented above; and Das and Padhiyar's (1991) paper called 'A model to study the socio-cultural determinants of fertility: an extension of Bongaart's model'.

Both papers aim to explain fertility change through a model that is adequate to measure and describe the proximate determinants of fertility outputs, but not to measure and explain fertility outcomes. Issues such as fertility preferences, desired fertility, ideal family size, and contraceptive use should be treated as single joint outcomes of both sexes, for the reasons already discussed. This does not mean that, for instance, the data from Demographic Health Surveys are completely useless. What it means is that, if investigators want to use the existing data to study issues for which not enough data were collected, the most they can do is to complement them with adequate and explicit additional assumptions.

The 'Procrustean bed' of reproduction: neuter intergenerational relations?

Ever since Becker's (1960, 1977) 'economic analysis of fertility' the neoclassical economic theory, and in particular the so-called Chicago School, have been part of the debate on the determinants of fertility. Becker has applied the individualistic consumer choice theory to a variety of issues relevant to demography, including the determinants of fertility and family size, the allocation of time, home economics, and intergenerational mobility (Becker, 1960, 1965, 1977).

Becker's basic framework can be and has already been criticized on several grounds (see, for instance, Arthur, 1982b; Daly, 1982; Folbre, 1984, 1986a, 1988). From the point of view of the conceptual theory developed in this thesis Becker's theory is basically a neuter theory, and thus shares the merits and the shortcomings of any neuter theory. Among his work, perhaps the most revealing of how far a neuter theorization can go is the paper published by Becker and Tomes (1979), 'An equilibrium theory of the distribution of income and intergenerational mobility'.

This is basically a model of individual utility maximization, in which the individual is replaced by family. Also the utility of a parent is assumed to depend not only on his own consumption, but also on the number of his children and the various characteristics of each child. The core assumption of the model is

> that children have the same utility function as their parents and are produced without mating, or asexually. A given family then maintains its identity indefinitely, and its fortunes can be followed over as many generations as desired. Asexual reproduction could be replaced without any effect on the analysis by perfect assortative mating: each person, in effect, then mates with his own image (Becker and Tomes, 1979: 1161).

Following these assumptions Becker and Tomes discussed the intergenerational decision-making of the family in rather peculiar way, which motivated a biting critique by Daly (1982: 307-312). Daly started by questioning the usefulness and even meaning of the lengthy algebraic exercise throughout 25 pages for the results produced. 'Given the exceptionally heroic nature of the assumptions', Daly writes, 'one would expect a rather heroic, or at least interesting, conclusion'. However, Daly pointed out that the main conclusion of the article is rigorously self-evident: 'We have shown that the family is more important when the degree of inheritability and the propensity to invest [in children] are larger' (cited by Daly, 1982: 308).

Daly took pains to discuss the paper because 'it reveals just how far some members of the Chicago School will go in amputating those limbs of human society that do not fit the Procrustean bed of individualistic utility maximization'. First, Daly questioned the substitution of family for individual as a way of extending the model to an intergenerational time frame. Secondly, Daly dismissed Becker and Tomes's claim that the assumption of asexual reproduction can be replaced by perfect assortative mating without undermining the individualistic basis of the model.

Daly's metaphor, the 'Procrustean bed', is telling and strikes at the root of the theoretical principle of individualism of the Chicago School. Time and again demographers, like economists, also behave like the ancient brigand who stretched or mutilated his victims to make them conform to the length of his bed. Often, one cannot have Daly's luck to have the critique so facilitated. In demography, scholars would not dare to set out explicitly the assumption that the individuals constituting a population reproduce asexually. But what seems more interesting in Daly's critique, for the comparison of what in this thesis I have called neuter, one-sex and two-sex frameworks, concerns the issue of when it is adequate to make counterintuitive assumptions.

> Becker and Tomes are driven to the incredible extreme of assuming that people reproduce asexually, like sponges! The reader is encouraged to believe that this assumption is for analytical convenience only and that nothing important hinges on it, because it supposedly can be substituted by the alternative and less absurd assumption of perfect assortative mating, without affecting the analysis. But, as we

> shall see, this is not the case. The analysis is strongly affected because the definition of the basic decision-making unit must be changed (Daly, 1982: 310).

Daly demonstrates that Becker and Tomes needed to explicate their assumption of asexual reproduction because the subject of intergenerational decision-making should take the discussion beyond individualism towards the level of community. However, this is not the thrust of the Chicago School, and so Becker and Tomes found it necessary to state the assumption of asexual reproduction explicitly, as a way to keep the world safe for the principle of individualistic utility maximization.

In addition to that, Daly continued, Becker and Tomes needed to soften and obfuscate their abstraction from the fundamental issue, namely that in the matter of intergenerational relations one must go beyond individualism and recognize communities or at least social class. Therefore, besides the assumption that asexual reproduction is for analytical convenience, the authors also argue that such an assumption is not really necessary; the evidence is that it could be replaced by the assumption of perfect assortative mating. However, if this second assumption is so obviously equivalent,

> why not make it from the outset and base the analysis on it rather than just appealing to it in the last act as a vague *deus ex machina*?. The answer is that perfect assortative mating actually implies the abandonment of the individualistic model and substitution of the social class as the intergenerational decision-making unit, thereby making irrelevant all theorems and 'results' derived with the assumption of the temporarily extended individual ('family') as the decision-making unit. If one wants a world of individualistic maximization on the intergenerational time scale (Chicago School), then one cannot have sexual reproduction. If one opts for sexual reproduction (and how can we do otherwise?), then one must go beyond individualism and recognize community or at least social class as fundamental in matters intergenerational. Becker and Tomes cannot have it both ways (Daly, 1982: 311).

When demographers use the neuter approach, for instance in case of classical demographic transition theory, they do not concede having assumed that population is asexually reproduced. But if demographers draws picture of population growth and demographic change on the basis of crude rates they should not entretain the thought that such a picture is sufficient to conclude anything about the underlying relationship of sexual reproduction.

The supply-demand framework for the determinants of fertility

Easterlin (1969, 1975, 1978; Easterlin and Crimmins, 1985) followed the neoclassical economic perspective on fertility introduced by Becker. Besides attempting to bridge economics with sociology, Easterlin also took into consideration the model of proximate determinants.

> For one thing, it is a notably sexless subject. This is not a mere quibble, for this omission has logical consequences for the theory. Without reference to sexual

> intercourse one is hard put to explain why households would engage in the 'production' of children once the desired number is reached, and consequently why excess fertility would ever occur (Easterlin, 1969:136). Similarly, there is a notable scarcity in the economic analysis of fertility of references to physiological or biological factors that may influence fertility. There is also inadequate consideration of the nature of and evidence on the ways in which fertility is actually regulated(Easterlin, 1978:59).

While Easterlin followed the basic theory of consumer choice, he proposed a way to transform a purely neuter analytical framework into a one-sex model. On this basis, the demand for children is said to be based on the household's balancing of its subjective tastes against externally determined constraints of price and income in a way that maximizes its satisfaction (Easterlin, 1978: 60-61). But now, if anyone will have to be altruistic, as Becker proposed in his *Treatise on the Family* (1991), it is not man but woman. Demography has never fixed its ideas as to any sort of self-interested *demographic woman*, though this term seems more appropriate for a model of *economic man* applied to the component of female population only.

Without moving into much detail with regard to the supply-demand framework developed by Easterlin for the determinants of fertility, it seems important just to draw attention to the way the basic operational definitions have been determined by the one-sex approach. For instance, supply of children has been defined as 'The potential output of children, C_n, the number of surviving children parents would have if they did not deliberately limit fertility' (Easterlin, 1975:55). This is a rather interesting definition of supply, as far as economics is concerned; with regard to economic markets it is unthinkable to imagine a definition of supply of goods or services as the number of surviving commodities a producer would have if his productive capacity were not deliberately limited.

The limitations of the notion of demographic supply are, at least in part, acknowledged by Bongaarts and Menken (1983):

> The supply of children can be directly measured only where demand factors have very little effect on fertility. The usual indication that fertility is governed by supply variables is provided by the absence of deliberate fertility control by contraception or induced abortion. In societies where many couples resort to contraception, the supply of children is not directly observable; however, its estimation may be possible through the application of reproductive models. ... Thus, an unbiased estimate of supply for the entire population can be obtained only when fertility regulation is absent (Bongaarts and Menken, 1983:27, 52).

