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Something Ventured, Something Gained: Progress Toward a Unified Theory of Fertility Decline¹

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Introduction

The status of theory in demography is higher now than when Rupert Vance asked, in his 1952 presidential address to the Population Association of America, 'Is Theory for Demographers?' [Vance, 1952]. Vance's answer was a resounding 'yes.'² But in the years following, mainstream demography remained ambivalent about 'theory,' finding comfort in the more familiar realms of quantitative data and technique [see Burch, 1996, pp.60-61.] In 1984, Livi-Bacci could warn that demography ran the risk of becoming 'more a technique than a science' [1984, p.iii]. The systematic intellectual history of this aspect of the discipline remains to be written, but examples of ambivalence and neglect are not hard to find. As late as 1991, for instance, plans for a special issue of *Demography* on 'explanatory theories of fertility decline' had to be abandoned due to a lack of sufficient contributions.³

¹. This is the latest revision of a paper presented at the Chaire Quetelet 1997, Louvain-la-Neuve, Belgium, 26-28 November 1997. It differs in a few small respects from the version submitted for publication. The work on which it is based has been supported by the Canadian Social Sciences and Humanities Research Council, grant #410-96-1303.

². Vance did not offer a formal definition of 'theory,' but rather seemed to assume his readers would understand what he meant. He mentions 'general theory,' but suggests that much demographic theory will be theory of the 'middle range' [following Robert Merton]. Transition theory is cited as a prime example of a major theoretical perspective, but one needing further work.

³. Mason [1997] writes that following Vance's 1952 address, '...demographers have indulged in social science theorizing with a vengeance' [p.443]. It depends partly on what one means by 'theorizing,' but I would tend to agree with her statement only with respect to the last decade or so, with the exception of economists [principally Becker, Leibenstein, and Easterlin], who began sustained systematic work on fertility theory as early as 1960, and who, in the case of Becker and Leibenstein at least, considered themselves theorists. Davis's PAA address [Davis, 1963], Coale's reconsideration of transition theory [Coale, 1973], Freedman's 'reappraisal' of fertility theories [1979], and Caldwell's restatement of transition theory [1976] all are landmark

This period of demographic history now seems behind us. The introduction of microeconomic theory into demography around 1960 was a turning point. After some delay, the last decade has seen a surge of interest in theory by non-economist demographers. In a different sub-field, the establishment of an IUSSP research committee on migration theory [Massey *et al.*, 1993] is symptomatic. But nowhere has the growing interest in theory been greater than in the area of fertility decline. The links of the topic to areas of practical concern such as family planning and population control, the enormous expansion of empirical evidence on fertility declines past and present, and the scientific stimulus provided by early theoretical statements in the form of 'transition theory' [whose oversimplification became apparent in light of an expanded evidential base] -- these and other factors have led to a steady stream of theoretical papers seeking some kind of general statement about key factors in fertility decline. Most recently, Caldwell, in his presidential address to the IUSSP [1997], assigns to the global fertility transition '...a dominance in demography possessed by no other theme....,' and calls for a unified theory of fertility decline that would avoid treating earlier transitions and more recent ones [assisted by large-scale family planning programs] as qualitatively different [p.803].

In approaching my assigned task of providing an 'overview of the main fertility theories,' I have focussed on papers published in the last decade, roughly 1987 or later. But even for this short period, it would be impossible in one paper to summarize, much less critically assess, all the important contributions. Rather, I have tried to identify several substantive themes and methodological tendencies that appear to stand out in recent work. If I neglect important work or themes, I am consoled by the fact that they probably have received their due in one or more of the several excellent review articles published recently [Hirschman, 1994; Van Da Kaa, 1996; Kirk, 1996; Robinson, 1997; Mason, 1997. See also the contribution by Piché and Poirier in this volume].

Overall, there has been much progress, but we still stand short of the comprehensive general theory of fertility decline -- much less verified theory -- that many feel is needed for both the scientific maturity of demography [Hirschman, 1994; Van De Kaa, 1996; Mason, 1997, Caldwell, 1997] and the cogency of its policy analyses [Robinson, 1997]. Whether such general theory is possible, however, and if so what form it might take, remain matters of explicit -- but more often implicit -- methodological dispute.

I state the main substantive themes and methodological tendencies in point form, with a few illustrations⁴ and a brief discussion of each. I return to what seem to me the most important

papers. But I believe it is true that none of these demographers, in contrast to the micro-economists, devoted large segments of their careers primarily to systematic theoretical work, or considered demographic theory their main scientific concern.

