

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

Short title: Population Paradigms

**John Hobcraft
University of York**

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Direct correspondence to:

John Hobcraft
Department of Social Policy and Social Work
University of York,
Heslington, York
United Kingdom, YO10 5DD.

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ABSTRACT

Much demography is concerned with answering questions about when or what rather than about why. We need to focus explicitly on processes. We should study becoming a parent or parenthood, not births; partnership processes not marriages; health and well-being, not deaths.

The other key shift of emphasis is to within-individual pathways. We must engage with the biological sciences and learn from the approach of psychology. The interplays of genes and their expression with the environment and of the neuroendocrine system with behaviour will prove essential in understanding demographic behaviour.

Demography can transform to a generic social and behavioural science linked to the biological sciences. We should emphasise frameworks and theories that can illuminate the processes and pathways involved in demographic behaviour.

KEYWORDS: genetics, neuroscience, contexts, pathways, processes, theory, paradigms, dynamics, new directions.

1. Prologue

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.

2. Processes *not* Events

Most of my concerns in this article will be with individual-level behaviour, since I passionately share the perspectives of Blalock and Wilken (1979) or of Coleman (1990) that human behaviour involves individuals making choices (or not) influenced by their own experiences and legacies and by interplay with other individuals, collectives, and institutions. However, I shall elaborate reasons why it is no longer appropriate (if indeed it ever was) to root understanding of human behaviour almost exclusively in a rational choice framework.

But let us begin with a simple illustration of processes versus events at the macro-level. Many commentators have deemed the outcome of the International Conference on Population and Development (ICPD) at Cairo in 1994 as a paradigm shift towards a rights-based reproductive health perspective. In some real sense Cairo was an important event or marker, but few would dispute the interpretation that it was a significant part of an ongoing process, with roots in the emergence of the women's movement and in the response to the funding crisis for family planning of the 1980s, and with the battles between the proponents and opponents of the rights-based reproductive health approach continuing today.

I now turn to a brief examination of why we should be concentrating on processes and not just events for all research into understanding demographic behaviour. One goal is to broaden the terrain that we consider, so that we can truly begin to address both the determinants and consequences of demographic behaviour. I consciously take issue with narrow conceptions of demography as being solely concerned with births, deaths and migrations, since population size is often regarded as all that really matters. Such a view

often sees marriage or partnership as only being of relevance in the context of fertility. A narrow concentration on events has certain analytic advantages, including ease of measurement, convenience for analysis, and clarity of focus, but demography has reached the limits of such essentially descriptive analysis and now needs to raise the bar towards dealing with understanding of dynamic processes.

Although events such as marriages, births, or deaths are of importance in their own right and often have important symbolic meanings, it is imperative that we recognize much more clearly that these markers are by no means the only elements in changes of state or status that are of profound importance for demographic behaviour, or have widely differing consequences. The implications of parenthood alter with family size, ages of the child, and in relation to a whole host of other factors. The shift to cohabitation or the fuzzier shifts in time allocation, degree of autonomy, or levels of attachment in a partnership are all of consequence both for partnership breakdown and for choices about entry into parenthood or for child-rearing arrangements. Subtle shifts in health and well-being status have important ramifications for living arrangements, employment, transfers of time and money, and many other aspects of life. The process of deciding (or not) to change place of residence (or job) is complex and involves many steps (or events) along the way, but also interplays with many other important processes, both demographic and other.

Recognition of these complexities not only poses formidable analytic challenges, but also makes it much more necessary that we think more clearly about what are likely to be the important factors involved in determining demographic behaviour or its consequences. This requires greater conceptual clarity than is usual. We need to spend much more time thinking through what may be the important connections across a wide range of disciplinary perspectives and to pay much greater attention to issues of 'causal ordering' or mediating routes through the many elements involved. This involves clarification of what may be the proximate real determinants, what may be the intermediate prior antecedents, and so on backwards to possible 'ultimate' causes.

To give but one example in the realm of parenthood and its relationship to values, attitudes, and ideas, it would seem highly plausible that those explicitly relating to parenthood would be the most likely to be the most directly influential; a step backwards might look at channels through world views or religiosity, and then there are more remote questions as to how globalisation (or genes or evolution or neuroendocrine systems) might act through these (and other) mediating routes. All are legitimate research questions, but we have to begin to make much greater efforts to track through such chains of influence. I believe that headway led by demographers will necessarily begin with the proximate real determinants and gradually trace routes backwards (Hobcraft 2003).

But tracing the routes forwards is an equally important enterprise, perhaps led by those in other disciplines, but where demographers should have important and influential inputs. Even where we do not exert this influence, we nevertheless need to monitor and absorb results from elsewhere and be equipped to critique and inform those who over interpret their findings. It is interesting that a psychologist and a demographer quite independently

chose the example of differing propensities for risk-taking behaviour as an illustrative example of potentially important gene-brain-behaviour pathways for aspects of human reproductive behaviour (Rutter 2003 and Hobcraft 2003). Both emphasised the multiple and reinforcing effects that could arise for reproduction through the life-course.

Moreover, I shall argue that improvements in knowledge, measurement, and understanding have increasingly made clear that most 'events' are relatively fuzzy and very often part of more prolonged processes. One illustrative example is the study of leaving the parental home, which began to receive serious attention during the 1980s (Grebenik, Höhn and Mackensen 1989). It is clear that, for many families, the departure of children is a very protracted process with complex and repeated departures and returns.

2.1 Partnership and intimacy *not marriages and divorces*

Demographers working on developed societies have been forced to abandon their focus on formal marriage because of the rapid changes in partnership behaviour over the past few decades. Our roots in civil or parish registration and simplistic questions in censuses and surveys inhibited understanding. Those working on developing countries that lacked registration systems had long been more aware of consensual unions and those working in the Caribbean in particular had been aware of long-term visiting unions. Developed country demographers have relabelled consensual unions as cohabitation and visiting unions as 'living apart together' (LAT) or 'romantically involved' relationships respectively. The meaning and nature of marriage has altered quite radically in much of the world and is beginning to change in other areas.

But entry into partnerships has always been a process, with many steps along the way. The locus of control in the process has often shifted away from parents, other relatives and community leaders to the individuals concerned, but identification and sifting of potentially suitable partners is an intrinsic part of the process, even where the couple first meet at the time of the marriage. There are almost always clearly identifiable transitions along the way, although the sequencing of attachment, sexual intercourse, living together and any formal betrothal or marriage can vary significantly, with the latter elements sometimes being optional. Yet these sequences surely have major implications for the nature of the relationship. Contrast an arranged marriage, when the partners first meet at the marriage and the bride moves to a distant village away from her friends and family, with one where the couple have formed an ongoing attachment and sexual intimacy before cohabiting and eventually decide to marry. Marriage in these two contexts does not even have the same symbolic meaning, let alone being the same key event for intimacy and reproduction. The partnership context of parenthood in this sequence can also vary significantly and the desire to become a parent may play an important part in decisions to cohabit or marry. Societal and gender structures clearly deeply affect the nature of these processes and also partly determine the consequences of marriage for the individuals concerned.

Many demographers are only interested in marriage because of its association with reproduction. Yet marital status has surely always been a poor proxy for frequency of

sexual intercourse, which is the behavioural proximate determinant of fertility partially captured by this status. Evidence from western societies where partners have chosen each other and are often romantically involved suggests quite rapid declines in frequency of sexual intercourse with duration of marriage. I am unaware of similar studies for arranged marriages, but would be surprised if the patterns were the same.

Decisions about reproduction are also deeply bound up with the nature of the relationship, which evolves over time. The shy young bride who meets her husband at the wedding and then moves in with his family or into his village gradually adapts and adopts new behaviour. A generation later she will have become the mother-in-law who may control the destiny of her son's new bride. In between a wide range of experiences ranging from possible domestic violence to attachment and intimacy, possible emerging autonomy, success in childbearing and childrearing may all have played a part in changing the nature and meaning of the relationship, as will employment, land tenure and a range of external forces. Although much of the negotiation process may take place before cohabitation or marriage, similar evolution of the nature of intimate relationships takes places for the modern Western couple.