In turn, the definition of demand for children has been defined as 'the number of surviving children parents would want if fertility regulation were costless' (Easterlin, 1975: 55). Usually, couple’s desired fertility is represented by women only. So while supply has been associated with potential output, demand is defined in terms of desired fertility.

In 1993 Bongaarts proposed some modifications to Easterlin's model with the objective of simplifying its application by changing some key features, while maintaining the original conceptual structure largely intact. In this case, reproductive performance as well as supply and demand are measured in terms of births rather than surviving children; the model became period rather than cohort-based; and there is a new variable, 'the degree of preference implementation', which quantifies the roles of the costs of fertility regulation and unwanted childbearing.

Curiously, despite the fact that Easterlin's model or Bongaarts's alternative share with Becker's model the same economic theory, they have recently been placed into conflict. Pritchett's (1994a) paper called 'Desired fertility and the impact of population policies' has raised, as Bongaarts (1995: 4) admitted, 'one of the most vigorous attacks ever mounted on [the] scientific underpinnings' of the family planning movement. Pritchett demonstrated with numbers what several authors had for some time put in words, and concluded: 'it is fertility desires and *not* contraceptive access that matter' (Pritchett, 1994a: 39; see also Bongaarts, 1994, 1995; Knowles, Akin, and Guilkey, 1994; Pritchett, 1994b).

This recent controversy involves problems that go beyond the validity of the scientific evidence that family planners use to justify their movement, which is far from surprising. As any social movement, family planning programs often depend less on scientific evidence than well articulated politics. While the former may help to find an abili for any of the positions in the controversy, the latter depend mainly on political and moral views concerning the control of population, whether past experiences have effectively influence fertility, and the quality of the services provided by specific family planning programs.

The 'Procrustean bed' of the one-sex intergenerational fertility theories

In 1976 Caldwell proposed a restatement of demographic transition theory and outlined his approach on the 'intergenerational wealth flows'.[7] While this approach stresses the flows between parents and children, with regard to gender it lies squarely in the mainstream of the swing view of gender discussed in Chapter 8.

> All fertility transitions are generated by two factors: the primary one of the decreasing economic advantage (or increasing disadvantage) of having children, and the secondary one of women's increased ability to determine their own fertility (Caldwell, 1983: 470).

This statement, seen either as a conclusion or a simple conjecture, does not seem to have enough theoretical support and adequate empirical evidence. On the one hand, from a conceptual point of view Caldwell's approach is meant to explain rather than just describe the magnitude and directions of fertility decline; but on the other hand, the model of intergenerational wealth flows has never developed to a stage in which

[7] The same may be applied to Handwerker's variant of the intergenerational fertility theory.

demographers would be able to formulate clearly designated and reliable measures of fertility outcomes. An approach to 'wealth flows' as part of a gender-generational setting based on a two-sex methodology awaits adequate elaboration.

The decade of the 1980s was marked by widespread complaints but few attempts to overcome the ‘widely agreed’ lack of ‘an adequate *theory* of fertility’, as McNicoll put it in his very much cited remark made in 1980. Less cited, but probably even more important, is McNicoll’s inference as to the negative implications of the theoretical limitations in the study of fertility determinants:

> It is an anomalous situation, the more so in that quite important allocative decisions may be influenced by considerations of 'population policy', considerations in turn based on statistical linkages between fertility and other variables whose theoretical interpretation is by no means clear (McNicoll, 1980: 441).

In the following year, the Population Council (1981: 311) reviewed the existing research on the determinants of fertility and priorities calling 'attention to several conspicuous deficiencies in the cumulative record of the past - most notable, perhaps, being inadequacies in theoretical grounding'.

Yet throughout the 1980s demographers have found no reason for any particular satisfaction with the state of fertility theorizing. In 1985, a group of prominent demographers discussed the 'Insights from the World Fertility Survey' and accepted, among other aspects: 'that the WFS surveys are superficial and very limited in explanatory power' (Scott and Chidambaram, 1985: 21); that the WFS surveys can 'be described as a "world fertility intermediate variables survey" ... [and] there is the feeling that some of the causes of fertility decline have been explained rather than the mechanisms of the decline' (Caldwell, 1985a: 45). At the beginning of the 1990s Greenhalgh wrote:

> Three decades ago, there was wide consensus on why fertility falls. Now, however, it seems that the closer we get to understanding specific fertility declines, the further we move from a general theory of fertility transition (Greenhalgh, 1990: 85).

As I have demonstrated in this thesis, the growing dissatisfaction with the state of demographic theorizing concerns its explanatory ability and tools, rather than the well-established theoretical, mathematical and empirical areas of descriptive demography. In this respect, I have also questioned the anxiety about an adequate fertility theory when demographers have hardly discussed how to deal with the two-sex causal mechanisms that underlie fertility change and many other demographic phenomena. Some months ago Caldwell insisted on one of his most remarkable challenges to contemporary demographers:

> The fertility transition has a claim to be the most important happening of our time. It has changed the nature of the family and the relationships between old and young as well as guaranteeing that the advances against mortality can be maintained. It has altered whole societies. Research in this area can be, as I have found, fulfilling over a lifetime. Research on the origins and nature of this phenomenon is of profound intellectual interest (Caldwell, 1995: 1).

For many social scientists in the neighbourhood of demography, particularly those used to regard fertility as a passive and dependent of other factors, the idea that fertility transition has a claim to be the most important happening of our time, may read as an exaggeration. However, demographers do not make things easier if they regard themselves as scholars who are basically testing hypotheses produced elsewhere. Moreover, the chapters comprising Part II of this thesis have identified the limitations but also the important strengths of the two principal analytical frameworks in which current fertility theories stand. Caldwell's confidence about the origin and nature of fertility transition can find much support on the existing neuter and one-sex descriptions of fertility, but the core of his explanatory conjectures needs to survive the scrutiny of a two-sex consideration.

The two-sex demography has the potential to handle the essential set of contingencies on which fertility events and relations depend in ways never experimented in demography. Caldwell's statement quoted above touches on the central concern of the twentieth-century of demographic science, that is, fertility transition; but the very term 'fertility transition' is itself the product of an important shift from the classical demographic transition based on a neuter approach to its modern restatement standing mostly on a one-sex theorizing. What we need, though, is a two-sex fertility transition theory. Can demographers accommodate it?

By the evidence of this thesis, demographers can no longer entertain the thought that conventional theoretical and empirical areas of demography lack 'any particular conceptual strait-jacket', as Cleland and Wilson (1987: 10) asserted in reference to the European Fertility Project (EFP) and the World Fertility Survey (WFS). Cleland and Wilson (1987) seem correct in asserting that the generality of WFS data is impressive. However, the generality of demographic data is far from absolute and neutral. Rather, the generality and usefulness of WFS data need to be understood in the context of the conceptual and methodological frameworks that fit in the distinction between descriptive and explanatory demographic purposes. The EFP, WFS and also DHS surveys provide data which enable the same measures of fertility 'to be compared for a wide variety of countries and regions with greatly differing social, economic and cultural characteristics' (Cleland and Wilson, 1987: 10). Yet such comparability is in terms of demographic measures of fertility output, and this alone is not enough for assessing theories which aspire to explain fertility as a demographic outcome.

To sum up the overview of important aspects for the argument of a two-sex demography, it seems useful to draw attention to at least one Ph.D thesis, just as I did above with regard to the four Ph.D theses relevant to the 'two-sex problem' (Karmel, 1948c; McFarland, 1971; Feeney, 1972; Wijewickrema, 1980).

The Ph.D thesis by Malhotra (1990) called 'Gender, intergenerational relations and marriage patterns in Indonesia' identified nuptiality as a key component of the demographic transition, as well as an integral part of changing family structure.

Moreover, Malhotra's thesis sketched its conceptual model on the basis of a sociological and anthropological literature concerned with family, generational relations, and kinship and gender stratification.

