

# **Population Paradigms: Pathways, Processes, Progressions, plus Pointlessness**

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Profound changes are needed in our approach to understanding demographic behaviour and the consequences thereof. Currently the dominant analytic approaches in the study of demographic behaviour and its consequences often rely on simple event history analysis or an economics paradigm (e.g. two of three papers in a recent symposium on causality in demography were essentially rooted in econometric models – see Moffitt 2003 and Smith 2003). Moreover, most work relies very heavily on secondary data sources that have not been tailored to answering specific demographic questions (see McNicoll 1992 for similar sentiments). The combination of these elements leads to a far too restrictive approach to modelling, measurement, and theory. In this paper, I seek to identify healthy trends that are already under way in broadening our compass and approach and to point the way to further developments: a combination of evolution and revolution.

Demographic behaviour is at the core of human existence, being concerned with crucial and intimate aspects of our lives. Understanding partnership, parenthood, well-being, position in society and in space, and the family as a key nexus of caring, intimacy, and commitment is the heartland of the determinants of demographic behaviour. The ramifications of such behaviour for the ways our lives play out are the heartland of the consequences of demographic behaviour. These are issues of profound importance and deserve much greater attention.

We need to stop being accountants, who are predominantly interested in answering questions about *when* events happen, rather than asking *why* behaviour occurs, or in describing in much detail *what* happens, rather than addressing *why* things happen. The use of the word pointlessness in the title reflects my concerns that we make demography more pointless by becoming less obsessed with events (or point-occurrences), but a great deal less pointless by concentrating on pathways, processes, and progressions. In order to achieve this transformation, we need to place much

greater emphasis on building and developing mid-level theories (for useful and quite different takes on theory in demography see Burch 1993, 1996, 2002 and 2003a and b, Massey *et al* 1993, Van de Kaa 1996, and Lesthaeghe 1998). Brass (1986) lauded demography for being ‘specific, pedestrian and modest – underrated qualities in social science’ and, by implication saw much social science as concerned with ‘the speculative, the diffuse, the ill-defined and the pretentious’. In this paper I want to emphasize a third way for demography that might be characterized as interdisciplinary, innovative, and focussed. To echo Hajnal (1955, p.321) we need ‘less computation and more cogitation’, although there may be useful insights from combining computation and cogitation (see Billari and Prskawetz 2003).

I argue that enhancing our understanding requires attention to *pathways* within the person, to *processes* whereby the person interplays with their context or environment, and to *progressions* through the life-course or over time, which involve the interplays of pathways and processes. But I also emphasize that the distinction between pathways within the person and processes outside the person is an expository convenience, since the really interesting challenges and research agendas arise from the interplays and interactions among and within these domains. Pathways, processes and progressions as organizing principles also indicate the departure from an event-oriented perspective.

Moreover, the rich tapestry of the interplays mandates that our understanding is ultimately rooted in broad-ranging, large, and expensive prospective studies, though real insights can be obtained along the way from small-scale, rigorous, prospective in-depth studies, combining qualitative and quantitative elements, that may well be embedded in the larger-scale endeavour. Much recent demographic research is using important prospective studies to explore some of the pathways involved in demographic behaviour.

However, I believe that we have a long way to go in developing studies that are both closely focussed on explaining particular aspects of demographic behaviour and sufficiently broad and multi-disciplinary in their compass to enable us seriously to begin discovering what really matters, and to start disentangling the mediating routes and feedbacks in the pathways, processes and progressions involved. In doing so, we

shall have to pay much closer attention to genetics, neuroscience, and psychology in understanding the within-person pathways, as well as radically improving our theory, conceptualisation, measurement, and subtlety in dealing with both inter-personal and institutional contexts. Illustrations and justifications of the broad-ranging approach needed are contained in the recent reviews of progress on child development (Shonkoff and Phillips 2000), on health (Singer and Ryff 2001), and on fertility (Wachter and Bulatao 2003).