Our emotions play an important part in any long-term relationship. A 'good' partnership can successfully meet many of our basic needs for sex, for nurture, and for intimacy (Panksepp, 1998). On the other hand intimate partnerships are too often associated with emotions of fear, disgust, or anger (Fiske 2004). Demographers need to engage with neuroscience and gain a better understanding of the role of emotions in relationships (see also Massey 2002). Moreover, we need to pay attention to some emergent suggestions that pair-bonding and love generate lasting changes in brain structure (Young 2003). In other words, the key importance of feedback loops in relationship formation and breakdown need to be included in our consideration.

Partnership breakdown is also clearly a process, partially recognized by the distinction often made between separation and divorce. The gradual deterioration of a relationship can involve many elements, including sudden shocks such as unfaithfulness, but also often has lasting effects for all individuals involved, differentially affecting the emotional, physical and socio-economic well-being of both partners and of children. The opportunities and constraints regarding partnership breakdown also need to be incorporated in our understanding. How far do the options for dealing with a deteriorating relationship include all of exit, voice, and loyalty (see Rusbult *et al* 1991 for an adaptation of Hirschman's (1970) framework in the context of relationships)? What is the balance of these options and the consequences for the man and usually more significantly the woman?

Since the pioneering work of McLanahan and Bumpass (1988) and Kiernan (1992) on the consequences of divorce for children, the recognition of the pre-divorce stresses for children of a decaying partnership has been evidence of attention to process. But partnerships break down in a variety of ways, ranging from acrimonious and possibly violent disputes following which the partners hate each other, want no contact, and use child custody as a weapon, to those where the decision to part is harmonious, and

friendship, frequent contact between partners and shared childrearing are maintained. The ramifications of the partnership breakdown for all involved, perhaps most importantly for childrearing, are a key concern.

One of the key features of partnerships that demographers and other disciplines have failed to address properly is their dyadic nature. Two key actors are involved and both bring legacies of their inheritance, upbringing and past circumstances and behaviour to the relationship. Yet all too often it is only characteristics of one partner that are examined. I return to this issue later in the paper. A good example of a serious attempt to provide a framework covering the diversity of genetic, evolutionary, neural, and psychological pathways and processes involved in partnership formation is given by Miller and Rodgers (2001; also see Spotts *et al* 2004).

2.2 Parenthood *not* births

It is curious how demographers have lost sight of reproductivity, one of the key themes of work in the subject up to the 1950s (e.g. Hajnal 1950). The concentration on births as the key events is dominant. Yet it is surely the case that few individuals or couples choose to have a baby *per se*, although some interpreters would see having a birth as fulfilling a basic need to nurture (e.g. Foster 2003). Rather, they have the goal of producing socialized, healthy and successful or fulfilled adults, which may also involve lineage issues. For parents, even the traditional demographic notion of reproductivity, which merely requires survival, is inadequate. These concerns are also partially captured in the evolutionary notion of fitness, which captures a still too narrow role for establishing the circumstances for successful reproduction. The concern with parental investments in a broad sense also features in discussions of behavioural ecologists or evolutionary psychologists (e.g. Worthman 2003). In economics some of these concerns are encapsulated in the notion of quality-quantity tradeoffs (Becker 1960; for a recent review linked to evolutionary biology, see Lam 2003).

But, as we have argued at greater length elsewhere (Hobcraft and Kiernan 1995; Hobcraft 2001; Hobcraft 2003), this notion of becoming a parent and the long-term nature of the investment should deeply affect the way we view and analyze decisions about childbearing. In particular, we have argued that parents who make choices need to make judgements about their prospects for the next twenty years or so, both at the individual and societal level, and that changes in these mid-term security prospects have played a significant part in fertility trends in the developed world (Hobcraft and Kiernan 1995; Hobcraft 1996). The mid-term security factors identified include shifts in partnership stability, in welfare smoothing over the life-course and child services, in income and employment security, housing circumstances, in work-life balance, money-time trade-offs, and gender relations. The contribution of changes in security to understanding the recent collapse in fertility in Central and Eastern Europe is fairly evident, although the relative balance is still unclear.

In so far as parents can successfully anticipate their futures on these elements, there are some interesting issues of potential reverse causality involved in the decision process.

Moreover, there is a need for demography to come to terms with the issues involved in rearing and nurturing ‘successful’ citizens as part of our realm of study. At the very least, these are among the key consequences of the narrow demographic behaviour of the event of a birth. As such, they form a legitimate and important focus for our attention, though we too often neglect consequences of demographic behaviour at the micro-level. Recognition of the potential importance of the fluidity of partnership contexts early in the life of the child has recently become a focus of attention both in the US and in the UK (Sigle-Rushton and McLanahan 2002; Kiernan and Smith 2003).

Of course, the process of becoming and of being a parent is also bound up with a series of legacies of the past for both parents and is taken under constraints of reproductive biology, personality and emotions, genes, the means of control over reproduction, ideas, and interpersonal and institutional contexts.

2.3 Potential or Well-Being *not death*

We are all familiar with the WHO definition of health as being a state of complete physical, mental, and social well-being and not just the avoidance of disease or death. Yet much demographic analysis, using our staple tools of life-tables and survival analysis, is too often concerned just with death as an event. However, thanks in part to the influence of the US National Institute for Aging, demographic research on ageing has moved a considerable way to adopting an approach that focuses more broadly (e.g. Wise 2001 and several earlier volumes), though there is still an undue emphasis on disease and morbidity rather than positive health (but see Singer and Ryff 2001).

The broad health agenda, involving the need to increase cooperation across disciplines ranging from molecular genetics through neuroscience, psychology, public health, and medicine through to the social sciences is well encapsulated in Singer and Ryff (2001). This broader theme is one to which I return later in the paper. However, in an influential earlier paper, Ryff and Singer (1998) put forward a persuasive case that resonates with my position in this section (my italics):

‘Positive human health is best construed as a *multidimensional dynamic process rather than a discrete end state*. That is, human well-being is ultimately an issue of engagement in living, involving expression of a broad range of human potentialities: intellectual, social, emotional, and physical.’ (p.2)

We need to learn from and engage in such research. In particular, demographers seem not to have linked into nor learnt from research on hedonism and eudaimonism (see Ryan and Deci, 2001 for an excellent and wide-ranging review). Briefly, hedonism is concerned with happiness or subjective well-being (see Diener *et al* 1999), whilst eudaimonism is concerned with broader fulfilment of potential. Well-being and mental health are implicated among the pathways to physical health (Singer and Ryff 2001). Happiness has been a serious research issue for economists recently and they have been especially concerned that subjective well-being and income are not well correlated, especially among richer societies (Kahneman 1999; Frey and Stutzer 2002; Layard 2002). But there

is research to show that subjective well-being is strongly related to, and probably a consequence of, demographic behaviour: divorce matters as much as unemployment for short to mid-term unhappiness (Lucas *et al* 2003). In a broader context, these concerns with positive health link to a wide range of recent agendas, including a human development approach (UNDP annual), poverty alleviation and health (World Bank 2000), social exclusion as a concept in Europe and elsewhere, and a capabilities and functionings approach to development (Sen 1993). Ultimately living is about life not death.

2.4 Position *not* migrations

The great majority of changes of residence are bound up with other life processes, whether enforced through a build up of persecution, repeated crop failures, imprisonment, or failure to pay rent or mortgage, or chosen through change of or search for job, search for better school catchment area, changed health status, or on marriage. Location *per se* is rarely the prime motivation factor, though amenities or facilities can be important. But migration is part of a class of processes that are bound up with position in society. This includes not just residential stratification, but job and social hierarchies too. It is no accident that much of the pioneering work on social or occupational mobility was done by demographers (Hogben 1938 part II, Glass 1954, Blau and Duncan 1967), though this perception is possibly blurred by the heavy overlap of demographers with sociology departments, particularly in the US.

Evolutionary demographers also make much of the role of status and hierarchy in mating and reproductive behaviour. Just as the study of partnership processes is hampered by lack of serious treatment of the dyad, much study of mobility is hampered by inadequate attention to both origins and destinations, though simple push-pull interpretations have long partially recognized this issue (Ravenstein 1885).