However, despite the fact that Malhotra's thesis deals with issues concerning decision-making of both sexes and male-female nuptiality, it simply fails to even acknowledge the existence of the two-sex modelling in the debate on nuptiality. Only this oversight can explain Malhotra's (1990: 1) believe that 'demographic research is beginning to focus on marriage behavior beyond its importance for fertility'. I have already exposed this misconception, particularly when I demonstrated in Chapter 7 that as far back as 1917 Knibbs proposed a conceptual framework to study both fertility and demographic reproduction within the context of nuptiality. More recently, other aspects have been developed as 'dependent variables', to use Malhotra's (1990: 2) expression. For instance, the issue of 'marriage squeeze' is not immediately concerned with fertility but the effects on marriage behaviour produced by an imbalance in the number of males and females. Likewise, the issue of 'marriage market' has been developed to reconcile and optimize the marriage demands of both sexes.

Three important features concerning the definition of two-sex demography discussed above should be stressed with regard to Malhotra's thesis. First, its conceptual theory recognizes the significance of the two categories (gender and generation) here considered indispensable for any adequate two-sex approach. However, Malhotra did not try hard enough to overcome the *ad hoc* adjustments gender and generational concepts to the conventional neuter and one-sex theories. Secondly, if Malhotra accepts that conceptual theory and then research hypotheses direct and guide investigators, the data used and the type of analysis conducted in order to measure a relationships between the major independent and dependent variables are not as independent of theory as she has assumed (Fisher et al., 1983: 2).[8] For instance, in the last paragraph of her thesis Malhotra (1990: 222) wrote:

> Our biggest limitation has been the sample design, which in addition to being cumbersome, and containing only ever-married individuals, has continually left the trends regarding husbands more susceptible to selectivity biases than is the case for wives ... Better contextual data would not only make interpretations easier, but it would also make the theoretical propositions more specific and meaningful (Malhotra, 1990: 222).

This statement undermines Malhotra's own remarks on the relevance of her findings to the larger theoretical concerns that motivated her own research. But most importantly, Malhotra concluded her doctoral thesis with a misleading inversion of the logic of scientific discovery. Indeed, even her own thesis she basically started by identifying the problem in the context of existing theories, and other after that moved to further analysis on the basis of some specific methods and sources of data. Thus,

[8] Fisher et al. (1983) *Handbook for Family Planning Operations Research Design* is an interesting example on how the one-sex view has become thoroughly internalized in studies on family planning.

Malhotra’s final remarks simply show how the ghost of empiricism appears when one least expects it. However, what can contextualize demographic data better than more meaningful concepts, interpretations, and theoretical framework? If sample designs are not independent of theory, are the limitations of data only dependent on sample designs and questionnaires?

Thirdly, Malhotra's thesis identifies two aspects of nuptiality as dependent variables, the timing of marriage and freedom in spouse choice; these two variables are also assumed to be interdependent, in that a change in a marriage timing may be associated with a change in spouse choice, and vice versa (Malhotra, 1990: 17). Despite that, the two dependent variables are seldom dealt with as measures of outcomes combining the probabilistic results of both sexes with the objective to measure relationships strength between husbands and wives. But this point goes back to the weak level of theoretical inquiry that should anticipate the analysis of data and test of hypotheses.

In short, past distinctions between descriptive and explanatory demographic analyses have been more in terms of degree rather than in essence. The shifts introduced recently in the debate on the determinants of fertility have moved from one factor to others, but in the end they have left the nature of their problem untouched. Some propose to move from economic causation to anthropological, cultural or ideational determinants. Others have abandoned Marxist and other ideological approaches on population reproduction to propose, for instance, a feminist demography of reproduction. Still others consider that population problems are more practical than theoretical; what is needed is a transformation of the birth control 'industry' into a movement towards women's empowerment in reproductive health. In any case, despite all these shifts the fields of research known as 'two-sex problem' and 'determinants of fertility' have developed as if their own strengths were irrelevant for each other.

Unmet need for family planning: an evidence for the two-sex perspective

Regardless of whether family planning programs are justified in terms of demographic purposes and targets only, or in terms of a broader cause associated with the improvement of women's' health and position in society, the debate concerning their scientific underpinnings has barely started.

The idea of studying fertility preferences and desires has concerned demographers for the past four to five decades. According to Bhushan and Hull (1995) the term 'unwanted fertility' appeared for the first time in the 1960s, in the context of the debate about the role of subsidized family planning in a national population policy in the United States. The same happened with the concept of unmet need for contraception, which originated with the so-called KAP studies, or studies about Knowledge, Attitudes and Practices of birth control. However, the widespread collection data on fertility preferences occurred in the context of the two major international surveys carried out in the developing countries; first the World Fertility Survey (WFS) in the late 1970s and early 1980s; and then the Demographic and Health Survey (DHS) carried out since the late 1980s.

The shortcomings and improvement of fertility preferences measured on the basis of concepts such as ideal family size, unwanted fertility, unmet need, and KAP-gap have been discussed and improved among demographers (Bongaarts, 1991b; Bhusham and Hill, 1995; Westoff, 1988). But just as with fertility theory in general, the measurement and interpretation of desired fertility have generally left the one-sex conceptual and methods untouched. However, if the envisaged two-sex demography is not a flight of fancy it is doubtful that a controversy, such as the one between Pritchett and Bongaarts mentioned above, can be solved on the basis of the theoretical and methodological tools these authors have so far used. The reason is that bilateral or multilateral statistical correlations, in disregard to the neuter and one-sex frameworks that underlie them, may allow to conjure up a wide range of demographic scenarios. However, none of such possible scenarios can be said to represent the real demographic world of population, unless the demand for contraception is treated as the outcome of both sexes.

Fortunately, while this thesis was under examination another empirical piece of evidence in support of the argument for a two-sex demography emerged. Dodoo and Landewijk (1996) published a paper called 'Men, women and the fertility question in Sub-Saharan Africa: an example from Ghana' which contains a compelling empirical evidence for the argument of two-sex demography.

> This paper argues that the extent to which family planning will be used in sub-Saharan Africa lies somewhere between the estimates of the unmet need of women, and those provided by a measure of couples' need, where the latter includes the preferences of both partners. Simply put, the demand for contraception gleaned from female-only responses overstates the true or actual demand for family planning in a context where male fertility preferences are higher than those of females (Caldwell, 1983; Mott and Mott, 1985; Fapohunda and Todaro, 1988; Dodoo and Seal, 1994) (Dadoo and Landewijk, 1996: 30-31).

The Ghana Demographic and Health Survey (DHS) provides a fortunate but very isolated departure from orthodoxy in the way DHS studies were implemented; it is one of the few cases in which both men and women were interviewed. In spite of this, as Dadoo and Landewijk lamented,

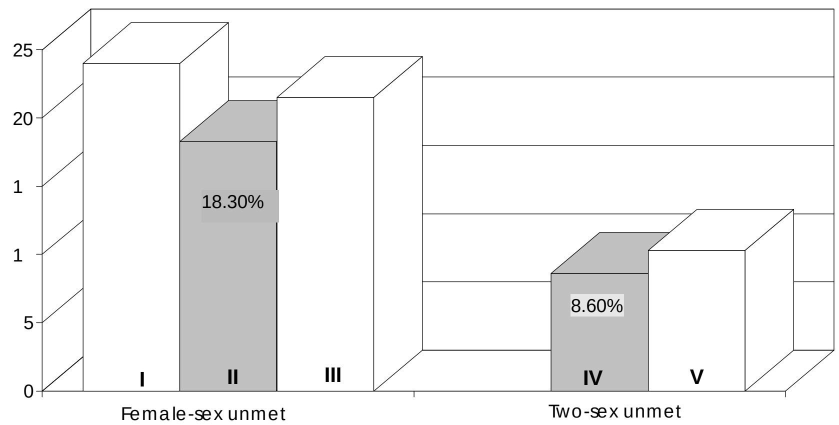
> none of the more sophisticated measures of unmet need (Bongaarts 1991; Westoff, 1988) can be utilized in this comparison of men and women's unmet need. This is precisely symptomatic of the focus of the entire population endeavor on women; the earlier measures are fine-tuned to correct for details such as current sexual activity or abstinence. The surveys, which have only recently begun to collect data from men, lack this detail on the male side. Thus, comparisons between men and women can only be made at a broader level, and we seek to do this without losing sight of the underlying concept of unmet need which captures individual preferences to

continue reproduction, against their current use status of contraception. For this reason, the most basic measures found in the literature, the instantaneous KAP-gap, is employed, because this variant of the concept easily accommodates the sparser data collected from women. It is important to note that this choice does not depreciate the validity of the findings, particularly because any findings from the instantaneous measure probably understate the level of contraceptive need provided by the more sophisticated models (Dodoo and Landewijk, 1996: 33-34).