⁴. References in these discussions to particular authors or their works do not aim to give a comprehensive view of their work. This underlines the fact that this is not a review article in the

issues in a concluding section, and suggest where the quest for a unified theory of fertility decline may be leading us.

Substantive Themes

1. Most authors would improve on existing theories of fertility decline by adding content, including factors previously excluded or neglected. The result is a trend toward larger, more complex theoretical statements, with more variables and more complex interrelations among variables. This trend is partly related to the felt need to deal with ‘culture’ and with ‘ideational’ factors, partly to the ever-growing body of detailed evidence on past and contemporary fertility declines, leading to greater appreciation of historical context. There is growing resistance to simple, single-factor explanations, fuelled in part by the influential attack on the dominance of demand theories by Cleland and Wilson [1987] -- somewhat ironically, since they eventually find ideational change to be ‘**the** fundamental cause’ of fertility decline [emphasis added].

Mason [1997] identifies ‘six major theories of fertility decline,’ all of which she terms ‘incomplete’ [pp.444-445]. She argues for greater attention to culture and ideation [which she considers conceptually distinct], including greater emphasis on **subjective** perceptions of the key variables in the Easterlin synthesis [p.450; see also Szreter, 1996].⁵ Since objective values of these variables are still relevant -- subjective perceptions are at least partly determined by objective realities -- the net effect is greatly to increase the number of variables in a theoretical scheme, each objective factor having its subjective counterpart. Mason also would re-introduce an explicit focus on mortality, an element in early transition theory that has occupied a less central place in more recent work, due in part to the empirical finding of the European Fertility Project that mortality decline did not always precede fertility decline [Mason cites Chesnais, 1992 to question both this empirical generalization and its interpretation].⁶

Robinson [1997] would provide a much more detailed treatment of costs of fertility control, differentiating among various methods, and recognizing three separate kinds of costs -- time and money, social disapproval, and psychological discomfort.

Bongaarts and Watkins [1996] expand on the concept of *diffusion* [they prefer the term *social interaction*], putting greater emphasis on external or ‘point-source’ diffusion in addition

usual sense of that term.

⁵. Easterlin and Crimmins regularly conceptualized their variables in terms of relevant perceptions, but often as not in empirical work used empirical indicators of objective conditions due to a lack of data on perceptions as such.

⁶. For an excellent review of the role of mortality, see Van De Kaa, 1996. On the neglected issue of **perceptions** of mortality change by couples, see Montgomery, 1996.

to the internal or ‘interaction’ diffusion highlighted earlier by Rosero-Bixby and Casterline [1993]. Montgomery and Casterline [1996] also broaden earlier treatments of diffusion, expanding the concept to include not just ‘learning,’ but also ‘influence’ or social control. If one thinks in terms of the number of variables included in a theory or associated model, all of these authors have greatly increased the content of theory.

Most authors do not simply add more variables. They also introduce more complex interrelationships between and among variables. Mason [1997] speaks of ‘interaction,’ in the statistical sense of the term, and of delays, leads and lags. Her emphasis on interaction harkens back to Coale’s [1973] three preconditions for marital fertility decline, in effect a multiplicative model [see Lesthaeghe, 1997b].

Robinson and Cleland [1992] complicate standard microeconomic theories with the suggestion that costs of fertility control [not just the costs of children] affect the demand for children, in contrast to the Easterlin model, for example, in which demand for children is independent of costs of control. Lesthaeghe and Surkyn [1988] bridge the gap between ‘economic’ and ‘cultural’ factors by arguing that culture is partly endogenous to economic changes, and economic changes partly endogenous to culture. That is, there is mutual causation and feedback between the two realms.

The Chicago microeconomic school has not been immune to the trend towards bigger, richer theories. Characteristically lean and spare, based on the tripartite assumption of utility maximization, fixed preferences, and equilibrium, the Beckerian theory has been opened up to incorporate an expanded notion of utility involving altruism and ‘dynastic’ considerations [Becker and Barro, 1988; see also Seiver and Lage, 1996].

Finally, several authors have called for a return to an approach similar to Davis’s [1963] ‘multiphasic’ theory of fertility decline, in which marital fertility is seen as just one part of a larger demographic system including, notably, marital patterns and out-migration. Hirschman [1994] cites the work of Davis, of Lee [1987], and of Friedlander [1969; 1983]], while urging return to an ‘equilibrium’ or ‘homeostatic’ framework for the study of fertility. He questions the nearly exclusive focus on marital fertility, and comments: ‘If the homeostatic principle is to maintain demographic equilibrium in order to avoid community and household strain, then changes in fertility are only one of several mechanisms that can respond to ...rapid increases in population growth...’[p.227]. Mason [1997] also urges a re-examination of the Davis theory. Anderson and Morse [1993] sketch and make use of such a framework in their detailed study of Scotland’s demographic transition. Namboodiri’s [1994] suggested ecological approach to population dynamics would study fertility transitions in an even broader context.