Some of these concerns can be illustrated on a more modest scale through the work on understanding parenthood that I have undertaken in part with Kathleen Kiernan. We began by elaborating a mid-level conceptual framework, that addressed the range of elements that need to be considered in trying to understand the process of becoming a parent and applied this in a broad sweep to interpreting fertility trends and variations in Western Europe (Hobcraft and Kiernan 1995). Subsequently I used the same framework in a more detailed consideration of fertility levels and trends for England and Wales (Hobcraft 1996). A further step in the process was my attempt to elaborate the design required for a much more focussed study of the transition to parenthood (Hobcraft 2002), which has influenced but not determined the design of the more omnibus UNECE Gender and Generations Survey. More recently, I began trying to elaborate some of the requisite within-person pathways that need to be considered (Hobcraft 2003). This incomplete endeavour, of course influenced and informed by the work of many others along the way, is much needed in other areas of demographic behaviour. I might have used the range of studies on ageing, which have been influenced by the US National Institute of Aging, as a more elaborate and fully developed example if space had permitted.

In the space available I am only able to sketch some of the issues involved in how we examine various aspects of demographic behaviour and its consequences. In doing so, I focus initially on elements of demographic behaviour in turn and sketch some of the ways in which pathways, processes, and progressions are essential to our understanding, whilst emphasizing the need to move away from a narrow focus on symbolic events. Some of the important issues and findings relating to genetics, neuroscience, and social science are then reviewed. I then look at some of the lessons we can learn from other disciplines, notably biology, psychology, and epidemiology,

both about *how* to approach problems and for *what* we can learn substantively. In doing so, I draw some contrasts with the dominant economics (/sociology) rational choice paradigm and differences in conceptualisation, measurement, and approach.

I then briefly consider some of the issues involved in and consequences for the design, analysis, and interpretation of relevant research. Inevitably, given my broad compass, much detail will be omitted. An important issue that I shall not discuss in detail concerns the whole set of issues concerned with the ethics of research, especially on links between genetics and behaviour. An excellent and extended treatment of these issues is provided by Finch, Vaupel and Kinsella (2001) and a brief but valuable addendum by Rutter (2003).

Finally, I shall take up the theme that demographers will increasingly have to work in multidisciplinary teams in order to make serious progress in comprehending demographic behaviour. I believe we are better placed than most social scientists for this endeavour for a range reasons, including our history of being multidisciplinary and of engaging with the biomedical and natural sciences.

### **Some Implications**

I have argued at some length that demography need to be made *more* pointless by shifting away from an often too narrow focus on events *per se*. But, more importantly, demographic research needs to become much less pointless: focus on the rich tapestry of pathways, processes, and progressions; tackle the difficult and interesting problems of why behaviour occurs, rather than undertaking elaborate description; pay more attention to mid-level theories or frameworks, including judgements on what really (might) matter(s); look at mediating and protective factors; sharpen understanding of and distinctions between proximate, distal, and ultimate factors (at least).

One of the key judgements involved in working through the relationships suggested as a priority for investigation here is how to make real progress: do we move ever further backwards from demographic behaviour through proximate and then just less proximate (and so on) factors, or do we begin with the genome and explore forwards? Both are probably necessary and will undoubtedly often meet in the brain. I would

argue that the forwards exploration may, however get a more effective return in beginning to understand pathways through exploring genetic markers and their links to behaviour, informed by animal neuroscience, rather than from fMRI brain scans (largely on grounds of relative cost).

The broad agenda laid out here implies many shifts in the way that we research demographic behaviour. The emphasis on pathways necessitates that we collaborate with geneticists, neuroscientists and psychologists, since we require knowledge about candidate genes, likely neural pathways, and the underlying physiology and endocrinology. But it is also important for these disciplines that we collaborate, since the highly probable importance of environmental factors triggering endocrine responses among those that can cross the brain-blood barrier, and of interplays and differential expression of these proteins and their brain reception for different alleles, require a deep knowledge of the key contextual linkages too. Moreover, our substantive knowledge about environmental associations may help in identifying pleiosis. We all have much to gain from a fruitful collaboration of this kind. Moreover, such connections may help to move demographic behaviour into greater prominence for neuroscientists, geneticists and psychologists.