Based on a survey of 1,010 couples interviewed in Ghana DHS, Dodoo and Landewijk compared the estimated unmet need by taking into consideration women only and then both sexes. As Figure 2.11.3 shows, 24 percent of the women in the survey want no more children. However, this figure drops to 18.3 percent if one considers the conventional KAP-gap, that is women who want no more children but are not currently practicing contraception.

In turn, the results become substantially different when the unmet need is treated as the outcome of both sexes; as Figure 2.11.3 illustrates the two-sex unmet need reduces by more than one-half, from 18.3 percent to 8.6 percent. This is a compelling empirical evidence for the argument of a two-sex demography. for it shows how one-sex methods when incorrectly applied to demographic phenomena of a two-sex nature can lead to misleading results. Just as the crude vital rates mislead because they do not capture population structure. So demographers have to come to terms with this new type of crudeness; in the case of the example presented here, a crude 'overstatement of the potential need for contraception'.

Figure 2.11. 3: Unmet need: one-sex versus two-sex measures



Categories refer to:

I Women wanting no more children

II Women wanting no more but not using any modern form of contraception

III Women wanting no more children but not using modern contraception

IV Both partners waiting no more children but not using any form of contraception

V Both partners wanting no more children but not using modern contraception

Source: Dodoo and Landewijk, 1996: 35

Unfortunately, Dodoo and Landewijk (1996: 34) made this overstatement conditional and dependent on '**if** men are the primary reproductive decision makers, or male approval is at least required for women's contraceptive adoption'. This 'if' is just one among many 'ifs' that constitutes the subject of the two-sex demography. Dodoo and Landewijk's hesitation reflects the unfavourable circumstances in which the two-sex approach has so far developed and, above all, its weak level of theoretical basis. Certainly that Dodoo and Landewijk will have no difficulty to admit that the problem with the conventional one-sex measures and methods conceptual and goes beyond any isolated empirical example.

Epilogue: towards a two-sex demographic transition theory

In a certain way, this thesis is finishing just when I was getting ready to start it as I wished at the beginning of my Ph.D research project. But at this stage it is hard to imagine how things could have been any different when this work has started. Looking lack, three years ago I was simply unable to even formulate the simple but central question that constitutes the subtitle of this thesis. This is apparent when I now browse earlier drafts of this thesis and, in particular the research proposal I discussed at my department in May 1993.

Although I identified the research problem for this thesis quite earlier,[9] at the initial stage I certainly did not expect that the specific definition of the problem would turn into the extensive discussion presented in this thesis. In spite of that, I continue to believe that some of my firsts attempts to outline a two-sex linear general model for an analysis of fertility determinants contain the best imaginative ideas in the whole work of my doctoral project. However, I decided to put such ideas temporarilly aside once it

[9] The following questions have been drawn from the Research Proposal I presented in May 1993:

> Have demographers ever thought that they might have overlooked two simple but very important universal demographic asymmetries from their analyses of the determinants of fertility? Are we really convinced that the fact that men cannot give birth and breastfeed children is demographically irrelevant and scientifically trivial? Under what conditions is it legitimate to gather fertility information from women of childbearing age only? Why have demographers failed to apply to fertility analysis the basic mathematical coordinate system, and what has been wrong with the efforts of those economists who attempted to apply it within the so-called economic approach of fertility? Why is it now time for shifting frames of reference in demographic analysis? How relevant is feminist gender theory to the analysis of the determinants of fertility? Is the neglect of gender issues by demographers justifiable and, conversely, is the feminist pressure to foster an awareness of their importance a senseless crusade? Or, on the other hand, is the concept of gender too important to be wasted on political agendas rather than being the focus of a more systematic and comprehensive population studies?

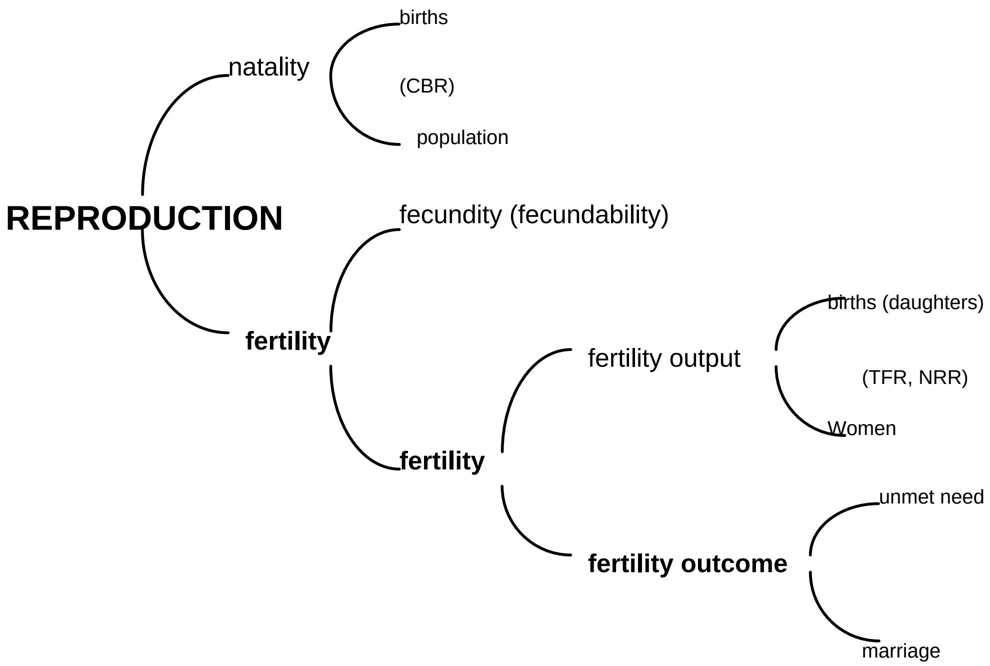
became clear, as is stressed in the Dedication, that I had not so much lack of data but, above all, lack of adequate theory.

I have little doubt that many pithy examples such as the one provided in the previous section would be worth more than most of the theoretical disquisition offered in this thesis. The problem, though, is that the confidence on a two-sex demography depicted in this thesis has not blossomed from any sudden and mysterious inspiration. Rather, it is the product of a careful search for conceptual and methodological strands in demographic theory from which one can hope to weave a two-sex approach. In this context, a convincing argumentation for a two-sex demography became indispensable and first priority.

What can this thesis suggest for future research and for policy?

With regard to research, this thesis has delineated which aspects in demographic phenomena are determined by the complementarity between the sexes and thus are essentially of a 'two-sex' nature. In so doing, the thesis also demonstrated that the overwhelming majority of demographic analysis and research has fallen into either a 'neuter' or a 'one-sex' category. But rather the latter theoretical and methodological anomalies, the thesis shows the scope of their validity and shortcomings. In particular, the thesis defends that neuter and one-sex approaches are most powerful when used to describe, estimate and predict demographic characteristics, such as the size, levels and patterns of population. However, whenever one needs to understand the causal mechanisms which underlie the description of what has happened to a population, neither sex can be considered eligible to represent, theoretically and statistically, the whole population; in other words, neither sex should be used independently to explain, for instance, why and how fertility rises and falls over time. At this stage, an important conceptual distinction is needed between *demographic outputs* and *demographic outcomes*. While all demographic activities have demographic outcomes, not all outcomes result in demographic outputs. Figure 2.11.4 summarizes this approach discussed in Chapter 6; the difference with regard to Figure 1.6.1 is that in this last picture fertility is situated in the broader context of demographic reproduction.

Figure 2.11.4. Situating fertility in demographic reproduction



Rather than lamenting the underdevelopment of demographic science (see Scott and Chidambaram, 1985: 20), I have attempted to demonstrate that demography is not only ‘long’ on methods but, more importantly, endless on ideas. In this regard, further research aiming at two-sex empirical research designs should always start from a firm theoretical foundation.