The decision to move involves an ongoing evaluation of alternatives, though this is not likely to be a continuous preoccupation. The evaluation of the current position (job, housing, location, friendship network, etc) is almost inevitably better informed (by experience) than can be the evaluation of (multiple) alternative positions (except for returnees). Thus, there is usually an informational asymmetry involved and we ought therefore to pay greater attention to the perceptual and risk-taking aspects of such behaviour. How did pioneer migrants from Europe to the US or Australia evaluate the benefits of such a move? They would have had much greater certainty about possible push-factors (e.g. the Irish potato famine) than about the trials and tribulations and gains that awaited them if they survived the journey. This kind of informational asymmetry affects almost all choices about position (and many other demographic choices too, e.g. partnership and parenthood).

How do we deal with this in modelling and understanding the positional behaviour of individuals (and some collectives)? What can we learn from psychology and other relevant disciplines about the nature of such decision-making? For example, how relevant is the research on prospect theory that suggests that decision-making is reference-

dependent (and thus incompatible with expected utility theory) and risk-averse concerning gains, but risk-seeking for losses (see Kahneman 2002 for a summary)?

2.5 Personal ties: Family, Kinship, Intergenerational and Friendship links *not household structure*

Co-residence in households is of considerable importance for a reasonable fraction of (reciprocal) transfers of time, money, and emotional support and can have particular ramifications for nurture of children. However, key aspects of demographic behaviour can be just as affected by such transfers across household boundaries, through individual transactions, public and private provision of welfare and child services, and tax and benefit systems. We cannot hope to understand the nature of demographic choice properly without looking at this wider connectedness.

These interlinkages are increasingly complex and important with rising partnership fragility, especially for any children involved. It is no longer implausible to examine experiences of children who are connected to up to eight grandparents or step-grandparents (or occasionally even more). Step-siblings and half-siblings have also become fairly commonplace. Vastly different arrangements of time, money, and nurture can be involved.

But similar points can be made in the context of transfers in old age. There is some demographic research and much more theorising on an old-age security rationale for childbearing in traditional societies and on the risks involved for those, especially widows, with no surviving (and often co-resident) children (e.g. Cain 1986 and the useful review by Das Gupta 1993). But this ignores the lifetime or post-marriage networks of reciprocal kinship and friendship networks that exist in any long-standing (rural) community. Transfers of care, emotional support, food and other resources across household boundaries are normative and yet somehow ignored in much demographic and evolutionary theorising on these issues. If you don't look, you do not find such links. They are rarely even discussed or contemplated, let alone explored.

In developed societies too, inter-household transfers, often reciprocal, play a large part in people's support networks and systems. This connectedness is increasingly being recognised and incorporated in survey instruments. There are important questions as to what differences in the balance between intra-household, intra-family, other interpersonal, privately purchased, and public provision of care, services, and money make to childbearing decisions, partnership stability, well-being, and position. Moreover, it is possible that the source of such support is of secondary importance to its availability and accessibility.

3. Alleles, Brains, and Context: The ABC of Demographic Behaviour.

In this section I firstly consider some of the insights and questions that arise from consideration of the genotype, the brain, mind, and endocrine system, and other biological or physiological aspects of the progression of the phenotype. Secondly, I

consider the wide range of contexts that exert external influences on the person. It is essential to make clear that the apparent separation of pathways within the person from the processes outside the person is an expositional artefact. Much of what I have to say will emphasise the growing awareness of the critical importance of interplays between the person or phenotype and external or 'environmental' influences and of feedbacks, interactions and correlations. Gene expression is often determined by such feedback, frequently from the external environment. Evidence is beginning to accumulate that specific variations (polymorphisms) in alleles substantially condition sensitivity to environmental stress (see Caspi *et al* 2002 and 2003; and, for a useful and readable summary, Ridley 2002). There is also growing evidence that external stimuli evoke endocrine responses that can bring about lasting changes in brain structure. The deep and unresolved issues concerning the interplays of free will and instinct in cognitive processes and some of the work on decision-making heuristics will also feature. I shall have least to say concerning reproductive biology, because this is the area already most familiar to many demographers.

3.1 Genetics and demographic behaviour

There has been a flurry of recent work using behavioural-genetic models to explore the extent to which genetics play a part in various aspects of demographic behaviour. Almost all of this work relies on quantitative genetic (or ACE) models, using designs such as twin or adoption studies that can in principle separate the sources of variability in behaviour into the Additive genetic component (A), a Common or shared environment component (C), and a non-shared Environment element (E) (see DeFries *et al* 200?, Rutter *et al* 2001 and Rutter 2003). This literature is most extensive in relation to mental health, especially psychopathologies. I have reviewed the studies that relate to fertility, divorce, and age at first intercourse at some length elsewhere (Hobcraft 2003), so my discussion here will pick up on some of the major underlying issues.

Those who are sceptical about the role of genes can only make a powerful case for the lack of genetic influences through studies that explicitly include a genetically-sensitive design (Rutter *et al* 2001). Moreover, it is only possible to demonstrate 'environmental' effects on behaviour through such designs, since the dual inheritance aspect of genetic and shared family environment otherwise confounds the two. Some ingenious work is now taking place using identical twins to demonstrate unequivocal 'environmental' effects (see Caspi 2004, for example).

However, the behaviour-genetic models do have a number of limitations. Firstly, the genetic component almost always includes all gene-environment interactions, which may overstate the direct role of genes, possibly substantially. The more homogeneous the population the smaller will be the 'environmental' elements and thus the higher the measured heritability. Gene-environment interactions complicate this further (Rutter and Silberg 2002). The 'shared' environment is essentially the role of common experiences from growing up in the same family. There is a large literature concerned with the evidence that this fraction of the variation is small (Plomin and Daniels 1987; Maccoby

2000; Plomin, Asbury and Dunn 2001). Concomitantly, the share of variability attributed to non-shared environment is often quite large, though this includes any residual unexplained variance, which is not always acknowledged (see Turkheimer and Waldron 2000). The models depend quite heavily on an assumption that children reared in the same family experience equal environments and there are serious issues concerning twin studies (Rutter 2003). The assumption often made is that identical twins may have more similar environments because they are genetically and physically indistinguishable; this would lead to an understatement of the genetic component. I am not aware of any serious discussion of the extremely plausible idea that twins, and especially identical twins, actually try much harder than other siblings to differentiate themselves and thus to shape their environments to be more different (for an interesting and relevant treatment of this issue in the context of IQ scores see Feldman *et al* 2000). Moreover, for twins reared apart and for adoption studies, there is usually very little serious attention to the issues of sample selection through selective placement to higher status environments. One recent study estimates that this selective placement in homogenous environments could account for genetic sources of variance in adoption studies being overstated by about a third compared with the general population (Stoolmiller 1999).

Although there are a number of reasons for supposing that direct genetic heritability could be overstated it is exceedingly unlikely to be of negligible importance for almost any human behaviour examined to date. However, as discussed by Hobcraft (2003), some of the behaviour-genetic modelling of fertility (Kohler, Rodgers and Christensen 1999, Rodgers, Hughes *et al* 2001, Rodgers, Kohler *et al* 2001, and Kohler and Rodgers 2003) and of divorce (McGue and Lykken 1992, and O'Connor *et al* 2000) raises at least as many questions as it answers and is not always convincing (see also Morgan and Berkowitz King 2001 and the comments by Capron and Vetta and by Kohler).

The real issues are to identify the pathways through which genes work or interplay with the 'environment', rather than using 'black box' models that simply partition the variance. The advances in molecular biology make this possible to explore, although such exploration can be very complex and time-consuming (Risch 2000; Plomin and Crabbe 2000). It is likely that the role of genes in most complex behaviours that are not pathological will result from an accumulation of small effects over several genes, rather than few genes with a large effect (Plomin *et al* 2003). Increasingly the availability of microarrays or 'gene chips' (Lockhart and Barlow 2001, Carpenter and Sabatini 2004) and new methods of analysis (Butte 2002) make the search for multiple genetic markers (or Quantitative Trait Loci) faster and more plausible.

