Data, Models, Theory and Reality: The Structure of Demographic Knowledge[1]

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Introduction

The central argument of this paper is that the development of demography as a science has been hampered by confused ideas and inconsistent practice with respect to scientific methodology. In particular, demography has been preoccupied with empirical data and with techniques for the analysis of empirical data, to the neglect of the systematic development of theory. Theory development in turn has been hampered by the widespread acceptance of the methodological ideas of logical positivism [the philosophy of science of Nagel and Hempel, but also a statistical tradition tracing back to Karl Pearson] according to which social science theory, if possible at all, must based on empirical generalisations, preferably universal empirical generalisations.[2]

With respect to abstract analytic theory and abstract models, demography has been schizoid. Few demographers would deny the 'validity' of the stable population model or of its fruitfulness in generating substantive conclusions, even though few real world populations closely fit the stable model. By contrast, abstract behavioural theories such as transition theory or the microeconomic theory of demographic behaviour are often dismissed on the grounds that they admit of empirical exceptions or are 'unrealistic.' The tendency is to draw a sharp line between 'formal' demography, built on necessary relationships, and 'behavioural' demography, built on empirical generalisations regarding 'contingent' relationships.

Insight into these methodological issues can be garnered from a consideration of two leading demographers of the last half of the 20th century, Ansley Coale and Nathan Keyfitz. Coale, to the best of my knowledge, has never written systematically on the methodology of demography, so one must look at his actual work, and methodological remarks made therein, often in passing. In the case of Keyfitz, we can turn to a 1975

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[1] This paper has been prepared for a workshop on 'Agent-Based Computational Demography,' at the Max Planck Institute for Demographic Research, Rostock, Germany, 21-23 February 2001, Francesco C. Billari and Alexia Fürnkranz-Prskawetz, organisers. Ongoing research on modelling and demographic theory has been supported by the Social Sciences and Humanities Research Council of Canada.

[2] Another factor, not further discussed here, has been the heavy involvement of demography in purely descriptive work, due to its close association with government statistical bureaus, whose central mandate is description, not the advancement of demographic science as such. This close association clearly has had advantages for demography, but also costs.

paper, more or less neglected, in which he argues that much of our best demographic knowledge was produced by the use of abstract models, not by means of data analysis.

Interestingly, Coale relied heavily on abstract models in much of his earlier work on such formal topics as the demography of population aging, but also on such behavioural topics as the impact of high fertility and rapid population growth on economic development in low-income countries. In his later work on historical fertility transitions in Europe, Coale seems to have reverted to a radical positivist approach, not willing to generalise beyond the data, and ending with a somewhat anticlimactic restatement of transition theory reminiscent of Notestein's statements forty years earlier. It is arguable that the theoretical returns to the heavy investment of time, money and personnel into the European fertility project were disproportionately small. Hobcraft has recently made the same argument with respect to the World Fertility Survey and its successors [notably the Demographic and Health Surveys].

Keyfitz's claim for the greater fruitfulness of abstract modelling over data analysis is neither new nor unique. Descartes favoured thought over observation as the way to knowledge; the 'new scientists' such as Francis Bacon favoured observation [data]. John Locke attempted a synthesis that comes close to a balanced view of empirical science: experience and reflection on experience, or, observation and theory. More recently, Karl Pearson and Ronald Fisher are reputed to have parted ways over the issue of correlational studies of large samples [data] versus experiments on smaller samples to test ideas about mechanisms [theory].

John Platt [1964], in a classic paper on scientific methodology, recalls a 1958 conference on molecular biology, at which theoretical modellers were criticised by empiricists. Leo Szilard is quoted as commenting about protein synthesis or enzyme formation that 'If you do stupid experiments, and finish one a year, it can take 50 years. But if you stop doing experiments for a little while and *think* how proteins can possibly be synthesized, there are only about 5 different ways, not 50! And it will take only a few experiments to distinguish these' [Platt, 348]. An empirical researcher is reported to have replied 'You know there are *scientists*; and there are people in science who are just working with these oversimplifed model systems -- DNA chains and in vitro systems – who are not doing science at all' [p.346]. The subsequent history of molecular biology suggests who was on the right track.

Keyfitz's view is echoed and supported by a wide variety of writings on scientific methodology, all seeking an alternative approach to logical positivism, seen as leading to dead-ends and theoretical frustration. As examples, I cite and briefly discuss an early statement by Eugene Meehan [1968], a political scientist, some representative authors of the 'semantic' school in the philosophy of science [Giere, 1999] and Cartwright, 1999], and a recent call for a return to abstract analytic theory and the search for social mechanisms by a number of sociologists [Hedström and Swedberg, 1998].

These issues can be conveniently discussed by means of a diagram of the closeness or distance among four key elements in science, namely, data, models, theory and reality, with appropriate distinctions among various types of models.

This methodological thinking, along with the recent history of demographic research, point to the value of greater respect for and attention to abstract models in demography. They also suggest abandoning the sharp distinction between 'formal' and 'behavioural' demography, on the grounds that all good scientific theories or models are in fact formal. Similarly, the distinction between 'theory' and 'models' is minimised, since a rigorous theoretical statement will shade off into a model. Finally, given the complexity of the real world and the ability of the computer-aided scientist to handle larger amounts of complexity, much of our more fruitful theoretical work will consist of computer modelling [Burch, 1996, forthcoming 2001]. The pervasive but false contrast between theory as verbal, and modelling and empirical research as quantitative will be abandoned.

The Methodology of Ansley J. Coale

By all accounts, Ansley Coale has been one of the most versatile, creative, and influential demographers of the late 20th century. Like most demographers, he was not self-conscious or explicit about the methodology of demography -- logic and epistemology as opposed to technique. Implicitly, his work might be taken to suggest ambivalence regarding the proper roles of data, models, and theory, or at least some changing emphases over time.