Chapter 11 has discussed the strengths and limitations of relatively recent conceptual and methodological frameworks that seem paramount for any attempt to transform a two-sex conceptual theory into two-sex operational research designs. However, much work remains to be done in order to reconcile, for instance, the features in the fields of 'two-sex problem' and the 'determinants of fertility' that best meet the definitional conditions for a coherent two-sex demography discussed above.

Many other research tasks could be enumerated here, but there is one that holds a promising challenge for future work. Can demographers really accommodate the idea of a two-sex fertility transition theory?

Members of the anti-demographic school who believe that 'The demographic transition model [is] a ghost story', as Abernethy (1995: 3) put it recently, might use the idea of a two-sex demographic transition as a symbol of a non-stopping resistance of some demographers to accept that their field is badly in need of assistance from elsewhere. However, from the evidence of this thesis demographers should look more carefully to the disciplinary history of demography's best tradition before trying to outline any new throe. Moreover, demographers seem to have no better alternative but answer positively to the question concerning a two-sex demographic transition theory.

What has come to be known as the classical theory of demographic transition entails various interpretations and hypotheses framed within the neuter conceptual and methodological framework. The factors often considered to have caused demographic transition (i.e. modernization, urbanization, industrialization, and Westernization) are as crude and neuter as the demographic measures used to depict the transition in classical terms (i.e. crude vital rates and population growth).

By taking the neuter aspects of demographic transition theory for granted demographers managed to conjure up virtually any determinant. However, as Chapter 9 shows the distress and disappointment that the classical demographic transition from high to low vital rates is said to have caused among population analysts is unfounded. This is so because demographers could have expected that since the classical transition theory draws on crude vital rates, its prediction are likely to lead to very misleading results; especially if used for comparing different populations, or the same population in different periods.

Among many other studies, this is what the European Fertility Project revealed when demonstrated that contrary to many cases, mortality decline did not precede fertility decline. The failure to grasp and acknowledge the neuter nature of classical demographic transition has nourished the claim that demographers have in the past overlooked 'culture' but it has become inevitable and 'natural that demography should look to anthropology for assistance' (Kreager, 1980: 237).

In the past three decades the neuter demographic transition theory has undergone its own 'transition' from a neuter to a one-sex theory; the term 'fertility transition' can be interpreted in line with such a development. Fertility, as it is perceived in the context of fertility transition theory draws on the concept of fertility output and the one-sex models of fertility and marriage. Most of the attempts to develop fertility theories, from Caldwell's intergenerational wealth-flows theory to Mason's hypotheses about the link between 'female status' and fertility, can be considered two-sex theories in a state of chronic denial of the one-sex frameworks that they stand on. The same can be said about recent attempts to 'create a different kind of demography', as Greenhalgh aspired in a book she edited in 1995 called *Situating Fertility*.

Just as the various versions of neuter transition theory are likely to lead to misleading and unexpected results, the same can be said with the various versions of one-sex transition, such as the 'independence' perspective studied and exposed, for instance, by Oppenheimen (1988, 1994, 1995). And again, like with the neuter versions of demographic transition, if demographers alter their interpretations of fertility transition without taking into consideration the limitations of their one-sex setting, they can conjecture up virtually any explanation of fertility transition. This is what has in recent decades happened. Demographers and other population analysts have moved from socio-economic to cultural and ideational; from Marxist to feminist theories; from institutionalism and functionalism to anthropological theories of meaning, semantic and symbolism.

Whether the same will happen to whatever versions of a two-sex demographic transition theory might be proposed in the future remains to be seen. But one thing seems certain. The argument for a two-sex demography should change the terms of debate about what the fundamental problems are, and how to formulate them without reducing it into a question of scale or level of analysis.

In terms of policy, first of all policy-makers and funding agencies need to be made aware that however useful descriptive demographic information is, this information provides little guidance as to how fertility should be influenced. Caldwell pointed out in 1985 with regard to the WFS surveys:

> In an authorized report on the programme, it was emphasized that "the principal objective of the project was to provide information which would be of value for those policy-makers who aim to change fertility (Caldwell, 1985a: 45).

Yet this goal is at odds with the immediate objectives discussed in the same occasion by Scott and Chidambaram (1985: 20):

> Only a broadly descriptive survey could hope to gain support as a *world* fertility survey, destined to consume a substantial fraction of all the funds available for social research on Third World fertility over a decade (Scott and Chidambaram, 1985: 20).

While demographers should despair if only descriptive surveys hope to gain support as *world* fertility surveys, policy-makers and funding agencies should wonder whether empirical research designs standing on *ad hoc* theories can lead to adequate population policies.

In view of the envisaged two-sex demography outlined in the present work, it seems clear that demographers can rest assured they do not need to entertain the view that a phenomenon does not exist if they can find no satisfactory way to measure it. Demographic knowledge is becoming both broader and deeper, and any change in line with the best tradition of demography should improve its role as a social science. But much more has been advocated here which should lead to startling changes. First, changes with regard to the scope of validity of conventional neuter and one-sex concepts and methods. Secondly, changes related to what demographers consider to exist in population reality beyond whatever they can already observe and measure. And finally, changes concerning the main purpose of demography; as a social science, the most valuable purpose of demography is to reveal and explain demographic reality; not just the reality that people can experience and observe directly, but whatever constitutes and underlies the fabric of population reality.

Appendix A: Extracts from 'Graunt's Observations: a model of demography's whole design'

(A new reading on the first and most influential
book ever written in demography)

The material in this Appendix A have been drawn from a paper submitted for the *1994 W. D. Borrie Essay Prize* in March 1995, but carried out in the context of this Ph.D thesis. Preliminary ideas expanded in this paper can also be found in a paper presented at the Seventh National Conference of The Australian Population Association (Francisco, 1994c). .

The full paper provides a new reading on Graunt's *Observations*, surely the most influential book ever written in demography. Five interconnected but relatively distinct scientific features are discussed about Graunt's book: its main subject-matter and significant epistemological issues; its fundamental methodological basis and specific methods of inquiry; the scientific institutional context in which the book emerged; its language of communication; and the utility of its results and conclusions. Preceding the debate of these five features is a critical review of the current portraits of Graunt's contribution to modern demography.

Appendix A includes only the introduction of the paper and some figures, boxes and tables

1. Introduction

A new reading on the most influential book ever written in demography

At the heart of this essay is a paradox. The deeper we dig into the content and structure as well as the social and intellectual context of Graunt's *Natural and Political Observations made upon the Bills of Mortality* (1662), the more the established historiography on earlier demography turns into a creaking and ugly edifice. This is an unfortunate finding, for two main reasons. Certainly, current demographic textbooks usually make one or two references to Graunt's work. But disturbingly, after more than three centuries demographers have not yet come to terms with the full dimensions of Graunt's contribution for the making of demographic science.

Contemporary demographic historiography usually reduces the importance of Graunt's book (*Observations* hereafter) to the utility of its findings and conclusions; following its first edition in 1662, time and again some authors have called attention for an even more important feature in Graunt's book: the originality and power of Graunt's new method of inquiry. But as this paper demonstrates, overall demographers' failure to recognize the long-lasting influence of Graunt's contribution to modern demography seem to be associated with their failure to accept the *Observations* as the paragon for demography's whole design, and in particular for the bulk of its content.

The bulk of demography can be called 'descriptive' as opposed to what I call elsewhere an 'explanatory demography' (Francisco, 1996). It is more than a coincidence that the distinctions between description and analysis, or description and explanation, are frequently drawn are the concern of both statisticians as well as demography. It is true that the distinction among the cognitive content entailed by such concepts like description, analysis, and explanation is somewhat blurred; but after all, the historians of statistics seem to have come to an agreement that the descriptive statistical analysis of numerical data on population 'occurred first in 1662 when John Graunt analyzed the weekly reports on vital statistics for London, which had been published regularly since 1604' (Hald, 1990: 82).