One study has now shown an association between multiple marriages and the 7-repeat allele of the dopamine D₄ receptor gene (Rowe 2003), which is also linked in some studies to ADHD (attention deficit hyperactivity disorder, see Faraone *et al* 2001 and Thapar 2003) and to risk-taking behaviour. Other studies have begun to explore linkages of the dopamine D₂ receptor gene and low fertility for men (e.g. MacMurray *et al* 2003) and of the risk allele for G6PD-deficiency in females to poor intrauterine growth and possible lower fecundity (e.g. Gloria-Bottini *et al* 2003).

Several issues hinder this search process, although significant progress is being made. Firstly, most such searches treat the effects associated with the various markers as being additive (the broader general linear model without interactions that also plagues much social and behavioural research). At the moment sample sizes and statistical procedures can identify individual markers associated with about one per cent of variance (as good as many of our own predictors) but it is anticipated that many genes will account for smaller components of variation (see Plomin *et al* 2003 and Plomin 2003). Secondly, there is emerging evidence for the importance of gene-gene interactions (epistasis) in many contexts, including the link of the APOE gene (that demographers are familiar with in the context of Alzheimer's disease see Ewbank 2000) to coronary artery disease (see Grigorenko 2003). There is also accumulating evidence for epistatic interactions without identifiable 'main' effects of the individual genes (Grigorenko 2003). Thirdly, gene expression is often triggered by events external to the phenotype and there is much concern about the importance of gene-environment interactions and correlations (Rutter and Silberg 2002 on emotional and behavioural disturbance; Rowe *et al* 1999 and Turkheimer *et al* 2003 on IQ; Reiss *et al* 2000 on adolescent development). Most genetic screening efforts do not have good measures of these external factors (strictly external to the genome, but usually in this context external to the phenome or person). Perhaps the most compelling evidence to date for gene-environment interactions at the molecular level comes from the Dunedin study: one showing that a genotype conferring high levels of expression of the neurotransmitter-metabolizing enzyme monomamine oxidase A (MAOA) appears to moderate the effects of child maltreatment on the subsequent development of antisocial behaviour (Caspi *et al* 2002); and a further study showing that individuals with one or two copies of the short allele of the 5-HTT promoter polymorphism (in the serotonin transporter region) are more likely to show depressive symptoms, diagnosable depression, and suicidality following experience of stressful life events than those homozygous for the long allele (Caspi *et al* 2003). The latter finding was both guided by the role of serotonin reuptake-inhibitor drugs in treating depression (providing a 'candidate gene') and is more plausible as a consequence of this known link. Of course, as Caspi and his collaborators readily acknowledge, these findings require replication.

Demographers potentially have much to contribute in this field of teasing out or replicating molecular biological gene-environment interactions in representative prospective population samples that collect DNA (unlike behaviour, experience, or attitudes where retrospective information is often difficult or impossible to collect, the invariant nature of DNA permits collection at any point in time).

3.2 Brain, mind, and endocrine systems

In addition to engaging with behavioural genetics I also want to argue that demographers have much to gain from knowledge of and interaction with neuroscience and with cognitive psychology. Demographic behaviour usually involves choices and our understanding of choice processes has to be fatally incomplete if we ignore developments in these fields.

Valuable insights can come from non-biological work on decision heuristics (Kahneman 2002; Brocas and Carrillo 2003; Gigerenzer *et al* 1999, especially the chapters on mate search and on parental investment; and Billari and Prskawetz 2003) as to how choices may be made using simple decision heuristics rather than full or bounded rational choice. Such work, for example Kahneman (2002 for a summary) on differences in being risk-averse for gains but risk-seeking for losses may give us useful insights into the asymmetric decision processes involved in partner choice, becoming a parent, or changing position. Moreover the recent concerns of psychology and economics with hedonic happiness or subjective well-being (Kahneman Diener and Schwartz 1999, and Frey and Stutzer 2002) and how it relates to the economic concept of utility are clearly relevant to our agenda on potential or positive health, although eudaimonic well-being is closer to this agenda (Brocas and Carrillo 2003, Ryan and Deci 2001, Ryff and Singer 1998)

But we may also be able to gain purchase on mating and reproductive behaviour from affective neuroscience (Davidson, Scherer and Hill Goldsmith 2003; and Panksepp 1998, especially the chapters on ‘the varieties of love and lust: neural control of sexuality’ and on ‘love and the social bond: the sources of nurturance and maternal behaviour’). Since much of the research in neuroscience is based on animal studies we also need to take account of the trade-offs between innate systems and ‘free-will’ and the likelihood that these interact and feed back in complex ways through brain structure (e.g. links between the amygdala and the pre-frontal cortex). Evidence is also accumulating for lasting synaptic changes that result from stimuli, including emotions, from outside the person and that brain plasticity continues well into old age (e.g. LeDoux 2002, who makes the bold claim that ‘we are our synapses’; and Stern and Carstensen 2000).

The aspects of brain structure that demographers are most familiar with are those linked to sexual dimorphism. Udry’s (1994) controversial PAA Presidential address or his even more controversial article (Udry 2000) and the heated exchanges that resulted in the *American Sociological Review*, are an attempt to produce a biosocial theory of gender. Sexual dimorphism also plays a significant role in much evolutionary theory on partnership and reproductive strategies (Low 2000; Diamond 1997) and more implicitly in economic theory on fertility (Becker 1991, Lam 2003). Key to the emergence of sexual dimorphism is the role of testosterone, mediated by aromatase to create estrogen that masculinizes the brain and mediated by 5-alpha-reductase to create dihydrotestosterone that masculinizes the body (Panksepp 1998; Schulkin 1999). Some neuroscientists who work solely on animal studies forget that human populations (and other primates) have much more developed frontal lobes and are thus much more likely to modify innate tendencies through exerting control (having ‘free will’). This can be especially true in attempts to understand reproduction. There is little doubt that sexual dimorphism can result in exquisite mechanisms that relate sex and reproduction (Cameron 2003, also Simerly 2002 who entitles his review ‘wired for reproduction’), but there seems to be more evidence that humans (and other animals) have inbuilt desires or urges to have sex than to reproduce *per se* (for several takes on this see Hobcraft and Kiernan 1995, Foster 2003, Worthman 2003).

Some evidence is emerging from animal studies, especially from contrasts of monogamous prairie voles with promiscuous montane voles (Young 2003; Panksepp 1998 chapter 12) about the neuroendocrine bases of monogamy (Young, Wang and Insel 2002) and pair bonding (Curtis and Wang 2003). For female prairie voles oxytocin released during copulation acts on the oxytocin receptors in the brain to induce lasting pair bonding and key elements of parental behaviour. For male prairie voles the release of vasopressin during orgasm similarly acts on vasopressin receptors to produce lasting bonding and parental behaviour. These results have been fairly thoroughly substantiated through a variety of techniques. Differences among a wider range of vole species in social organization have also been linked to differences in patterns of brain receptor binding of oxytocin and vasopressin. Other studies of rats, sheep, and hamsters also suggest important roles for oxytocin and vasopressin. Thus there seem to be good grounds for studies of human populations to explore the genetic markers that lead to the expression of oxytocin and vasopressin receptors in the prelimbic cortex and nucleus accumbens, regions that are also involved in the mesolimbic dopamine reward pathway, suggesting a further role for dopamine D₂ receptor markers (taking Young, Wang and Insel 2002 further than they may wish to go). This is predicated on it being easier to explore these issues for human populations through the genetic markers that code for the receptors, rather than through direct brain observation by fMRI (and certainly not through the invasive approaches used in animal studies). Differences in relevant alleles for males and females encoding for these receptors would thus be ‘candidate genes’ for the study of sexuality, bonding, partnership breakdown, and parenting. For the human male sexual arousal leads to a short term peak in blood plasma levels of vasopressin, whilst ejaculation is associated with a short term increase in plasma arginine-vasopressin (Murphy *et al* 1987, as adapted in Panksepp 1998). I have not found evidence showing that these bind to relevant receptors in the brain to promote bonding in humans, though it may exist.