A *leitmotiv* of his career is formal mathematical modelling of demographic dynamics, popularising and extending the work of Lotka and other early pioneers. This work relies on mathematically necessary relationships in highly abstract population models, with the stable model as central. Generalisations emerge from the models rather than from extensive empirical research. A prime example is his work on the demographic determinants of age structure [see, for example, Coale, 1956]. Using the stable model and the standard projection model, he concluded that fertility variation generally has appreciably more impact on age structure than mortality variation, and that the effects of mortality are heavily dependent on the age-pattern of change, with the possibility that some patterns of change in overall mortality can make a population younger. Coale used data to calculate the models, of course, but his generalisations depend on the manipulation of relatively simple abstract models, not on the systematic analysis of empirical data.

His monograph with Hoover [Coale and Hoover, 1958] also involved abstract modelling rather than broad empirical research, but on issues generally viewed as behavioural rather than formal, and involving a considerably more complex model. The core of this work was a linking of a standard population projection model with a standard economic growth model. The model is fit to the Indian case at length, and to the Mexican case more briefly. And there is some discussion – but no systematic empirical research – of the wider applicability of the analysis. It was what today is known as 'large-scale simulation.' The researchers were no doubt hampered by the absence of computer capabilities we take for granted, although they did engage in what would now be called 'sensitivity analyses.'

Coale was more deeply involved in the demographic parts of the work, Hoover in the economic. But they stress that there was co-operation of both in all parts.

Sometimes they are clear about the abstract character of their analysis: 'Our calculations…entail a "model" of the Indian economy, designed to take into account as realistically as an extremely simplified model can the main relevant features of that economy…[1958, p.259]. But the conclusions are set forth as general, applying to India, Mexico, and to most other low-income nations in the developing world.

They seem at times to overlook an element of circularity that is characteristic of all simulation, namely that the conclusions follow from the assumptions. They conclude: '…through this whole gamut of projections, despite the wide variation in rates of progress that they imply, the differential associated with reduced fertility is remarkably persistent and stable. [p.281]. But earlier they have told us that the model does not contain all important growth determinants, but only 'the growth determinants most clearly affected by the difference between our alternative rates of population growth' [p.260]. Since alternative rates of population growth in their scenarios depend mainly on alternative fertility assumptions, in hindsight the conclusion quoted just above is inevitable. And it is completely possible that the model is not relevant to concrete cases in which the omitted growth determinants are crucial.

A number of passages in the book suggest that the abstract character of their work was partially obscured in their thinking by the fact that the work was quantitative, that it dealt with concrete cases, and that it dealt with a very specific question – what difference would different fertility patterns make to economic development? In his preface, Frank Notestein, who generally abhorred abstract theory, seems to have been fooled by the work, noting that it is '…a highly original demonstration of the way careful factual analysis can illuminate the vital interrelationships of economic and population change [pp.v-vi]. But clearly it is factual only if one accepts theoretical 'facts' and reasonable assumptions as well as quantitative empirical data. And perhaps it is factual by contrast with pure theoretical speculation, not grounded in the details of a particular concrete case. But it certainly is not empirical work in the ordinary sense of that term.

The central point is that general propositions that emerged from this project were based on the model not just on empirical data. Indeed, some of the critics of Coale-Hoover [Kuznets, Easterlin] criticised it precisely on the grounds that comparative empirical research showed no strong or regular relationship between population growth rates and economic development.

When Coale turned his attention to fertility transitions, the orientation became much more empirical. In one of his earliest papers on the European fertility project [1965], he presents his indirectly standardised ratios and a few early results at the national level. The paper does not explicitly deal with classic transition theory, but implicitly calls it into question. Methodological comments made in passing suggest a radical logical positivism. Speaking of the decline of marital fertility, he comments 'There are few, if any, universally valid generalizations about the circumstances under which neoMalthusian fertility reduction occurs' [p.5]. After a list of frequently hypothesised causal factors, he notes that 'Examples can be found illustrating the presumed influence of each of these factors, but counter-examples or exceptions are nearly as prevalent' [p.6]. He concludes: 'Fertility reduction seems to be a nearly universal feature of the development of modern, secular societies, but its introduction and spread cannot yet be explained by any simple, universally valid model or generalized description' [p.7]. Looking to the future he expresses the hope that further empirical research 'tracing the decline of fertility more systematically, and by geographic units smaller than nations, will certainly establish a fuller record of fertility reduction, and will perhaps make possible generalizations about the causes of the decline' [p.7].

Eight years later [1973] Coale deals with what would usually be called theoretical issues, in his IUSSP paper on 'The demographic transition reconsidered.' But the emphasis is still on the search for universal empirical propositions. Interestingly, Coale never uses the word *theory*, either in reference to Notestein's work or his own ideas [the word does not appear anywhere in the paper]. It is difficult to know just what he intended by his studious avoidance of the word *theory,* which would have seemed quite natural in the context.

In fact the paper eventually produces very broad generalisations that most social scientists would view as theory. Coale posits 'the existence of more than one precondition for a decline.' 'Three general prerequisites for a major fall in marital fertility can be listed': 1] it must be within the calculus of conscious choice; 2] reduced fertility must be advantageous; 3] effective fertility control techniques must be available [p.65]. The language is borrowed from mathematics; the three preconditions or prerequisites are in fact 'necessary conditions' for fertility decline [p.69]. A weakness of 'the idea [sic] of the transition is that it tells us that a high degree of modernization is *sufficient* to cause a fall of fertility, but does not tell us what degree (if any) of modernization is *necessary* to produce a fall' [p.69]. Coale notes that one or more of the three preconditions can exist in the absence of modernisation.