Yet from a demographic point of view today, just as for Graunt and the philosopher to whom he associated his own work, 'the end rules the method' (Bacon, 1875: 254). It is not the purpose of this essay to discuss directly the issue which have worried demographers over many times: is demography a substantive rather than a mere application of statistics? Nor will I consider any further the need to go beyond the scope of statistics as far as the distinction between the two types of demographic analysis I discussed in my Ph.D thesis. In any case, the content of this paper has been elaborated as a background for such a debate; thus, the alternative reading of Graunt's contribution to the debut of demography provided here may at least offer an indirect contribution to demonstrate how important the *Observations* has become to the making of descriptive demography.

Briefly speaking, descriptive demography can be defined as the body of demographic analysis directed at the systematic collection of information with the objective to describe population characteristics, primarily with respect to the levels and trends of its size and structure. On this perspective, it may be advanced that the style of work of the average demographer today seems rather more Grauntian than, say, Malthusian.

This assertion may be found, at this stage of the paper, strange for two main reasons. First, demographers have learned from conventional demographic historiography that it is well acceptable, as Kreager (1991: 207) criticized, 'to lump together all writings before 1800 as 'pre-Malthusian' and considered them chiefly for their anticipation of Malthus or of the controversies he aroused'. This a rather misleading reconstruction of earlier development of demography, in part for the reason mentioned by Kreager: that early population inquiry is judge in terms of later developments of which past writers could not have been aware.

The second, and perhaps the most fundamental reason, refers to the very odd picture that demographers have of themselves as social scientists. Often demographers regard themselves as scholars who are testing hypotheses produced elsewhere; this position is nourished by the misconception that they can study what the reality of population is independent of theory. On these grounds Graunt's work has not been regarded theoretical: either because 'description is not knowledge', as Wunsch (1984: 3) put it, or because apparently Graunt did not set his research aiming at testing explicit hypotheses related to any underlying theory.

Graunt's remark, in one of his two dedications, that 'the whole Pamphlet, not two hours reading' has shown to be only partially true, if not somewhat deceiving. It is true that in less than two hours one can learn the main factual findings and conclusions about the population of London which Graunt described. However, after 334 years the controversy that the content and the circumstances of the publication of the *Observations* occurred are issues not yet settled.

The allegation that we now have no time even to read papers published a few, let alone 334, years ago is nonsense when applied to Graunt's book. This is not just any book, but the first in more than three centuries of demography. Secondly, the *Observations* have long ago passed the test of ageing, and it is the one can safely lay claim as the fountainhead of demography as a science. However, this picture is hardly the one students get in demographic literature. Indeed, nor can the accounts of Graunt's legacy to modern demography be regarded as monolith and well established. So what are the main portrait of Graunt's contribution to demography found in demographic literature? This question is dealt with in the first part of this essay. Three somewhat distinct portraits of Graunt's work are identified below; each of these three portraits suggest that contemporary historiography depicts the inception demography as a cracked mirror, for the images students can learn from the debut of demography are of three main types: completely distorted, very fragmentary, or somewhat reductionists. Taken together, these portraits illustrate the failure to recognize the broad and long-lasting legacy that contemporary demography has inherited from Graunt's *Observations.*

Against the overview provided in the first part, the second part of this essay provides an alternative reading of Graunt's book, one which portraits the *Observations* as the paragon for demography’s whole design. The bulk of this discussion is framed around five elements: (1) its subject matter and objectives; (2) its methodological approach and specific methods of inquiry; (3) its intellectual and institutional context; (4) its discourse or language of communication; and (5) the utility of its results. These five features have been chosen because they appear to play an crucial role in the making of any science, and in this case, demography as well. Moreover, they take into consideration not only the limitations of the portraits identified in the first part, but also the need to recognize that even though they are closely interconnected to each other, they should not be conflated into one another because the role of each depends upon the others.

The main purpose of this essay is not to encourage the reader to seek out Graunt's book and read it in its original form and for its own sake. It might be true, as Flew (1970: 8) put it, that 'The classics, as the cynic said, are like the aristocracy; we learn their titles and thereafter claim acquaintance with them'. But there is a certain irony in this statement that runs against itself. Flew's remark was made in his Introduction to Malthus's *Essay on the Principle of Population*, a classic which seems to have provoked rather more reasons than Graunt's *Observations* to motivate people to go beyond the title of Malthus's book. If one assumes that the latter has been read more often than the *Observations*, both within and across several allied disciplines of demography, Graunt's book would have to be classic as a super-classic. In any case, even if it has become widely acceptable in our field to mention the classics without having to bother reading them at all, this habit can neither be attributed to negligence, nor even dismissed as an absolute anachronism. While demographers trust on the reconstructions elaborated by the historians of their own field, in the end historiography of science exists just for that: to avoid that all scholars of each age should seek out the classics and trace in their original forms the development of the main ideas of their field.

Therefore, more than just encourage the reader to seek out Graunt's book and read it for its own sake, or even as an outsider or naive reader, this essay is concerned mainly with the historical reconstructions of the evolution of population inquiry. As Crombie put it in the paragraph chosen as epigraph of this introduction, the interrogating historian his guided by his proper art; the interrogated past is reconstructed from what the historian sees and understands.

With regard to the debut of demography, and Graunt's book in particular, the existing historical reconstruction seem less ingenuous than contemporary historians make their readers to believe. On this, some will perhaps be puzzled that I find it necessary to place a great deal of attention to the philosophy of science lying behind the dialogue between the practice of present historians and the events and aspects of Graunt's work which they to see as much as to ignore and understand. To borrow a phrase from Blaug (1980: 33), if Popper is right about the myth of induction, those who want 'to tell it as it is' will find themselves driven 'to tell it as it should be'.

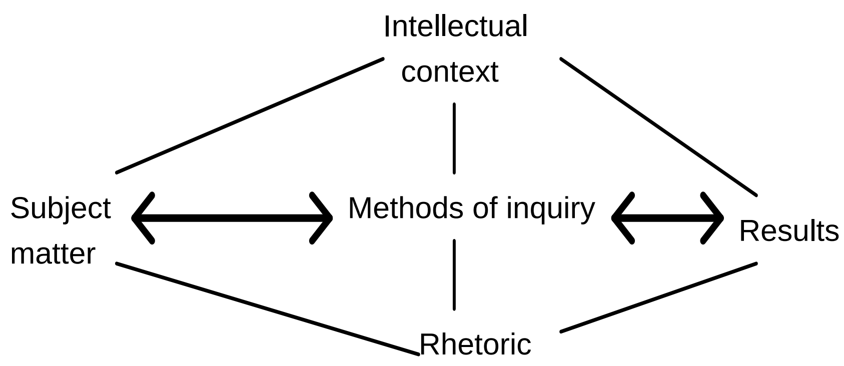
In short, this paper is mainly concerned in challenging those who are interested in the history of demographic ideas to re-consider the existing portraits of Graunt's book. Just as demographers cannot understand what the reality of population is independent of theory, it would be naive to believe that the historical recollection and the practice of historians are generally guided by the events of the interrogated past. If Crombie (1994: 8) is right, 'We are alerted to the past by experience of the present'.

Demographers need to know the historical background of their field to be able to comprehend their past as much as to be well-situated to comprehend what lies ahead. So in a third and final part, this essay discusses the usefulness of the new reading of Graunt's *Observations* proposed here. Three main aspects are discussed concerning the possible implications of a more accurate and comprehensive understanding of the debut and earlier development of demography for teaching and research.

Reading Graunt's *Observations* and everything that appeared relevant to understand its context was more than entertaining. Moreover, it is pleasing that this essay has been motivated by a topic of which Graunt was certainly not aware: a two-sex demography as I envisaged it in my Ph.D thesis (Francisco, 1996); but how and why a two-sex demography has anything to do with Graunt's *Observations* is not addressed in this essay. Beyond that, I can just feel pleased for the understanding and support I received from the supervisors of my Ph.D thesis; the words of one of them are of particular interest here, in that they sum up the circumstances against which this essay has developed: 'I am glad you are reading Kreager and the classics of the field. I fear that few people passing through the ANU demography program even get to hear of Graunt, let alone Kreager's other "early moderns" '.[*]

[*] The bulk of this essay was finilizaed in March of 1995 and appears in my PhD thesis as Appendix A (Francisco, 1996). Preliminary ideas expanded here can be found in a paper presented at the Seventh National Conference of The Australian Population Association (Francisco, 1994). I am grateful for the encouragement I received from my supervisors and advisers, Professors Geoffrey McNicoll, John Caldwell, Gavin. Jones, Dr. David Lucas and Dr. Chris Young. I thanks also the editing by Mrs Wendy H. Cosford, the reading by Dr Miroslava Prazak, and discussion with the participants at a seminar on 30 April 1996 in Department of Demography at the Australian National University.