A striking exception to ‘bigger theories’ is the paper by Friedman, Hechter, and Kanmazawa [1994] setting forth an ‘uncertainty theory’ for the value of children. Although they develop it strictly speaking as a theory of parenthood [that is, the first birth], they suggest it might have wider applicability in the explanation of fertility transitions. Far from adding content to existing theories, the authors take pride in their attempt to state a theory derived from

even fewer assumptions than standard microeconomic theory -- two rather than three. They comment: ‘...a superior alternative will consist of an internally logically consistent theory which has fewer behavioral assumptions [that is, only one assumption]...,’ but that is fruitful in terms of implications, including hypotheses about previously unobserved relationships [p.387]. Given the originality of this paper, it seems a shame that the main [only?] published comment is so thoroughly negative [Lehrer, Grossbard-Shechtman, and Leasure, 1996] -- a dismissal of ‘uncertainty theory’ as heretical and a reaffirmation of orthodox microeconomic theories. A more open examination of the theory seems likely to lead to new insights, whatever its flaws.⁷

Pollak and Watkins [1993] toy with a theoretical direction whose effects on complexity would be ambiguous -- it would lead to theory with fewer distinct concepts, but with some of the concepts, notably *culture*, much broader and more complex in themselves. As a way of reconciling the conflict between economic and cultural explanations of fertility decline, they consider abandoning ‘the analytical distinction between opportunities and preferences’ [p.490], in a major departure from conventional microeconomics, which considers preferences and constraints as conceptually distinct and independent [indeed, it often is said that one can speak of differences in tastes or preferences as leading to differences in behaviour only in cases where prices and income constraints are identical]. They write: ‘...accounts that emphasize the unity of culture, viewing culture as a coherent whole, a bundle of practices and values...deny the validity of dichotomous opportunities-preferences classification of the rational actor model... [T]his cultural perspective emphasizes that, in some cases at least, the bond between opportunities and preferences is strong’ [p.490]. Strictly speaking, this approach reduces the number of variables, but only by greatly expanding the empirical referent of the concept *culture* to include almost everything in society. Culture is viewed as a ‘coherent whole’; opportunities [and constraints] and preferences are ‘the warp and woof of the fabric of culture’ [p.485]. Their justification for this approach is that ‘the bond between opportunities and preferences is strong’ [p.490].

Their idea may be seen as a small instance of a seeming loss of faith in abstract analytic science, to be discussed later in more detail. But they face an uphill struggle to justify giving up conceptual distinctions which are so firmly rooted in behavioural theory [in economics, psychology, sociology, and anthropology] and in common sense, simply because of theoretical or empirical ‘bonds’ between the concepts. The fact that two things co-vary does not imply that they are not different things. In the case at hand, clearly some opportunities [or constraints] are cultural [social norms derived from culturally given values, definitions or perceptions of

⁷. Notably, the authors accord little recognition to other strategies for coping with uncertainty, some of which have been institutionalized: religion; magic; astrology; suicide; gambling; drugs and alcohol; hobbies; political action; fanaticism [as in being an avid fan]; etc. It also is not clear that all of their implications and hypotheses can be derived solely from the two axioms in the absence of many auxiliary assumptions. The ‘uncertainty theory’ has potential links with a large social-psychological literature on social roles and identity, but the authors fail to develop these links.

opportunities -- e.g., the common cultural myth of equal opportunity for all]. But some are more fundamental, more objective -- culture cannot define away a shortage of arable land, or a lack of modern contraceptives. It also is the case that opportunities may help to shape preferences over time, and that collective and individual preferences may shape opportunities and constraints -- the social construction of reality. As I have suggested elsewhere [Burch, 1996, p.69], this is a strong argument for more complex dynamic causal models, but not for a retreat from analysis.

Independently of Pollak and Watkins and in very different contexts, Bongaarts [1992] and Lee, Galloway, and Hammel [1994] develop models that involve a blending of formerly distinct concepts very much along the lines sketched by Pollak and Watkins. Both papers are primarily empirical, but both also claim to be empirical implementations of the Easterlin supply-demand theoretical synthesis.