Coale acknowledges many good points about 'the idea of the transition' [Notestein's transition theory] but faults it finally on its inability to make more than qualitative statements about the course of demographic and fertility transitions. He notes, for example, that with respect to developing countries, transition theory was 'accurate in direction but inaccurate in detail, with respect to mortality' [p.68]. Transition theory was qualitatively correct regarding the past of developed countries and qualitatively correct in its predictions for less developed countries. But, 'In neither instance does it specify in terms that can be translated into quantitative measures, the circumstances under which the decline of fertility began' [p.68].

But Coale's three preconditions clearly are subject to the same criticism, especially since they are not presented as quantitative variables. He speaks of 'the degree of change that must occur before the preconditions are introduced…,' but does not always discuss the preconditions themselves as matters of degree, using words that suggest a 0-1 variable – whether the preconditions are 'present' or 'absent' [p.66]. There is little attention to the issue of how they might be quantified and operationalised.[3]

Coale's last major statement on fertility transitions is in his introductory chapter for the multi-authored summary volume on the project [Coale and Watkins, 1986]. The spirit of this essay is different from that of the 1973 paper, with a return to reliance on

[3] Only in 1997 has a serious effort been made to do this. See Lesthaeghe and Vanderhoeft, 1997.

abstract models to gain insight into population dynamics. There is, for instance, considerable discussion of what might be called a cyclic logistic model to describe pre-modern or even pre-historical population dynamics. Population growth leads to rising mortality; populations react by reducing marriage and/or fertility or otherwise reducing population growth; mortality declines to former levels; and the cycle starts over. Interestingly, the model is purely qualitative, and, of course, there is little empirical evidence to support it.

On transition theory, Coale seems to have given up the hopes expressed in earlier papers that the project would arrive at 'universal empirical generalisations.' The three preconditions are not even mentioned. One long paragraph [p.24] summarises the causes of transitional mortality decline in broad terms that would not have passed muster according to the standards of his 1973 paper. Ultimately, he writes of the fertility transition in language not so different from that of Notestein forty years earlier, with reference to 'typical' patterns of transition and some exceptions [pp.28-29]. There is no attempt to quantify the 'idea of transition' beyond the presentation of empirical measurements of fertility and nuptiality, and their time trends and intercorrelations with a limited number of independent variables. It is as though the sheer mass of data has led to an abandonment of attempts to develop new and better theoretical ideas or models, including a revised and more rigorous statement of transition theory.

This is a long story, but it makes an important point: a massive twenty year project with substantial resources and collaboration by a large number of first-rate demographers did not result in a substantial improvement in theory. This is not to deny the immense scientific value of the work as an empirical, descriptive study. And, in Coale's mind, description may well have been its central aim. Recall the quote cited earlier: '…tracing the decline of fertility more systematically…will **certainly** establish a fuller record of fertility reduction, and will **perhaps** make possible generalizations about the causes of the decline' [1965. p.7, emphasis added]. But theory did not flow from the detailed data; the methodological stance was such that theory could flow only from universal empirical generalisations, and these were not forthcoming.

John Hobcraft has recently commented in a similar vein on the small theoretical returns to the large number of comparative fertility surveys under the aegis of the World Fertility Survey and its successors such as Demographic and Health Surveys. Entitled 'Moving beyond elaborate description: towards understanding choice about parenthood' [2000], the paper argues that 'the results [of these surveys] did not live up to my own or to others' highest expectations; comparative analysis projects today are much less common; the Demographic and Health Surveys, the daughter of the WFS, have never had a serious comparative analysis capacity (beyond the mainly descriptive Comparative Studies)' [p.1]. He speaks of 'meagre returns,' and of 'meagre progress to date in moving forward our real understanding of fertility behaviour through cross-national comparative surveys' [p.11]. He adds that 'a profound shift of emphasis is required in order to make real progress'[p.1], that is, progress towards understanding.

Hobcraft's diagnosis:

> …the main problem for comparative analysis, over and above the sheer scale of data manipulation, has always been the rather limited number of explanatory variables which are sufficiently standardised and accorded enough credibility to be collected in every country. In part, this problem arises from a lack of a commonly accepted theoretical framework for understanding fertility behaviour, but it is also arguable that we shall never remedy the problem without better agreement and testing of comparable information [p.2].

He seems to agree with Griffith Feeney, who earlier [1994] noted that the surveys in question contained a lot of data but not necessarily the right data for testing or developing explanations and theory. Hobcraft comments, with respect to the comparative fertility surveys, that there has generally been '…a lack of conceptual and theoretical clarity about what elements should receive priority…,' and that '…an explicit theoretical orientation has been lacking…' [p.4], noting that the surveys were done for policy or even political purposes, not primarily to advance science.

Hobcraft's remedy would be to collect more and better data, with variables to be selected on the basis of the best theoretical thinking about the determinants of fertility. The emphasis would be on variables that are comparable cross-culturally, and especially on variables relating to the fertility decision-making process [defined broadly to emphasise the decision to become a parent], and on community-level variables. Analysis of such data would aim at 'global models,' models involving '…not just the same range of regressors but also…the same parameters' [p.3]:

> 'A deeper understanding would involve a closer specification, whereby the strength of the relationship was the same everywhere net of the correct range of other controls, or, the development of models which incorporate the factors which bring about variations in the strength of the relationship as a step towards the fuller model' [p.4].

Hobcraft thinks '…progress toward such models is essential for good theory' [p.3-4], but not that theory will flow from the data. Rather the appropriate data-collection and analysis will necessarily be informed and guided by theory. Of comparative description and 'detailed society-specific accounts,' he concludes that 'Neither…holds out great hope for reaching general theoretical understanding' [p.5].