Other tantalising evidence is beginning to emerge on the opioid reward systems in the brain and social attachments. Reward systems are likely to be important in feedback mechanisms that establish lasting synaptic responses to a partner or child involved in long term nurturance or pair bonding. One recent study (Bartels and Zeki 2000) examined brain responses of 17 subjects who were ‘deeply in love’ to photographs of their love and of other friends of the same sex using fMRI and found that differential increased brain activity was, perhaps surprisingly, restricted to very few areas, but seemed to be unique compared with other responses. Bartel and Zeki conclude:

‘It is however striking that studies of cocaine- and mu-opioid agonist-induced euphoria have shown increased activity in foci that seem to overlap with all foci activated in our study: the anterior cingulate cortex, the insula, the caudate nucleus and the putamen. This suggests a potentially close neural link between romantic love and euphoric states.’ (p.3833).

Perhaps we shall soon see emerging evidence from brain scans of ‘happily’ married long-term couples beyond the first burst of romantic love and of couples undergoing separation or divorce, which may give us greater insights into the neural pathways

involved in attachment and partnership breakdown (see also Diamond 2004 on the distinctions between romantic love and sexual desire).

Further topics where we can engage with neuroscience include the neurobiology of parental behaviour. A useful review of issues concerning hormones, parental care and attachment behaviours is given by Schulkin (1999). Key to that review is an understanding that the hormones prolactin, oxytocin, and vasopressin are also neuropeptides and thus can feedback to bring about changes in synaptic structure necessary for long-term bonding. Broader issues covering brain structure and maternal behaviour, postpartum depression, child abuse and neglect, and intergenerational continuities in abnormal maternal behaviour are discussed in the final chapter of Numan and Insel (2003), although their knowledge of social science and psychology literature on these issues seems patchy and dated (as no doubt would my knowledge of brain structure to them, one of the problems of trying to engage across disciplinary boundaries).

One other key theme of relevance to sexuality, partnership behaviour, personal ties or connectedness, and parenthood is the apparent need to nurture (in this context see Foster 2002 and Hobcraft 2003; see also Taylor 2002 and Panksepp 1998), although the reciprocity involved in such interplays is perhaps better captured by the ‘core social motive’ of ‘belonging’ (identified by Fiske 2004 as having primacy among her five core social motives for behaviour, the others being understanding, controlling, enhancing self, and trusting). This reciprocity and the social aspects of behaviour require that we shift attention to the other side of the nature via nurture or person-situation interlinkages. We emphasize again that this apparent separation of nature and nurture is illusory and can be deeply misleading.

3.3 Context: other persons

Demographic behaviour is evidently deeply bound up with the person or phenotype interacting with other persons. Children are born into families and are reared by parents and other adults. Partnership or marriage, reproduction, and sexual activity quintessentially involve interactions with other persons. Beyond these clear and directly relevant interpersonal demographic linkages, there are many other potentially important contexts where the interplay with other persons matters and may shape the values, attitudes, health, and behaviour of the phenotype and generate feedbacks within the person, through synapses or gene-environment interplays. These important social contexts include family and kinship networks, peer groups, friendship or support networks, care and service providers, employers and workmates, local community, and wide range of civil society institutions. Yet much research on demographic behaviour does not address these issues.

Let us begin with the partnership dyad. Sex, cohabitation, marriage, partnership breakdown, and decisions about childbearing all involve both members of the dyad and inevitably involve interplays and behavioural adjustments. Both partners bring their phenotype at the time of relationship formation to the union. Yet we typically know and study only the most superficial aspects of these characteristics, paying little attention to

diverse and possibly important issues such as: genetic predispositions, including the major histocompatibility complex and possible links through pheromones (e.g. Jacob *et al* 2002; and comments by Wedekind 2002 and reply by McLintock *et al* 2002; and from an evolutionary perspective Thornhill and Gangestad 2003), previous partnership experiences (not just occurrence), personality traits, peer and family pressures, etc.

Analyses of divorce or partnership breakdown that simply examine the background of one partner (e.g. whether they experienced parental divorce, or their age at entry into the union) clearly omit all of the background of the other partner and the intimate and lifecourse interplays involved during the partnership. This applies equally to the extant attempts to examine the potential roles of genes and behaviour in divorce. It is rare indeed to find careful studies that begin to explore these aspects, though see Robins, Caspi, and Moffitt (2000) on how personality traits of both partners are linked to relationship quality and Jaffee *et al* (2003) on an interesting interaction of divorce consequences with the father's antisocial behaviour. Relationships evolve over time and much of the changing character will be attributable to the joint experiences and interpersonal interplays of the two actors: this can involve shifting power relations, patterns of intimacy, consequences of childbearing and parenting, employment or health shocks, and shocks from infidelity, among others.

Similarly, most decisions to bear and rear a child involve two parents: this is a biological or genetic certainty, but not necessarily a behavioural one though predominantly so. Since reproduction is such an essential component of evolution and of demographic behaviour, it is really quite surprising how little attention is paid to the dyadic aspect of childbearing decisions. At the biological level the fecundity of both partners matters and genetic mismatches play a part in inhibiting reproduction. But we need to move well beyond reproductive biology and particularly begin to pay much more attention to how decisions on reproduction are made and what roles genes, brains, structural constraints, and interpersonal relationships play in these decisions. This undoubtedly requires major research projects that involve behavioural geneticists, neuroscientists, psychologists, social scientists, and demographers at the very least. So does the study of partnerships. A key element of progress beyond this necessary multidisciplinary approach will be to find clever ways of designing studies that recognise the dyadic nature of these processes, as well as sifting available evidential clues so as to identify promising paths to progress (for a useful attempt to lay out many of the issues involved for human bonding see Miller and Rodgers 2001).

Family networks, including both kin and partner kin, are also often linked with demographic behaviour, though the pathways are often more diffuse and less well-defined. The influence of parents or of parents-in-law on residence, on partnership formation, particularly for arranged marriages, and on fertility behaviour is claimed more often than carefully documented. The roles of kin and partner kin in childrearing can also be considerable. One of the issues that demographic studies do not address is the relative importance of genes and behaviour in such linkages. We have virtually no evidence, for example, on the extent to which the observed intergenerational linkages in out-of-

wedlock childbearing or in partnership breakdown are transmitted through nature, nurture, or the interplay of both.

One of the broader puzzles to emerge is the evidence from behaviour-genetic studies that suggests common or shared family environment during childhood plays a limited role in subsequent behaviour (Plomin and Daniels 1987, Dunn and Plomin 1990). The proponents of this make quite strong claims that this is evidence that most family influences operate through genes rather than through environment (Plomin, Asbury and Dunn 2001). Moreover they also claim that most 'environmental' components of behaviour operate through nonshared environment, rather than the narrowly defined common childhood family rearing environment. However, despite the seemingly clear results from behaviour-genetic models, there is still lively questioning of these assertions. Twin studies are regarded by some as largely discredited (Rutter 2003). Since all gene-environment interactions are allocated to the genetic component rather than to nurture, the role of family experience is thereby understated (Maccoby 2000). Evidence is beginning to emerge that shows very strong gene-environment interplays for some outcomes (Caspi *et al* 2002 & 2003). Moreover an increasing number of studies of identical twins are beginning to show significant (clearly) non-genetic differences in parenting and in behaviour, and may indicate a broader phenomenon of sibling differentiation, perhaps arising from the siblings themselves reacting to genetic similarities (most strongly for twins and especially MZ twins) to create different environments and responses (a 'reactive' gene-environment correlation – see Plomin 1994 for a useful discussion of the differing types of gene-environment correlations).