Bongaarts effects a decomposition of fertility by expressing actual fertility as a weighted average of wanted fertility and natural fertility:

$$F = F_w * I_p + F_n * [1 - I_p] \quad [\text{Eq.1}]$$

where F is actual fertility, F_w the number of births couples want, F_n the number they would have in the absence of deliberate control, and I_p an appropriate weight. Bongaarts terms I_p the ‘index of preference implementation,’ and estimates it by solving Eq.1 for I_p:

$$I_p = [F_n - F] / [F_n - F_w] \quad [\text{Eq.2}]$$

F_n is estimated by formula from actual fertility and the proportion of married women using contraception.

The most direct interpretation of I_p is that it is the proportion of unwanted births that are averted. The numerator [F_n - F] gives the number of births averted; the denominator [F_n - F_w] gives total unwanted births. I_p can be related to the Easterlin formulation by noting that the denominator [F_n - F_w] is similar to Easterlin’s concept of *motivation*, defined as the difference between surviving children in the absence of control and surviving children wanted if control were viewed as cost-free. In Easterlin, birth averted, corresponding to Bongaarts’ [F_n - F], is proportional to the difference between motivation and costs [economic and psychic], or, assimilating Easterlin’s idea to Bongaarts’ notation:

$$F_n - F \sim \{ [F_n - F_w] - \text{costs} \} = [\text{motivation} - \text{costs}]$$

Substituting into the equation for I_p yields:

$$I_p \sim \{ [F_n - F_w] - \text{costs} \} / [F_n - F_w]$$

$$\sim 1 - \text{costs} / \text{motivation}.$$

In other words, I_p reflects **both** motivation to reduce fertility **and** the costs of the means of doing so. The index of preference implementation tends to be lowered by high costs of control, and raised by high motivation. But the two behavioural mechanisms are captured in one index. Bongaarts says as much: ‘ I_p ...rises as fertility regulation costs decline and its benefits [i.e., the elimination of unwanted births] rise’ [p.343]; I_p is a measure of ‘the role of the costs *and* benefits of fertility regulation’ [Abstract, p.382, emphasis added].

Whatever else may be said for it, this decomposition exercise does not correspond to the Easterlin model, which consistently distinguishes motivation and costs, and tries to measure them separately. What is gained in elegance and empirical tractability may be lost, however, in difficulties of interpretation arising when end and means are merged into one. Applying the system to simple scenarios yields strange results:

Case A: $F_n = 7; F_w = 6; F = 6$ -----> $I_p = 1.00$

Case B: $F_n = 7; F_w = 6; F = 7$ -----> $I_p = 0.00$

In other words, I_p takes on its extreme values for cases that do not differ much behaviourally, a difference of one child at high levels of fertility.

Or consider:

Case C: $F_n = 7; F_w = 3; F = 5$ -----> $I_p = 0.50$

Couples in case C have averted twice as many births as those in case A, but their index of preference implementation is only half as large. Finally

Case D: $F_n = 7; F_w = 1; F = 3$ -----> $I_p = 0.67$

In other words, couples in case D have a relatively high index of preference implementation despite the fact that they have three times as many births as they want.

The real-world data used by Bongaarts also poses some interpretive challenges. For example, across the 18 nations studied, I_p ranges from 0.19 to 0.94, suggesting a wide range in the ability of couples to implement their reproductive preferences. Yet the correlation coefficient between actual fertility and wanted fertility is high -- 0.95, $r^2 = 0.90$ -- suggesting relative uniformity in the degree of preference implementation [or, alternatively that much of the causation runs from actual to desired fertility rather than vice-versa, a possibility worth further exploration].

A paper by Lee, Galloway, and Hammel [1994] involves a measure that is conceptually identical to that of Bongaarts, but uses different terminology and symbols. They define D [cf. Bongaarts’ I_p] as ‘the proportion of excess births which is avoided [p.354],’ noting that it is a measure of ‘...the **combined effects** of the proportions seeking to avert births and the efficiency

with which they achieve their goal through contraception, abortion, and abstinence' [p.354, emphasis added]. The confounding of means and end is mitigated in their analysis by the inclusion of D in a multivariate equation which controls for fertility control cost-related variables such as religion.