Figure A1: A framework for the analysis of Graunt's 'Observatic



To get an idea of Graunt's overall demographic work one should look at the fifth edition of the *Observations* because this was the much enlarged and last version published while Graunt was still alive. The fifth edition comprises the following rubrics: two dedications, one to Lord Roberts and the other to Sir Robert Moray; a synoptic Index of 106 propositions on 'the positions, observations, and questions contained in the discourse'; the preface; an Appendix of Tables; 'some further observations of Major John Graunt'; the 'advertisements for the better understanding of several Tables: videlicet', and the following 12 chapter headings:

1. Of the Bills of Mortality, their beginnings, and progress
2. General Observations upon the Casualties
3. Of Particular Casualties
4. Of the Plague
5. Other Observations upon the Plague, and Casualties
6. Of the Sickliness, Healthfulness, and Fruitfulness of Seasons
7. Of the differences between Burials, and Christenings
8. Of the difference between the numbers of Males, and Females
9. Of the growth of the City
10. Of the Inequality of Parishes
11. Of the number of Inhabitants
12. Of the Country Bills

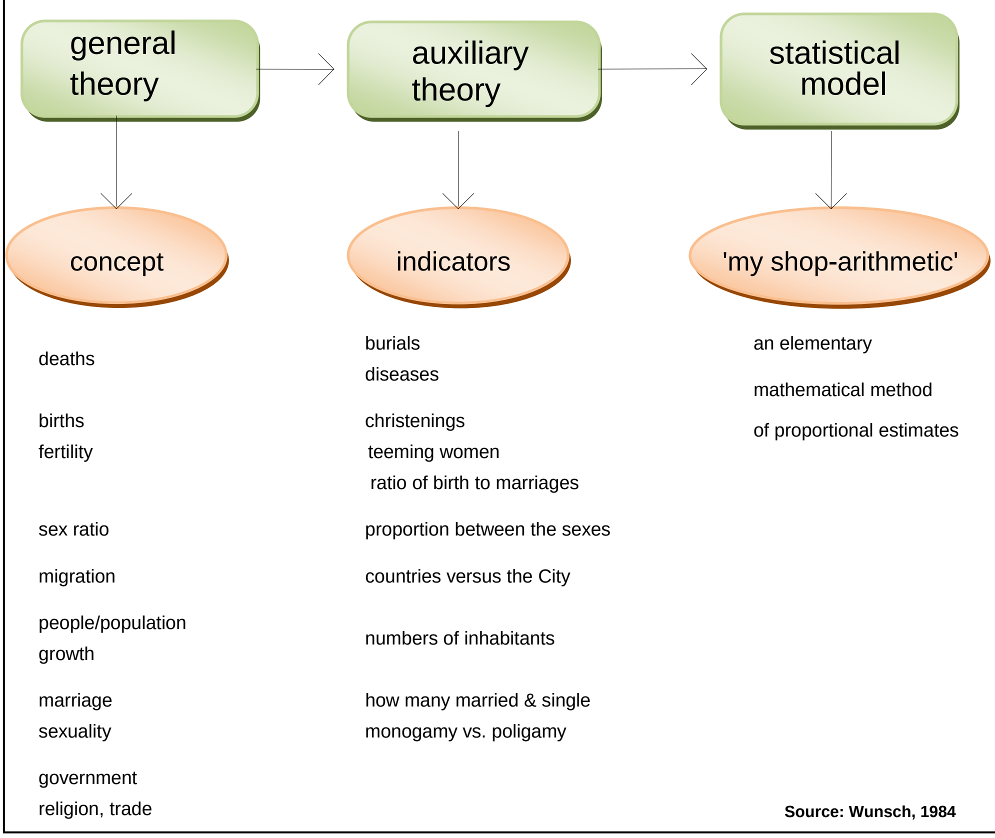
The Conclusions

The content of the these chapters was summarized by Graunt in his Index of 106, which provides a good idea of 'the positions, observations, and questions contained in this discourse'. Graunt's Index in reproduced in Table 1 in annex, not just to give the read the opportunity to grasp quickly the content of the whole book; the last four columns in Table 1 estimate the portions of the book dedicated to eight main topics drawn from the topics enumerated in the sub-title in the cover of the book and first of the two dedications: (1)

data appraisal; (2) death - diseases, health and longevity; (3) births - fruitfulness and productivity of marriage; (4) sex ratio - proportion between the sexes and ages; (5) migration between countries and the City of London; (6) Air (enviroment); (7) population change - the numbers of inhabitants and population growth; (8) population policies - government, trade and religion. The estimate of the portions of the book dedicated to these topics will be used below in the section dedicated to the utility of Graunt's results.

Box 1 provides what, in the language of today, may be called an 'executive summary' of Graunt's book. The paragraphs contained in Box 1 are extracted from the two dedications and the preface with the objective to illustrae the five elements which need to be taken into consideration to claim that the *Observations* provided the paragon for the whole design of demography as a social science.

Figure A2: The logic of demographic discovery in Graunt's 'Observations'



Source: Wunsch, 1984

Box A1: A sort of a Executive Summary of Graunt's *Observations*

1. Having been born and bred in the City of London, and having always observed that most of them who constantly took in the weekly Bills of Mortality made little other use of them, than to look at the foot, how the burials increased or decreased ... I though that the wisdom of our City had certainly designed the laudable practice of taking and distributing these accounts, for other and greater uses than those above-mentioned, or at least, that some other uses might be made of them: and there upon I casting mine eye upon as many of the General Bills as next came to hand, I found encouragement from them, to look out all the Bills I could (Preface, p. 14).

2. The observations, which I happened to make (for I designed them not) upon the Bills of Mortality, have fallen out to be both political and natural, some concerning trade and Government, others concerning the air, countries, seasons, fruitfulness, health, diseases, longevity, and the proportions between the sex and ages of mankind. All which (because Sir Francis Bacon reckons his discourses of life and death to be natural history ... (Dedicatory to Sir R. Moray, p. 6)

3. as it relates to natural history, and as it depends upon the mathematics of my shop-arithmetic (p. 6)

4. Now having (I know not by what accident) engaged my thoughts upon the Bills of Mortality, and so far succeeded therein as to have reduced several great confused Volumes into a few perspicuous Tables, and abridged such observations as naturally flowed from them, into a few succinct paragraphs, without any long series of multiloquious deductions ... (Dedicatory to Lord Roberts, p. 4).[1] ... I have reduced into Tables (the copies whereof are here inserted) so as to have a view of the whole together, in order to the more ready comparing of one year, season, parish, or other division of the City, with another, in respect of all the burials and christenings, and of all diseases and casualties happening in each of them respectively (Preface, p. 14).

5. I did then begin not only to examine the conceits, opinions and conjectures, which upon view of a few scattered Bills I had taken up; but did also admit new ones, as I found reason, and occasion from my Tables ... Moreover, finding some truths, and not commonly believed opinions to arise from my meditations upon these neglected papers, I proceeded further, to consider what benefit the knowledge of the same would bring to the world (p. 14).

6. I conceive ... how few starve of the many that beg:
   - that the irreligious proposals of some, to multiply people by polygamy, is withal irrational and fruitless
   - that the troublesome seclusions in the Plague-time is not a remedy to be purchased at vast inconveniences
   - that the greatest Plagues of the City are equally and quickly repaired from the country:
   - that the wasting of males by wars and colonies do not prejudice the due proportion between them and females
   - that the opinions of Plagues accompanying the entrance of Kings is false and seditious
   - that London, the Metropolis of England, is perhaps a head too big for the body, and possibly too strong
   - that this head grows three times as fast as the body unto which it belongs, that is, it doubles its people in a third part of the time
   - that our parishes are now grown madly disproportionable
   - that our temples are not suitable to our religion
   - that the trade, and very City of London removes westward
   - that the walled City is but a one fifth of the whole pile
   - that the old streets are unfit for the present frequency of coaches
   - that the passage of Ludgate is a throat too straight for the body
   - that the fighting men about London are able to make three as great armies as can be of use in this Island
   - that the number of heads is such as hath certainly much deceived some of our Senators in their appointments of Pollmoney, etc. (Dedicatory to Lord Roberts, p. 4, 5).