It may well prove to be the case that almost all of the genetic variation within families arises from gene-environment interplays and that, perhaps, ten per cent of total variation in behaviour is due to genes *per se*, ten per cent to environment *per se*, and something like 40 per cent to gene-environment interactions: quite a different story from the more usual attribution of about 50 per cent to genes. Attempts to explain the variation due to nonshared environment in behaviour-genetic models have also met with difficulties, with measured variables capturing little – again, gene-environment interplays may prove important (Turkheimer and Waldron 2000; Plomin, Asbury and Dunn 2001; and Plomin *et al* 2003). Whatever the outcome on these debates, it is overwhelming clear that the only routes to understanding the relative importance of gene-environment interplays arises from paying close attention to genetically-informed designs (Rutter *et al* 2001), or as is increasingly likely, having direct measurements of genes and alleles. Simple fixed effects models for siblings, beloved by many economic demographers (e.g. Geronimus and Korenman 1992 and Lopez Turley 2003 on young motherhood, but see Moffitt 2003 for a more cautious view) are inadequate for this purpose and can give seriously misleading results in the presence of interactions between the measured and unmeasured variables (that include shared genes and often childhood experiences too) and often involve other potentially distorting sample-selection problems (Moffitt 2003).

Nonshared environment for siblings or twins arises both during childhood and after they leave the natal family. Interplays with other people occur in schools and some childcare settings, and with peer groups. Later in life these interplays involve workmates and

employers, friendship networks, carers, neighbours, and a potentially broad range of civil society institutions. So far, the limited work on the possible impact of these complex interlinkages on demographic behaviour has been largely limited to estimates of (often poorly-delineated and conceptually vague) community or neighbourhood effects (e.g. early examples by Casterline 1985 and 1987, also Brooks-Gunn, Duncan and Aber 1997). These limited studies have not always proved very effective at eliciting clear neighbourhood or community effects, which have often been disappointingly small for those who think they should be stronger. In a different field, I have come across one example that pays closer attention to a range of these interpersonal effects impact on early adolescent development, looking at the joint influence of neighbourhoods, nuclear families, friendship groups, and schools (Cook *et al* 2002). Interestingly, the individual contexts each appeared to show weak influence, but the joint influence was large.

We have a long way to go in conceptualising and measuring interpersonal contexts and their likely paths of influence onto demographic behaviour. The current uses of multi-level models (clearly one useful tool) suffer from a lack of conceptual clarity about the levels and groups identified and about the likely pathways involved, especially probable differential responses to the group-level influences for individuals with differing characteristics. Moreover, without identifying measures at the higher levels of aggregation (and this may well not be enough) it is not possible to distinguish the influences arising from interpersonal interplays from those arising from structures: is it community or neighbourhood that matters; school environment and teachers or fellow pupils; carers or transfers? Most demographers could benefit from a close reading of two classics on the topic of macro-micro interplays that did pay close attention to conceptual issues and pathways (Blalock and Wilken 1979; Coleman 1990; see also Cacioppo *et al* 2000 in the context of neuroscience).

3.4 Contexts: structures, institutions, opportunities and constraints

There are also a large number of potential influences on demographic behaviour that arise from structures, rather than from interpersonal relationships. These structural elements, too, can operate at quite different levels of aggregation. One evident example, researched in the family planning and health literatures, is the role of provision and accessibility of services (Casterline 1985 & 1987). These services often most directly impinge on the individual's reproductive behaviour or health outcomes at a fairly local level, but are shaped and constrained by issues as diverse as national (and sometimes international or sub-national) policies, political will, supply chains, and management structures.

In the context of health and well-being there are important and largely unanswered questions about how differing health systems and differences in public/ private mixes of health provision matter, about the pathways and mechanisms through which public health measures affect well-being, and about the ways in which these structural features are mediated by individual attributes, including behaviours, socioeconomic status, and genetic predispositions. More broadly, potential is affected by disease ecologies, welfare systems, labour markets (often fairly localized), education and training opportunities, and human rights.

Partnership and parenthood are also influenced by a variety of structures at differing levels of aggregation: education and training compete; housing markets shape and constrain choices; welfare regimes and child service provision affect parenting decisions and choices; gender, class, ethnic, and intergenerational structures exert their impact; normative and cultural pressures and government interference in or support for sexual and reproductive rights and choice all matter too.

Occupational and geographic positions are strongly bound to housing and labour markets and class structures. State policies and welfare regimes also influence such movements.

There are considerable difficulties in demonstrating and teasing apart these structural influences on demographic behaviour. Firstly, as is clear from the brief discussion above, there is a wide range of such elements, each of which may have small impact, but these may cumulate into a much larger influence, partly through interplays. A simple example would be the potential interplay between school hours and meal provision and normal work hours. Secondly, many issues themselves cross-cut: parenting decisions may be highly dependent on child services being available, affordable, and accessible, but may be unaffected by whether such provision is from the public sector, the private sector, or the family. Another illustration comes from the recent series of European governments that are explicitly trying to raise fertility with one-off cash incentives for having babies. These payments are usually quite small compared with the real long-term costs of rearing a child and serious exploration of likely differential responses by income or socioeconomic status, of potential perverse incentives for risk-takers and of unintended consequential burdens for the welfare state is urgently required.

In order to make real progress in documenting and disentangling such complexities we need clearer mid-level theories or frameworks that help to shape and structure data collection and analysis. Moreover, since many of these influences often do not vary much within a single country, we shall have to use clever and fairly descriptive approaches based on comparable information to tease out their relative importance. The UNECE Gender and Generation Programme contains an important contextual component that should enable some progress to be made. But these topics are conceptually and analytically difficult.

3.5 Progression of the person through the life-course

Throughout our discussion so far, we have emphasized that genes, brain, mind, person, other persons, and structures all interplay over time, both within and across these domains. Understanding human behaviour, including demographic behaviour, thus demands attention to multiple dynamic processes and involves many complex feedbacks and interactions (Billari and Prskawetz 2003 illustrate one potentially useful approach to such issues). As we progress in our understanding we shall surely find that a whole series of 'life packages', sequential chains, key precursors, and conditional triggering or moderating events will be involved.

The key actor to be considered as the focus in this study is the individual person or phenotype. Ultimately, a lot of human behaviour involves decisions or choices made by individuals. As so nicely captured by Fiske (2004), these individuals need to belong (and thus interact with other persons), to understand (in order to relate and to make choices more informed), to control (their own lives and perhaps the interplay with others and structures), to enhance themselves, and to trust (other persons and institutions). Individual capacities, traits, and attributes change through their life course; even apparently fixed characteristics such as sex at birth or race change their meaning and are mediated through gender structures or discrimination according to the life-history and temporal and spatial context of the individual concerned. So, at any one time, individuals bear the legacies of their inheritance, endowments, and past experiences and bring these to any current choice or behaviour. Their choices and behaviours are also affected and constrained by those legacies and by their current circumstances.

But the goal of science is to try to put structure on this potentially infinite set of legacies and current attributes and contexts and disentangle what really matters for particular decisions or behaviour. To do so requires sharper conceptualisation and measurement, a multidisciplinary approach, large-scale investment, and innovative analysis and modelling. This is always one of the great challenges and there is no simple recipe for progress, but what is needed is a much greater effort to make progress and some serendipitous or talented individuals to make the bold steps required.

A great deal of emphasis in demographic research is on the current (or at the time of a recent event) characteristics of the person: for example income and other measures of socioeconomic status, housing status, marital status, parity, age. Yet the pathways by which this status was reached can clearly matter too. Does the current socioeconomic status reflect recent (or longer term) upward or downward movements? These directional changes may have profound implications for decisions about housing mobility, partnership coresidence, or becoming a parent. Yet our literature all too rarely deals with the impact of shocks on demographic behaviours. For a noteworthy and thoughtful exception to this see Michael (1988 and the earlier Becker, Landes and Michael 1977).

Occasionally attention is paid to trying to capture more lasting attributes: for example human capital, 'permanent income', or social class (usually really based on current or most recent occupation). The proliferating set of concepts around cumulative 'capital', including wealth, human (Becker 1964), social (Coleman 1988), health (Grossman 1972), and sexual (Michael 2004) capital also reflect a greater attention to the person's life-course. Equally, however imperfect, measures of the 'big five' personality traits (Extraversion, Agreeableness, Conscientiousness, Emotional stability or Neuroticism, and Intellect or Openness to experience, see Costa and McCrae 1992), of IQ or 'general ability' (Plomin 2003), and more recently the concept of emotional intelligence (Goleman 1995) try to capture more lasting characteristics of the person. There is also a huge psychological literature on resiliency, lasting characteristics that can serve to protect individuals from entering disadvantage or can promote easier exit (Rutter 2000, Luthar, Cicchetti and Becker 2000, Luthar 2003). This in turn relates to recent theorising about social policies relating to dealing with social exclusion (Hills 2002 and Hobcraft 2004).