When all is said and done, however, Hobcraft's faith in future progress tilts towards better data and more sophisticated data analysis. And the goal of 'global models' suggests an assumption that widespread, if not universal, empirical generalisations are there, if only we can find them. The logical positivist approach maintains its hold.

Keyfitz on the Fruitfulness of Abstract Modelling

An alternative approach is to be found in an interesting paper by Keyfitz [1975], one of the few extant papers specifically on scientific methodology [as opposed to technique] by a demographer. In answer to the title question 'How do we know the facts of demography?', Keyfitz comments 'Many readers will be surprised to learn that in a science thought of as empirical, often criticized for its lack of theory, the most important relations cannot be established by direct observation, which tends to provide enigmatic and inconsistent reports' [p.267]. Citing E.O. Wilson, he speaks of 'the resistance of data to generalisation' [p.286].

To illustrate his point, he first looks at the issues of the interrelations among growth and proportion of elderly, and of the relative impact of fertility and mortality on age structure, both of which are best answered using populations models. In another section, entitled 'No model, no understanding,' he notes that statistical observations of differential incidence of breast cancer remain largely unexplained, and comments 'Here is just one more question that is unlikely to be solved by any volume of statistics by themselves' [p.276].

He then considers the issue of the effect of marriage delay on completed fertility, that of promotion in organisations, and the effects of development on population growth – all questions involving behavioural models on which there is less consensus than on the stable model used to solve the problems on age structure.

The important point is that Keyfitz attributes our answers to these issues to work with theory or models; also, he does not make a sharp distinction between formal models [e.g., the stable model] and behavioural models [e.g., transition theory]. The logical procedures involved in the statement and use of the two sorts of models are seen to be much the same. In a final section entitled 'The psychology of research,' he comments

> 'The model is much more than a mnemonic device, however; it is a machine with causal linkages. Insofar as it reflects the real world, it suggests how levers can be moved to alter direction in accord with policy requirements. The question is always how closely this constructed machine resembles the one operated by nature. As the investigator concentrates on its degree of realism, he more and more persuades himself that his model is a theory of how the world operates' [p.285].

Note the equation of model and theory in the closing sentence.

A Model-Based View of Science

The logical positivist view of science has dominated social science, including demography, in the latter half of the 20th century. According to this view, theory – a summary of what is known in a field – must be based on valid empirical generalisations or laws. Explanation, in this perspective, consists of subsuming some fact under a broader general proposition, which in turn is subsumed under a still broader generalisation, etc. – the so-called 'covering law' approach to explanation. Laws are subject to empirical test, to be 'proven,' or, in keeping with the widespread Popperian view, to survive efforts at falsification.

Not all social scientists have adhered to the dominant view, as Keyfitz's essay attests. As a discipline, economics has departed from a literal logical positivism, at least to a degree sufficient to allow and encourage abstract analytic theory and models, even if they seemed to some 'unrealistic.' Milton Friedman's essay on 'The Methodology of Positive Economics' is representative [M. Friedman, 1953]. Theories and models are viewed as analytic tools that may or may not be useful for analysing particular empirical phenomena, with their usefulness judged by their ability to yield understanding, and to predict phenomena not previously observed. Friedman acknowledges the formal or logical character of models, but still speaks of 'falsifying' or 'validating' them in terms of their ability to predict with regard to whole classes of empirical phenomena.. The notion of the search for universal laws, so characteristic of logical positivism, lurks just below the surface of the essay.

Many sociological theorists have attacked logical positivism, but the attack often has been aimed at positivism or empiricism as such, especially in its quantitative forms, not just at the particular approach advocated by Nagel and Hempel. The resulting schism between theory and empirical research in sociology remains strong.

A frontal attack on logical positivism that rejects neither empiricism nor formalisation is to be found in an extraordinary but neglected work by the political scientist Eugene Meehan, *Explanation in Social Science: A System Paradigm* [1968]. Meehan argues that the search for universal empirical generalisations is largely doomed to failure in social science, since such generalisations are and are likely to remain few and far between. He proposes instead explanation by the use of 'systems' [contemporary terminology would call them 'models' or 'theory'], totally formal structures which 'entail' or logically imply the phenomena to be explained. The systems are true only in the sense of being logically coherent. The relevant empirical question is not whether they are 'valid' or 'invalid' but whether, when 'loaded' with specific empirical information, they sufficiently resemble some portion of the real world [in his words, whether the system is sufficiently 'isomorphic' with reality] to enable the analyst to accomplish his or her purpose. The purpose can be prediction, which enables human beings to adjust to the world. Or, even better, it can be explanation, which provides insight into process and mechanisms such that one could at least in principle **control** the real-world system in question.

Meehan is wary of trying to explain large classes of events for the same reason that he is pessimistic about discovering universal empirical generalisations – classes of social events are often defined independently of attempts at scientific explanation, and typically are not particularly homogeneous.[4] The focus is on particular concrete events, but the key tool is abstract analytical reasoning.

Contemporary philosophy of science has increasingly challenged the classic logical positivist view of Nagel [1961] or of Hempel [1965], arguing that it is neither an accurate description of what scientists actually do nor a good guide to what they should do for their work to be fruitful. In this newer view, scientific laws are seldom if ever true representations of reality, but at best idealisations of certain features of an indefinitely

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[4] Chemical compounds may **behave** the same everywhere to the extent that they **are** the same everywhere. But demographic categories like *marriage* or *fertility transition* pertain to very heterogeneous classes of events. See Burch and Bélanger, 1999.

complex real world. Nor are they so much 'discovered' in nature as constructed by the human mind. Cartwright [1983, 1999] speaks of *nomological machines*: models created by the scientist generate laws rather than vice-versa [recall Keyfitz's use of the machine analogy].