7. How far I have succeeded in the premises, I now offer to the world censure (p. 14).

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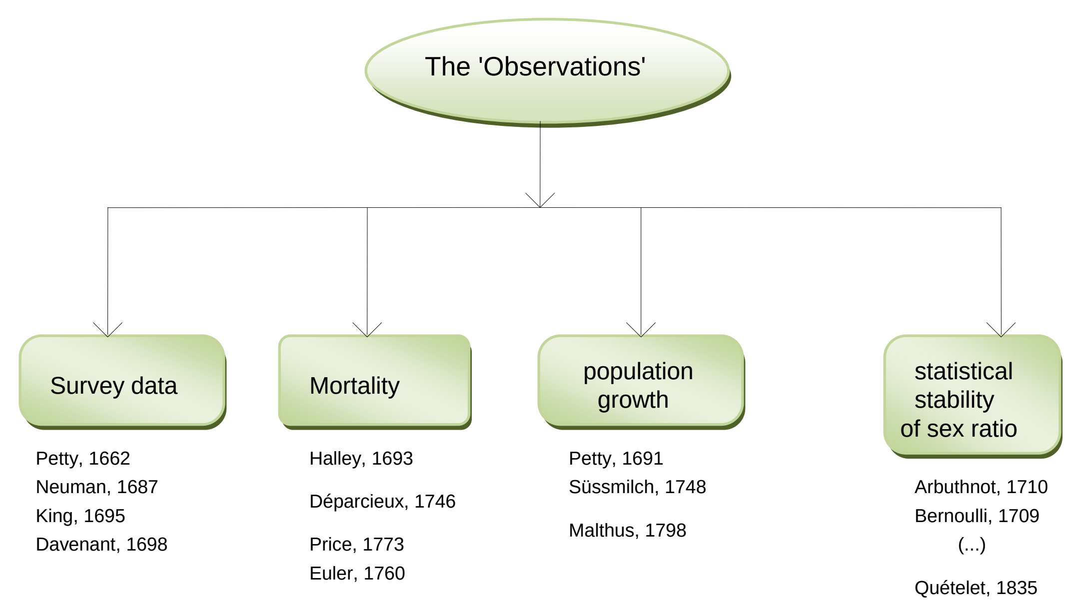
[1] Hald considered this paragraph an admirable program for descriptive statistics which 'has ever since been a goal for any statistical office'.

Table A.2: Percentages of the main subjects found in Graunt's book according to selected categories

Code	Designation	Percent
1	Data appraisal	15
2	Deaths -Diseases, health and longevity	22
3	Birth - fruitfulness and productivity of marriage	11
4	Sex ratio - proportion between the sexes and ages	16
5	Migration between counties and the City of London	7
6	Air (environment)	2
7	Population change - the numbers and growth	19
8	Population policies - government, trade and religion	8
	Total	100

Source: see Table A2.

Figure A3 The 'Observations' as a source of diverse theoretical developments



Appendix B:

Chronological literature: the 'two-sex problem' versus the 'determinants of fertility'

1917

Knibbs: A pionner of a new world in demographic theory

1917 Knibbs, G. George H. (1917). *The Mathematical Theory of Population, of its Character and Fluctuations, and of the Factors which influence them, being an Examination of the general of Statistical Representation, with deductions of necessary formulae; the whole being applied to the data of the Australian Census of 1911, and to the elucidation of Australian Population Statistics generally*. Appendix A, V. I, 1911 Census of the Commonwealth of Australia. Melbourne: Minister of State for Home and Territories.

1922 - 1946

Direct recognition of the male-female conflict

1922 A. Lotka, 'The stability of the normal age distribution'. *Proceedings of the National Academy of Sciences,* 8: 339-345.

1925 L. I. Dublin and A. J. Lotka, 'On the true rate of natural increase'. *Journal of the American Statistical Association*, 20, 150: 305-339.

1932 R. Kuczynski, *Fertility and Reproduction: Methods of Measuring the Balance of Births and Deaths.* New York: Falcon Press.

1937 R. Husson, 'La fécondité bigène de la population française en 1931'. Congrès International de la Population 1: 220-221, Paris.

1939 A. Lotka, *Théorie Analytique des Associations Biologiques. Deuxième Partie, Analyse démographique avec application particulière à l'espèce humaine. 780-XII*. Paris: Hermann et Cie, Éditeurs.

1939 C. Tietze, 'Differential reproduction in England'. *The Milbank Memorial Fund Quarterly,* 17, 3: 288-293.

1941 C. Quensel, Method of measuring reproduction should give the same result when applied to the male and female populations.

1941 R. Myers, 'The validity and significance of male net reproduction rates'. *Journal of American Statistical Association,* 36: 275-285.

1946 P. Vicent, 'De la mesure du taux intrinsèque d'accroissement naturel dans les populations monogames'. *Populationm,* 4: 699-712.

Karmel, 1948c: 34-44)

1906 - 1946

Indirect recognition of the male-female conflict through nuptiality

1906 E. Yule, 'On the changes in the marriage and birth rates in England and Wales during the past half century; with the inquiry as to their probable causes'. *Journal of he Royal Statistical Society*, 69: 88

1911 L. Bortkiewicz, recognized the interdependence of male and female nuptiality through the sex and age distribution.

1926 L. Connor, 'Fertility of marriage and population growth'. *Journal of he Royal Statistical Society*, 89: 553.

1928 A. Wolfe, 'The population problem since the World War'. *Journal of Political Economy*, 36: 529.

1929 W. Thompson, 'Population'. *American Journal of Sociology*, 34, 6: 959-975.

1933 W. Thompson and P. Whelpton, *Population Trends in the United States*. New York.

1934 F. Lorimer and F. Osborne, *Dynamics of Population Growth*. New York.

1936 R. Chaddock, 'Age and sex in population analysis'. *Annals of the American Academy of Political and Social. Science.*, 188: 185.

1936 H. Pollak, 'European population growth since Union'. *South African Journal of Economics*, 4: 1.

1937 S. Somogyi, calculated elaborate nuptiality tables for Italy 1930-32 for both males and females.

1937 F. Honey, 'The estimated population of Great Britain 1941-71'. *Journal of the Institute of Actuaries*, 68: 323.

1938 E. Charles and P. Moshinski, *Political Arithmetic*, London.

1938 P. Depoid, 'Tables de nuptialité et de fécundité pour la France 1930-32'. *Bulletin. de la Statistique Générale de la France*, 27: 269.

1939 C. Quensel showed how much higher the female net reproduction rate would be in 1955 if it were based on the assumption of constancy of *male* nuptiality conditions than if it were based on the assumption of contancy of *female* nuptiality conditions.

1939 C. Glass and Blacker, *The Future of our Population*. London.

1941 C. Glass, *Population Movement and Policies*, Oxford.

1941 E. Charles, 'The nuptiality problem with special reference to Canadian marriage statistics'.

1941 E. Heberle, 'Social factors in birth control'. *American Sociological Review*, 6, 6: 794-805.

1942 W. Thompson, *Population Problems*. New York.

1943 P. Landis, *Population Problems - A Cultural Interpretation*. New York.

1945 A. Myrdal, *Nation and Family*, London.

1945 H. Hyrenius, H. showed that nuptiality conditions must be regarded as a function of the proportion of the sexes on the 'marriage market' and that male and female nuptiality conditions cannot be regarded independently.

1946 C. Clark and R. Dyne, 'Application and extension of the Karmel formula for reproductivity'. *Economic Record,* 22: 23.

Karmel, 1948c: 44-50

1947 - 1969

1947 J. Hajnal and W. Hopkin, 'Analysis of the births in England and Wales 1939 by father's occupation'. *Population Studies*, 1: 187, 275.

1947 P. Karmel, 'The relations between male and female reproduction rates'. *Population Studies,* 1, 3: 249-274.

1947 W. Hopkin and J. Hajnal, 'Analysis of the births in England and Wales, 1939, by father's occupation - Part I'. *Population Studies,* 1, 2: 187-203.

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