The mediating role of genes and brains in resiliency is also becoming a topic of interest (Rutter 2003a, Kim-Cohen *et al* 2004) and Serbin and Karp (2003 and 2004) provide a useful summary on intergenerational aspects of resilience and vulnerability.

Although all of these constructs, measures and theories are concerned with lasting features of the person, all are malleable. There is ever-growing documentation of brain plasticity, even into old age (Stern and Carstensen 2000, Kolb, Gibb and Robinson 2003, LeDoux 2003); effects of serious disadvantage can be overcome (as shown for Romanian orphans' IQ, Duyme 2004), knowledge and skills can always be accumulated, and shocks of unemployment, bereavement, or partnership breakdown can have short and long-term impacts on well-being and happiness (Lucas *et al* 2003).

4. Alternative paradigms for population research

Demography has always been an interdisciplinary or multidisciplinary programme (e.g. Stycos 1987) and has drawn on a wide range of approaches and tools to address our field of study. The life table, the tool that perhaps distinguished us as a discipline (along with stable population theory, with its origins in biology), had its origins in insurance and is now completely subsumed under survival or event history analysis and is no longer our special domain. So it is clear that population is a field of study, rather than a discipline.

We have always looked to a range of disciplines to provide us with ideas and approaches. In recent years, the dominant paradigm for much demographic (and empirical sociological) research has arguably become economics – for example, two of the three papers commissioned for a recent symposium on causality in population research dwelt entirely on econometric approaches (Moffitt 2003 and Smith 2003). We still draw on sociology and there has been a recent influx of (quasi-) anthropological approaches too (Fricke 2003). Our research on fertility also links to reproductive biology and on mortality and health to epidemiology and the health sciences.

The biggest movement of emphasis and a very clear shift of focus towards multidisciplinary work have come in the field of ageing and health research (Wise 2000, and Singer and Ryff 2001). Here demographers have distinctive insights to offer and have played a very significant part in the new approaches, but also very clearly linked to a much more wide-ranging research agenda drawing on many disciplines (e.g. Singer and Ryff 2001; and relatedly Wachter and Finch 1997 on longevity). Examples of this new approach are the massive multidisciplinary effort that went into the US Health and Retirement Study and the newer English Longitudinal Study of Ageing. Moreover, many existing ageing studies now collect biomarkers, including DNA (Finch, Vaupel and Kinsella 2000). This shift in emphasis to broad ranging interdisciplinary approaches for scientific research is beginning to impact other areas, such as child development (Shonkoff and Phillips 2000) and underlies the recent 'NIH Roadmap' for future research priorities in the USA (Zerhouni 2003). There are also beginnings of this wider approach in relation to research on the biodemography of fertility and family formation (Wachter and Bulatao 2003).

But there is still a long way to go in making demographers aware of, prepared for, and connected to this wider programme of research. I believe that we can learn a great deal from other disciplines that rarely feature among our collaborators or in our training. In looking to other disciplines, I shall emphasise four themes where we can derive real benefits. Firstly, we can learn a great deal from *how other disciplines structure questions and problems*. Secondly, they can provide *alternative theoretical approaches and tools for analysis*. Third, they provide a basis for *new knowledge and findings*. Fourthly, they act as a source of *collaborators and potential recruits for demographic research*.

Inevitably my treatment of alternative disciplinary paradigms will be selective and cannot possibly do full justice in the space available. I shall begin with a brief discussion of some aspects of economics as currently used within demography and briefly point to some promising newer developments linking economics to psychology that may help. I shall then give a fuller discussion of links to psychology, since it is clear from my preceding text that I believe engagement with this discipline is essential for progress in population research. Then I shall look at briefly at biology and epidemiology (see also Burch 2003b for a valuable discussion of links to physics and modelling philosophies within that discipline).

4.1 Economics

The strong theoretical underpinnings of economics give it great leverage and the relative absence of alternative analytic methods for large-scale datasets in other social sciences means that economics risks exerting a too dominant hegemony. If blame has to be attached to this situation, it has to fall on the other social sciences and their failure to develop rigour. However, current econometric practice and the underlying theory are probably acting as a straitjacket to progress in a number of respects.

In particular, a great deal of recent work has been narrowly focussed on the challenging task of trying to assess whether an observed association between (often only) one antecedent and an outcome is causal. This work is often very ingenious and technically sophisticated (e.g. a summary statement from one of the major contributors - Heckman 2000; for different takes on similar issues see Winship and Morgan 1999 and Little and Rubin 2000). Moffitt (2003) gave a summary treatment in the context of the debates concerning the association of teenage motherhood with subsequent disadvantage. A key focus in this approach is trying to remove the potential effects of unmeasured covariates, a real problem in assessing causality. However this has perhaps distorted priorities and focus in that more attention is often paid to the error term in statistical models than to the substantive elements and common usage has slipped towards calling all unobserved measures 'unobservables'.

Key themes of this paper are an emphasis on the importance of pathways rather than single causes, the likely crucial role of interplays and interactions, and the need to move away from 'black-box' approaches and capture as much as possible through measurement. There will of course always remain unmeasured (and possibly even unobservable) factors that we need to be careful about in making strong causal

statements. However, I am not convinced that causality can be established through ever more technical tricks in statistical models. Rather, I think progress towards understanding requires elucidation of the relevant pathways and dynamic theoretical models and that the balance of effort must shift in this direction.

Ultimately proof of causality may require carefully designed randomized experiments, although economists have made ingenious use of natural experiments to try to get closer to this (Rosenzweig and Wolpin 2000). However, even randomised controlled trials do not escape the problems of interplays and causal chains inherent in much of the within-person pathways discussed earlier and their interplays with the 'environment. For example, wherever there are gene-environment interplays that involve the stimulus (or intervention or drug treatment) under exploration, such trials will often mask stronger associations within the sub-group with specific susceptible alleles: think, for example, of assessing the results of a randomised control trial to assess the possible causal role of an intervention to reduce stress in dealing with the onset of depression that does not control for polymorphisms in the 5-HTT gene if the Caspi *et al* (2003) study proves robust and replicable.

An important related issue that bedevils many studies, including most black-box fixed effects or behavioural-genetics models, is that of sample selection. For example, sibling studies related to teenage motherhood restrict the sample to sibling pairs where one had a teenage birth and the other did not. Yet no-one would really maintain that there was a 'cliff' at the 20th birthday – there is plenty of evidence to suggest that the (possibly spurious) subsequent disadvantage for early motherhood extends beyond the teen years; the risks would appear to decline with increasing age at motherhood (e.g. Hobcraft and Kiernan 2001). Given that ages at first birth are correlated within families, the restriction to selected sibling pairs reduces the sample variation and thus provides estimates that are biased downwards. Moreover, it is highly plausible that any negative consequences of early motherhood are partly conditioned by the genome and by earlier childhood experiences. Hobcraft and Kiernan (2001) provide suggestive evidence for interactions of childhood poverty with early motherhood for some of the negative associations with subsequent disadvantage. It is fairly obvious and recognised by many (eg the recent summary by Moffitt 2003) that fixed-effects models give misleading results in the presence of interactions of the measured components with the unobserved ones.

Another topic of lively debate within economics is the rational choice paradigm (see, for example, the collection of readings in Elster 1986). At least two Nobel laureates in economics have gained their awards through challenging this paradigm: both come from psychology as a discipline. Herbert Simon (eg Simon 1957) introduced the notion of bounded rationality and some serious theoretical developments have taken place in this area. More recently these notions too have come under sustained challenge and scrutiny, with the extensive work by Kahneman and Tversky on decision-making heuristics, prospect theory and loss aversion, and framing and choice (eg Kahneman's 2003 revised Nobel lecture for a useful summary).