Giere [1999] notes that most scientific laws are not universal, and that they are in fact not even true: '…understood as general claims about the world, most purported laws of nature are in fact false. So we need a portrait of science that captures our everyday understanding of success without invoking laws of nature understood as true, universal generalizations' [p.24]. The reason is that any law of nature contains '…only a few physical quantities, whereas nature contains many quantities which often interact one with another, and there are few if any isolated systems. So there cannot be many systems in the real world that exactly satisfy any purported law of nature' [p.24].[5]

For Giere, the primary representational device in science is not the law but the *model* , of which there are three main types: physical models; visual models; and theoretical models [Giere prefers the term 'model-based view' of science to the older, philosophical term 'the semantic view' of science]. Models are inherently abstract constructions that attempt to represent only certain features of the real world. They are true only in the sense that definitions are true. The question of whether they are empirically true is irrelevant, since they cannot be. The relevant question is whether they correspond to some part of the real world in a] some respects b] to a sufficient degree of accuracy for c] certain well-defined purposes [compare point b to Keyfitz's phrase 'degree of realism' or Meehan's notion of sufficient 'isopmorphism']. Giere gives the example of the model for the earth-moon system, which is adequate to describe and account for the moon's orbit and perhaps for putting a rocket on the moon, but is inadequate to describe the Venus-earth system. The prototype of scientific knowledge is not the empirical law, but a model plus a list of real-world systems to which it applies.

A model explains some real-world phenomenon if a] the model is appropriate to the real-world system in the three respects noted above, and b] if the model logically implies the phenomenon, in other words, if the phenomenon follows logically from the model as specified to fit a particular part of the real world. It would never occur to most physical scientists to add the second condition. But in social science, including demography, we are so accustomed to loose inference that its explicit statement is necessary.[6]

Note that in this account of science, all models are *formally* true [assuming, of course, no logical errors or internal contradictions], that is, true by definition. The empirical question then becomes one not of empirical truth or validity, but whether a valid model applies to a particular empirical case.

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5 Giere, with considerable training in physics, draws many of his examples from that field. If his conclusions apply to physics, they would seem to apply all the more forcefully to other scientific disciplines, for example biology or the social and behavioural sciences.

6 As noted above, the notion of explanation as logical inference from a model is central to Meehan's *Explanation in Social Science* [1968]. The need for rigorous logic is emphasised by Platt [1964].

Of course some models are more widely applicable than others, and, other things equal, science will prefer the model with the widest applicability. In demography, for example, the fundamental demographic equation is true by definition and applicable to every well-defined real population [neglecting error in data]. The exponential growth formula is true by definition, and, with respect to calculation of the average annual growth rate over a period is also applicable to every real-world population. With respect to describing a population's growth trajectory, however, the exponential growth formula applies more or less to some populations, but is not at all applicable to others.

A behavioural model such as the theory of demographic transition can be stated in such a way that it is formally true. Its 'validity' has been a matter of debate for over fifty years. But it is worth noting, in terms of Giere's criteria of applicability, that it correctly represents a large number of actual cases of mortality/fertility decline, at least in qualitative terms.[7]

In my reading of Giere's and Cartwright's accounts of science, they come close to what has long been the standard approach in the literature on mathematical modelling, and more recently of computer modelling. A model is an abstract construct that may or may not be useful for some well-defined purpose. In science, that purpose often will be explanation or prediction as opposed to practice. And in some schools of computer modelling, the emphasis is on less abstract models, trying to capture more of the complexity of the real world. But the central ideas are the same.

The model-based approach to science described above prefers not to make a sharp distinction between a model and a theory. Some authors distinguish the two on a general/specific axis; but then differences are in degree only not in kind. Giere speaks of 'theoretical models,' and sometimes describes a 'theory' as a collection of such models.

Note that this position has nothing to do with the view that science is totally a social construction. A good model is good precisely because it captures some important aspects of the real world. In Giere's words, there is 'realism without truth.'

Elements of Science

In thinking about some of the above issues involving the description and evaluation of scientific knowledge, it is useful to think in terms of four distinct but interrelated sets of elements:

1] reality: the real-world as it exists independently of human knowledge;

2] theory: coherent sets of ideas about how some portion of reality works;

[7] An interesting point about transition theory is that there has been a tendency to dismiss it as not fitting all cases or as not providing details of timing, pace, etc. There seems to have been relatively little effort to accept it as a valid model and work towards a more precise specification by defining functional forms for fertility or mortality decline as functions of 'development,' and parameters representing size of lags, slopes, etc.

3] data: observations and measurements on some real-world system;

4] models: abstract, but rigorous and specific, representations of reality, based on theory or data or both.

In some sciences, but not so much in sociology or demography, another element looms large, namely, the controlled laboratory experiment. This is reality in the sense that it exists independently of the human mind, but it often is artificial reality in that it does not occur in the natural world, without human intervention.

Some further comments on these elements:

The first element implies a belief in the existence of objective reality, an assumption that will be taken for granted and not further discussed here.

The second element implies that it is possible to have theories that are not mere fantasy, but express important insights into the real world, insights that can provide a basis for explanation, prediction, and, sometimes, control. If one believes that such theory is not possible with respect to human behaviour, whether because of complexity, free will, ideological bias, or some other reasons, then there is no discussion.

By *data* is meant a limited set of measurements on some portion of the real world. Note that virtually all non-trivial data sets involve error and various kinds of abstraction.

The word *model* is troubling in its ambiguity, but the ambiguity reflects actual usage of the word in different contexts.[8] A richer set of terms would help. In the meantime, several distinctions are necessary. The emphasis here is on the distinction between models that represent, usually in more specific form, a set of theoretical ideas, and models that represent a set of data or a structure contained therein – *theoretical models* and *empirical models*. But these two categories are not mutually exclusive; there are many hybrid or intermediate types.