A further implication is the need to significantly improve our theories, measures, and data sources relating to the individual and contextual areas. This mandates large-scale prospective panel studies to enable the study of processes. Moreover these studies need a major cross-disciplinary investment, at least on the scale of the US HRS or ELSA or the UK Millennium Cohort Study, in order to make real progress in breaking out of narrow disciplinary silos and reaching some (tentative and revisable) consensus on what really matters and how to measure complex issues simply enough to make a broad-scale study workable without losing all content. As those who have been engaged in such cross-disciplinary endeavours will readily admit, this process is a challenging one, since individual disciplines always claim (often with some justification) that their topic requires very detailed measurement (e.g. components of income or personality measures). But these same researchers come out of the negotiation processes with a much better understanding of the issues involved and recognise the benefits from meeting the challenge.

Not only do we need such complex prospective studies, but we also need to make such studies more sharply focussed on addressing specific questions. We shall never be very likely to make much headway in understanding reproductive behaviour from 'omnibus' generic surveys that try to meet a very broad range of needs, even though quite a bit of useful work has been done using cohort studies of various kinds. Rather, we need studies with a clear and explicit design and focus that addresses one issue. An example would be the US Fragile Families Study (Sigle-Rushton and McLanahan 2002), which has drawn a sample very explicitly focussed on families where the partnership was 'fragile' at the time of the birth of a child. Hobcraft (2002) provides an example of how we might move towards a better understanding of reproductive choices.

There are a whole series of design issues that need addressing for these specific studies. What is the appropriate primary unit of observation: woman, child, or dyad? How do we follow multiple family members and trace changing circumstances of increasingly complex parenting? At what levels or groups do we need to obtain information on interpersonal or structural contexts? How do we avoid sample selection and bias in genetically informed designs (e.g. adoption studies, sibling studies, twin studies etc)?

Not least among the challenges will be improving (and borrowing from or working with a range of disciplines') analytic methods. The challenge of separating choice (or self-selection) from structures and constraints (or social causation) is an ongoing one (Caspi 2004). Dealing with endogeneity (perhaps an intimate and key part of the process that cannot simply be controlled away), path-dependence and 'life-packages' is difficult. Better conceptualisation and specification of levels of aggregation and of interplays across these levels (both external to and within the individual) is evidently also required. The specification and interpretation of interactions and interplays is also difficult (e.g. separating passive, active and evocative gene-environment correlations (Plomin 1994) and gene-environment interactions (Rutter and Silberg 2002)). Prospective or longitudinal studies are an essential component of this endeavour, but the challenges of imaginative and informed uses of such information are substantial (see the very illuminating discussion by Rutter 1994)

We also need to theorise more. The agenda of disentangling the plethora of factors discussed in this article is truly daunting and cannot be progressed without judicious simplification. This requires careful evaluation of available evidence and some innovative and speculative exploration of a variety of potential pathways and processes, mainly through empirical research but sometimes also through agent-based modelling approaches (Billari and Prskawetz 2003). As with most scientific endeavours, it will prove essential to enable several groups to explore the same theme (why do we move or partner or become parents or even more specific) so as to discover what really matters, but also to ensure that such large-scale investments are funded as wisely as is possible for innovative research.

The scale of investment in the human genome project is needed for a human phenome project too. There is a real need to bring together talented interdisciplinary teams working on the big issues of human behaviour: demographic behaviour is undoubtedly among these big issues, since both survival and reproduction (and the search for ecological niches) are essential elements of evolution (so biologists should need little convincing) and policy makers in both developed and developing countries are all too aware of the consequences of human population movement and reproduction.

I am all too aware that the agenda outlined in this paper is a daunting one, which has profound implications for the way we teach and research. I am also acutely aware that the broad, but still somewhat diffuse, agenda outlined here requires refining into proposals for specific projects. There is an evident need for ethical considerations to be part of this programme. However, I passionately believe that it is essential for population studies to make a major (paradigm) shift to becoming an integrative science of human demographic behaviour, engaging along the lines outlined here.