Much demographic behaviour is about choices made under constraints: to partner, to reproduce, to migrate, or to achieve well-being (for a pioneering attempt to get demography to engage with decision-making, see Burch 1980). In making such choices real people use heuristics or intuitive judgements since 'rational models are psychologically unrealistic' (Kahneman 2003, p1449). This is what Gigerenzer and Todd (1999; see also 2000 for a lively discussion) term 'simple heuristics that make us smart' or 'fast and frugal decision making'. For demographers this work contains two possibly interesting attempts to apply simple simulated heuristics to mate search (Todd and Miller 1999; also Todd and Billari 2003) and to parental investment (Davis and Todd 1999): the simulations rely on simplified evolutionary theory. There is a sharp distinction between the two approaches to decision making heuristics: Gigerenzer and Todd try to show that these heuristics work well, whereas Kahneman and Tversky more carefully and thoroughly demonstrate some of the failings and biases of heuristics in addition to their power and accuracy (see Gilovich, Griffin and Kahneman 2002 and Kahneman 2003). These developments, far too rich to cover in detail here, have spawned the exciting field of behavioral economics (Camerer, Loewenstein and Rabin 2004; relatedly see Glimcher 2003).

If we are to make real progress in understanding how demographic choices are made, we shall need to engage with this work. For example, I have already referred to the potential for taking account of prospect theory concerning greater aversion to losses than the risk seeking preference for gains (Tversky and Kahneman 1992). It would be fascinating to explore these domains for partner choice, reproductive choice, and migration decisions.

Another topic worthy of more subtle exploration by demographers is related to intertemporal choice (Loewenstein, Read and Baumeister 2003). There is clear emerging evidence of inter-individual differences in ability to postpone gratification or in discount rates, and thus in time perspectives or decision-making horizons. This is of relevance to our understanding and assessment of topics as diverse as the economic rationale for childbearing in developing societies (e.g. Das Gupta 1993, Chapter 12 for a good summary) or of early childbearing. Understanding the processes involved and the differences in brain and behaviour for early parents would be useful in this respect and both risk-taking and intertemporal choice are likely to be relevant.

We also recall the speculations of Rutter (2003) and Hobcraft (2003) about possible genetic predispositions for risk-taking behaviour and the possible resulting pleiosis (multiple influences for one gene) for many aspects of reproductive behaviour (early childbearing, contraceptive failures, sexually transmitted infections, partnership instability); the possible linkages to (subsequent) disadvantage through job instability and less informed choices may also be relevant in understanding the consequences of demographic behaviour. The neuroscientific aspects of risk-taking in this context are clearly likely to matter too.

4.2 Psychology

Psychology is the one discipline that seriously grapples with the range of issues involved in the programme for population research laid out here. It brings together, sometimes uneasily, behavioural geneticists, cognitive scientists, and social psychologists and involves collaboration with geneticists, neuroscientists, and social scientists. Its crucial concerns include the genome, mind, brain, emotions, decision making, and relationships. Close attention is paid to the design of studies and these are often rigorous (e.g. Rutter *et al* 2001; Plomin *et al* 2003). A major focus is on trying to elucidate pathways, processes and mediators. The widespread use of structural equation models forces some conceptual clarity concerning interlinkages and pathways that matter. There is sometimes a very constructive concern with trade-offs between ‘thick’ and ‘thin’ measurement (the rich tapestry from small-scale studies and the gruel from large scale surveys). Social sciences and demography could probably benefit from some of the more rigorous approaches used in these small-scale studies, where for example double-blind rating of qualitative or observational material is valuable.

However, there are many features of demographic research styles that could usefully shape our engagement with the psychology paradigm. Paramount is our interest in whole populations, rather than psychopathologies. We are already engaged with very different styles of in-depth studies used by anthropologists and can benefit from enrichment by both approaches. Our strong empirical tradition permits us to avoid the worst excesses of theoretical straitjackets that can narrow psychological research (and economics too), since if you do not look at alternatives you will not find evidence for them (as can happen for example with specification of gene-environment pathways over the developmental course where only genes are allowed to exert repeated influence). However, as argued earlier, we shall not make real progress in understanding demographic behaviour unless we develop firmer mid-level theories that sort the wheat from the chaff and begin to specify evidence-based linkages and pathways.

Since we have already extensively considered the need for population research to engage with behaviour-genetics, with neuroscience, and with interpersonal relationships and tried to give concrete examples where we already have knowledge, or have enough to explore further, we shall not dwell on these issues here. Suffice it to say that engagement with psychologists and with psychological research should be an explicit and high-priority agenda for real progress to be made in population research.

4.3 Biology

It is also clear that much of the agenda laid out in this paper requires us to engage with the life sciences and biology. We have much to gain from discovering relevant findings from genetics, neuroscience, endocrinology, and evolutionary theory and from working with relevant scholars to make real progress on bringing these areas into demographic research in a focussed and precise way.

Evolutionary thinking, especially evolutionary psychology and evolutionary ecology have much to say about survival, mating, and reproduction and such thinking has begun to make a real impact in demography in recent years (e.g. Clarke and Low 2001, Diamond 1997, Gangestad and Simpson 2000, Low 2000, Gangestad 2003, Worthman 2003, and Kaplan and Lancaster 2003; Haaga 2003). Bringing together evolutionary and economic (and other) theories about mate choice or reproductive behaviour and examining their (fairly common) basic assumptions, for example about gender roles, could ultimately prove quite fruitful (Hobcraft and Kiernan 1995, Lam 2003). One of the great challenges, though, is to formulate many evolutionary theories as testable propositions and to sort out the enticing speculation from the useful constructs (see Lickliter and Honeycott (with discussion) 2003, Freese and Powell 1999 and the comment by Kanazawa and reply 2001, and De Waal 2002).

The value of biology as a paradigm for sociology has received considerable recent attention. Lieberman and Lynn (2002) suggest that much of ‘what passes for scientific sociology is derived from classical physics, a model of natural science that is totally inappropriate for sociology’. They suggest that a more applicable approach to linking research and theory would be to adopt several key elements of Darwin’s approach to evolution. Interestingly, their dismissal of a physics paradigm (presumably mediated through an economics paradigm) is at variance with the several thoughtful papers by Burch (2002, 2003a & 2003b) on the potential to learn from how physics is really done. The key features that Lieberman and Lynn (2002, p1) identify and justify at some length are: ‘drawing rigorous conclusions based on observational data rather than true experiments; an ability to absorb enormous amounts of diverse data into a relatively simple system that did not include a large number of what we think of as independent variables; the absence of prediction as a standard for evaluating the adequacy of the theory; and the ability to use a theory that is incomplete in both the evidence that supports it and in its development.’

The emphasis on a holistic approach or a system not variables is related to, but subtly different from, the recent emphasis among many psychologists on ‘person-centred’ as opposed to ‘variable’ approaches to analysis (e.g. Singer 2002 and Bergman *et al* 2000). Lieberman and Lynn also make important points about the nature of causal chains, the role of time (relevant to the ‘stickiness’ of early fertility decline), and the importance of engaging with other disciplines.

In a different vein, Freese, Li and Wade (2003) provide a very useful review of how work on several of the topics covered in this paper is of relevance to sociology, including evolutionary psychology, behavioural genetics, and proximate biomarkers such as testosterone and serotonin; they also emphasise the likely emergence of an important role for neuroscience and fMRI in particular. Again this has some potentially useful insights for demography.

4.4. Epidemiology

Demographers have long been engaged with epidemiologists and public health issues. This, too, is an area that places great emphasis on pathways and processes involved in the aetiology of disease – a neat term that we might borrow, that avoids too strong an attribution of causality.

Epidemiology is fairly preoccupied with population-level inferences, though by no means as strongly as demographers. It is strongly an evidence-based subject area and has particular strengths (and inherent advantages) in the use of experiments to evaluate interventions.

A great deal of progress has been made towards implementing the broad paradigmatic shift emphasised here in the health sciences generally (for a splendid synthesis of the issues and research agenda see Singer and Ryff 2001) and demographers have been particularly involved in the burgeoning research on ageing (see Suzman 2004??).

5. 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.

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