Still, there many modelling exercises in which a particular data set has the upper hand, as it were, with little input from theory. An extreme case is that of *approximating functions*, where a functional form is found that best represents a data set [two or three-dimensional], without any regard to the substantive meaning of the function or its parameters. This typically will be done for purposes or smoothing, interpolation, or extrapolation. The TableCurve software provides a good explanation of this approach to modelling[Jandel Scientific, 1996]. It contrasts the use of approximating functions with what it terms *parametric functions*, in which the parameters can be given meaning.

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[8] The English language does not help in that there is only one verb form in current use, that is, *to model.* French distinguishes *modeler,* as in the acts of a sculptor, and *modeliser*, as in the fitting of a statistical model. The *Oxford English Dictionary* lists *modelize* as an obsolete English word, but the meanings of it and *model* do not seem to parallel the French distinction. Insofar as theory construction is a creative act of imagination, the French *modeler* seem the more apt word for it.

Parametric functions in turn can be theory based, with the functional form and its parameters mirroring some relevant theory. 'A parametric model with an underlying physical foundation relates dependent variable Y to the independent variable X via a postulated physical relationship between them' [p.3-19]. Or, in the absence of an underlying theoretical model, the parameters may simply characterise the data set. 'Here features of the data are known to have a direct relationship with the underlying…factors, but there is no quantitative underlying theoretical model' [p.3-19] – the system producing the data is treated as a black box. Parameters may describe such features of the data set as maxima and minima, transition points, slopes, etc.

Multivariate statistical modelling typically resembles atheoretical parametric modelling as described above. The parameters have substantive meaning [in fact as slopes], but the mechanism or underlying process leading from inputs to outputs is not specified. The selection of variables may be based on theoretical considerations, but just as often is based on common sense, availability of data, or previously observed empirical correlations. The statistical model is a representation of the structure, typically linear structure, of a data set. It is an abstract representation of an abstract data set using abstract concepts for measurement. But a statistical model is not purely empirical, fully determined by data. There must be *a priori* assumptions about what variables to include and about functional form. Such a model may be best-fitting, but from a very limited set of all possible models. A multivariate statistical model is not, or at least should not claim to be, a representation of the real-world system on which the data were measured.[9]

A good example of a theory-based model is provided by Hernes' [1972] study of first marriage. He starts with theoretical notions: a cohort has an underlying 'marriagability' or eligibility for marriage; for the individual, this marriagability tends to decline with age; pressures to marry increase as the proportion already married rises, but only up to the point where the scarcity of potential partners begins to take effect. These theoretical ideas are then expressed by means of a particular mathematical function, an asymmetric logistic, with the asymmetry due to an exponential decline in marriagability with age. Finally, the parameters are estimated from a particular data set on proportions ever-married by age. The parameters are estimated from data, and they take their original meaning from a theory. In a sense, the equation models both the theory and the data. This analysis can be compared and contrasted with a blind, mechanical fit of some mathematical function to a given data set on first marriage by age [an approximating function] and with a statistical model of event-history data on first marriage in relation to a set of co-variates, as in a hazards model, based only loosely on theory.

These distinctions are far from definitive. But the central distinction between modelling theoretical ideas and modelling a specific data set is paramount. Modelmaker, a systems modelling software package, makes a distinction between *empirical models* and *simulation or mechanistic models* [Walker, 1997]. Empirical models describe variation in

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[9] See the important distinction by Abbott [1988] of the *representational* versus the *entailment* interpretations or uses of the general linear model.. The entailment use of the general linear model reasons: given a theory about the social world, a certain linear structure should be observed in relevant data. The representational use involves the fallacy, in Abbott's view, of mistaking the linear model for a representation of the real-world system – the fallacy of reification or misplaced concreteness.

‘some observed data for a phenomenon which shows how it varies in relation to other factors’ [p.7]. ‘Simulation models…try to describe a number of sub-processes which can be combined to represent the behavior of a larger more complex system’ p.9]. The description of ‘empirical models’ – models of data – by Edwards and Hamson [1989] is worth quoting in full:

> An empirical model is one which is derived from and based entirely on data. In such a model, relationships between variables are derived by looking at the available data on the variables and selecting a mathematical form which is a compromise between accuracy of fit and simplicity of mathematics…. The important distinction is that empirical models are *not* derived from assumptions concerning the relationships between variables, and they are *not* based on physical laws or principles. Quite often, empirical models are used as ‘submodels,’ or parts of a more complicated model. When we have no principles to guide us and no obvious assumptions suggest themselves, we may (with justification) turn to data to find how some of our variables are related [p.102].

Both of the previous citations raise the issue of complexity, which cuts across the theory/data distinction we have been discussing. Hernes uses a relatively simple theory and model to account for a relatively simple data set, describing a relatively simple real-world phenomenon – cohort proportions first married by age. Hernes does not attempt a model of the complete marriage system, which would have to deal with issues such as endogamy/exogamy, spouse selection [by couple or by parents], premarital conception, betrothal, post-marital residence, etc. – all factors bearing on age at first marriage.

And so, we must keep in mind **three** kinds of modelling: modelling a limited set of theoretical ideas; modelling a limited set of data; and modelling a relatively complex portion of the real world, coming close to providing a replica – generally known as *large-scale simulation*. The last will of necessity require many theoretical ideas and much data, if it is to be at all successful, but success, as always, must be defined in terms of purposes.[10]

Assessing Scientific Knowledge

The aim of empirical science is to understand some portion of the real world. How well we do in this regard can be seen in terms of the closeness of fit among theory, data, models, and the real world. Empirical social scientists are used to thinking in terms of goodness of fit between a statistical model and data. We are less used to thinking in terms of the other relationships, diagrammed below:

[10] This kind of modelling is favoured in Casti’s *Would-be Worlds* [1997], although he is clear on the point that there are many kinds of modelling suitable to different purposes.

Reality

? ?

Theory ? Data

? ?

Models

The question marks between each pair of elements are inserted to emphasise that the closeness of one element to another is an open question. The distance of each of the above links may vary, and no one of them is necessarily or consistently shorter than another. It is important to include Reality in a diagram such as this, since it is the ultimate reference point of all empirical scientific work. Diagrams of the scientific process often do not explicitly include it, for example Wallace [1971].

In line with previous discussions, the bottom element, Models, might be re-written to reflect the distinction between theoretical and empirical models, but also the fact that they are best represented by a continuum.

Theoretical <--------------------- Models ------------------------> Empirical

In an older version of positivism, associated with Karl Pearson, theory and theoretical models drop out of the diagram. The most one can expect is to find stable correlations among data [Turner, 1987]. In the newer logical positivism of Nagel and Hempel, theory is possible, but it must be based on empirical generalisations arrived at by statistical analysis of data. In either case, it is assumed that data and empirical models are closest to reality. In the latter case, data and empirical models are the explicit foundation for theory, and a gatekeeper with respect to theoretical ideas.

But this positivist assumption is gratuitous; it needs to be demonstrated in any particular case and in general. It is completely possible that a good theory or a theoretical model can come closer to reality than data and statistical models. If nothing else, theoretical models can include variables which we know are almost certainly important, but which have not or cannot easily be measured. This point is seldom recognised in the demographic literature. An exception is an eloquent statement by Bracher, Santow, Morgan, and Trussell [1993], in their state-of-the-art analysis of Australian divorce, using unusually rich event-history data. They comment in the concluding section:

> However detailed and comprehensive the 'explanatory' factors we have had at our disposal, they are, after all, only dim reflections of the possibly unmeasurable factors that keep marriages together or drive them apart; and it would be unfortunate if the apparent sophistication of the measures were to obscure this fact [p423].

Theoretical models also can more easily incorporate non-linear functional forms and feedbacks, and deal with process and mechanisms. They can paint a plausible picture of how a real world system works, in a way that no data-bound statistical model ever can. Whether that picture fits that real world system closely enough to serve some purpose remains an empirical question. And so empirical data are necessary to describe selected features of real-world systems and observable phenomena to be explained. And, they may be necessary to help establish how closely a theoretical model fits the real world – they can never completely determine this. But data and empirical models do not close the understanding gap. For this theory is required.

A critical history of 20th century demography would show, I believe, that demography has tended to hold theory at arms length, only grudgingly giving it a small place in our discipline. The same has been true with regard to traditional mathematical modelling [apart from the core mathematical models such as the stable population] and with regard to later developments in computer modelling or simulation. The demographic mind has viewed data as solid and real, theory and simulation as airy and fanciful. A growing body of opinion in the philosophy of science would suggest that this view is faulty, as does the scant theoretical progress in the discipline, symbolised by the fact that transition theory is not in appreciably better shape now that it was forty or fifty years ago. More, and more informed, attention to theory is needed if demography is to be a science as opposed to a body of techniques.

Coda: On the Dangers of Dichotomies

Demography, along with other social science disciplines, has been plagued by the tendency to think, act, and institutionalise in terms of strict dichotomies: theory vs. empirical research; quantitative vs. qualitative; model vs. theory; scientific law vs. hypothesis; formal vs. behavioural demography, to mention a few of the more obvious. Different kinds of work and different approaches to science tend to be seen as discrete, polar opposites. Often they are seen as opposed, in the sense of being hostile to one another.

But some of them are false; others get in the way of the harmonious working together of the many parts of the scientific endeavour. The distinction, for example, between quantitative and qualitative overlooks the historical fact that that most quantitative tool of science – differential equations – often dealt only with qualitative solutions. In the more contemporary realm of computer simulation also, it is often the qualitative results that are of most importance. The precise quantitative results, after all, are essentially arbitrary.[11] In the burgeoning field of 'qualitative methods,' on the other

[11] In a recent book review, two immunologists have commented that in biology the real contribution of mathematics is not that it introduces quantification, but that it provides '…a precise qualitative framework

hand, it is ironic – but a healthy development -- to see the relevant software beginning to include utilities for the formation of frequency distributions.

Giere's and Cartwright's model-based views of science, by dethroning the concept of scientific law -- in physics of all disciplines, the one considered most lawlike – have underlined the abstract, unreal character of all scientific theory and theoretical models. In a sense all scientific theory is hypothetical. But the hypothesis is not whether it will eventually be proved true or valid, but whether it fits a particular part of the real world well enough to accomplish a particular human purpose.

The application of this thinking to demography appears to me to abolish the sharp distinction between formal and behavioural demography, as traditionally defined. The propositions of a behavioural theory or model must be formally true, true by definition. Otherwise, the model cannot serve as what Cartwright terms a *nomological machine*; it cannot generate implications or predictions that follow in strict logic from the model assumptions and structure. The idea of necessary relationships in our mathematical models in demography and contingent relationships in our theoretical model needs re-examination.

A final distinction that is breaking down is that between theory and modelling. Older texts on mathematical modelling and on social theory [as always, economics is an exception] have tended to occupy different worlds. Mathematical modelling books seldom had much to say about theory, and vice-versa.[12] The model-based view of science, on the other hand, sees the model as the primary representational device in science, encapsulating its theoretical ideas. The theory/model distinction is blurred. And, given the complexity of many social and demographic systems, the theoretical tool of choice becomes computer modelling or simulation.

Old words in a language often lead to striking insights. In English, there is an old word, *theoric* [also spelled *theorick*], described as obsolete and archaic by the *Oxford English Dictionary*. Its first meaning is 'theory.' Its third meaning is: 'a mechanical device theoretically representing or explaining a natural phenomenon.' Sixteenth century examples relate to astronomy. To many social scientists, the disjuncture between theory and a mechanical device is total. In Giere's model-based view of science, there are three kinds of models: theoretical; visual; and physical, that is, mechanical devices. Things have come full circle, back approximately to where they belong.

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of reasoning….As biological knowledge becomes ever more complex and detailed, so natural language becomes more inadequate for certain types of biological questions. Mathematics provides an efficient, precise, and rigorous alternative…. "mathematics is no more, but no less, than a way of thinking clearly"…' [Bangham and Asquith, 2001].

12 An interesting exception is by Doucet and Sloep [1992], who in their book on mathematical modeling in the life sciences, include a substantial discussion of the semantic school of philosopy, of which Giere and Cartwright are contemporary representatives.

References

Abbott, Andrew. 1988. Transcending general linear reality. *Sociological Theory* 6:169-186.

Bangham, Charles R.M.; and Asquith, Becca. 2001. Viral immunology from math. Review of *Virus Dynamics: Mathematical Principles of Immunology and Virology*. by Martin A. Nowak and Robert M. May. Oxford: Oxford University Press, 2000. *Science* 291:992.

Burch, Thomas K. 1996. Icons, strawmen and precision: reflections on demographic theories of fertility decline. *The Sociological Quarterly* 37: 59-81.

Burch, Thomas K. 2001 [forthcoming], Computer modelling of theory: explanation for the 21st century. In Robert Franck [ed.] *The Explanatory Power of Models: Bridging the Gap Between Empirical and Theoretical Research in the Social Sciences*. Nordrecht, The Netherlands: Kluwer Academic Press.

Burch, Thomas K.; and Bélanger, Danièle. 1999. L'étude des unions en démographie: des catégories aux processus. *Cahiers Québécois de Démographie*. 28:23-52.

Cartwright, Nancy D. 1999. *The Dappled World: A Study of the Boundaries of Science*. New York: Cambridge University Press.

Casti, John L. 1997. *World-Be Worlds: How Simulation Is Changing the Frontiers of Science*. New York: John Wiley & Sons.

Coale, Ansley J. 1956. The effects of changes in mortality and fertility on age composition. *Milbank Memorial Fund Quarterly* 34:79-114.

Coale, Ansley J. 1965. Factors associated with the development of low fertility: an historic summary. United Nations World Population Conference, Belgrade, Yugoslavia. WPC/WP/194. 8.pp.

Coale, Ansley J. 1973. The demographic transition. International Union for the Scientific Study of Population, *International Population Conference, Liege*. 1:53-72.

Coale, Ansley J.; and Hoover, Edgar M. 1958. *Population Growth and Economic Development in Low-Income Countries*. Princeton: Princeton University Press.

Coale, Ansley J.; and Watkins, Susan Cotts. 1986. *The Decline of Fertility in Europe*. Princeton: Princeton University Press.

Doucet, Paul; and Sloep, Peter B. 1992. *Mathematical Modeling in the Life Sciences*. New York: Ellis Horwood.

Edwards, Dillwyn; and Hamson, Mike. 1989. *Guide to Mathematical Modelling*. Boca Raton, Fla.: CRC Press.

Feeney, Griffith. 1994. Fertility decline in East Asia. *Science* 266:1518-1523.

Friedman, Milton. 1953. The methodology of positive economics. In *Essays in Positive Economics*. Chicago: University of Chicago Press. pp.3-43.

Giere, Ronald N. 1999. *Science Without Laws*. Chicago: University of Chicago Press.

Hedström, Peter; and Swedberg, Richard. 1998. *Social Mechanisms: An Analytic Approach to Social Theory*. Cambridge: Cambridge University Press.

Hempel, Carl G. 1965. *Aspects of Scientific Explanation*. New York: The Free Press.

Hernes, Gudmund. 1972. The process of entry into first marriage. *American Sociological Review* 37: 173-182.

Hobcraft, John. 2000. Moving beyond elaborate description: towards understanding choices about parenthood. Paper presented at UN Economic Comission for Europe, Fertility and Family Surveys Flagship Conference, Brussels, Belgium.

Jandel Scientific, 1996. *TableCurve 2D: Automated Curve Fitting and Equation Discovery*. San Rafael, CA: Jandel Scientific.

Keyfitz, Nathan. 1975. How do we know the facts of demography. *Population and Development Review* 1:267-288

Lesthaeghe, Ron; and Vanderhoeft, C. 1997. Ready, willing and able: a conceptualization of transitions to new behavioral forms. IPD Working Paper, Interface Demography, Vrije Universiteit Brussel.

Meehan, Eugene J. 1968. *Explanation in Social Science: A System Paradigm*. Homewood, Ill.: The Dorsey Press.

Nagel, Ernest. 1961. *The Structure of Science*. New York: Harcourt, Brace, and World.

Platt, John R. 1964. Strong inference in scientific research. *Science* 146 [16 October] 347-353.

Pollard, A.H.; Yasuf, Farhat; and Pollard, G.N. 1990. *Demographic Techniques* [3rd ed.]. Sydney, Toronto: Pergamon Press.

Turner, Stephen P. 1987. Underdetermination and the promise of statistical sociology. *Sociological Theory* 5:172-184.

Walker, Andrew. 1997. *Modelmaker: User Manual, Version 3*. Oxford: Cherwell Scientific Publishing.

Wallace, Walter L. 1971. *The Logic of Science in Sociology*. Chicago: Aldine Atherton.