The issue is not that Ip and D are in themselves invalid -- clearly they measure what they measure, namely a demographic outcome, unwanted births averted. But explanatory theory and policy analysis are crucially concerned also with process. And in the case at hand the behavioural processes of goal-setting and effective use of relevant means are confounded. My diet can be completely 'successful' if I only try to lose one kilo and do so; or it can be only a little bit 'successful' [largely a failure] if I adhere to a sound regimen of lowered caloric intake and exercise, and lose 5 kilos when my goal was to lose twenty. Common sense says that the second scenario involves greater success objectively. Subjectively, one can argue that success should be judged only in terms of an individual's personal goals ['consumer sovereignty'?), but not many [individuals or societies] would be willing to apply this principle across the board, including, I suspect, the above authors.

Lee [personal communication] points out that for their historical study of Prussia there are no direct measures -- or even good proxies -- for costs of fertility control. He questions whether there are really good measures of costs even for contemporary situations. To the extent this is so, then of course a cost measure cannot be broken out in a multivariate statistical model. But this is not a persuasive reason to drop the needed distinctions in theoretical statements and in theory-based models.

2. Virtually all authors have argued for greater attention to cultural and ideational factors in fertility decline, and most for the harmonization of economic and cultural explanations. This is one of the specific reasons behind the substantive enlargement of theories noted above. Again, the 1987 paper by Cleland and Wilson has been pivotal in this development, although most authors have not shared their tendency to favour culture 'rather than' economics. The more common approach has been to try to synthesize the two sets of factors: Lesthaeghe and Surkyn [1988; see also earlier works by Lesthaeghe]; Hammel [1990; 1993]; Mason [1992]; Pollak and Watkins [1993]; Greenhalgh [1995]. Montgomery and Casterline [1996] retain rational decision making as a key element in their tripartite model, along with social learning and influence. Bongaarts and Watkins [1996] aim to bring 'social interaction' to center stage, but their empirical evidence requires retention of 'development' [in the form of the U.N. human development index] as an important variable in fertility decline.⁸

⁸. Their rhetoric sometimes suggests differently: '...if fertility and development were closely linked...' [p.641, emphasis added]. But they are talking here about relationships with the timing and pace of fertility decline. In their Figure 2, virtually no nation with a human development index of greater than 0.8 has a total fertility rate above 3; virtually no nation with a human development index of less than 0.5 has a total fertility rate less than about 5.

There has been some expansion of the concept of culture as it applies to this context, with emphasis on the variety of cultural elements that may be involved: social norms regarding family size or fertility control methods; ideals on family size; social definitions of age and gender roles; secularization; individualism; 'family nucleation'; Westernization; etc. Some [e.g., Lesthaeghe] have emphasized the internal dynamics of cultural systems, such as the unfolding of ideas of The Enlightenment within the European sphere. Others [e.g., Caldwell, in his earlier work on contemporary developing countries] have emphasized external diffusion of ideas from outside cultures [Westernization]. Van Da Kaa [1996] emphasises the differences in this regard between the historical West and contemporary developing nations.

If most authors have thought culture and ideation important to fertility decline, there have been few concrete suggestions as to how these factors are to be conceptualized and measured, and how specifically cultural hypotheses are to be tested -- really tested instead of relying on the facile assumption that residual variance or differences must be due to 'culture.' Lesthaeghe's early work using voting statistics to proxy secular individualism, and van de Walle's detailed historical study of the rise of quantitative notions about family size [1992] are notable exceptions. Hammel [1990] and Greenhalgh [1995] have tried to introduce systematic ideas about culture from anthropology, with only partial success. The average demographer does not quite know how to systematically incorporate culture into his or her thinking about fertility. Culture is too specific, too concrete, too particularistic, too resistant to generalization. We have the same problem as Notestein [1945], who saw that ideas associated with The Revolution had helped bring about early French fertility decline, but relegated the insight to a footnote, rather than making it central to his emerging transition theory.

3. The Concept of *Diffusion* Has Taken Center Stage. The notion of *diffusion* has a long history in fertility decline theory. Carlsson [1966] was perhaps one of the first to give it prominence, posing the question whether fertility decline in Sweden was due to 'adjustment' [application of old ideas to new circumstances] or to 'innovation' [application of new ideas, partly diffused from elsewhere]. The work of Rogers [Rogers and Kincaid, 1981] touched the field briefly in the 1970's but did not have a large permanent impact.⁹ Massive empirical data from the European Fertility Project showing similar fertility trends across national borders, but differing fertility trends within nations but across linguistic-cultural borders, were interpreted as evidence of diffusion and of the important role of culture in explaining fertility decline.

To attribute fertility decline to diffusion, however, is not necessarily to explain it. It can be just a restatement of the explicantum, the **spread** of fertility decline or of the practice of family planning. There is danger of tautology. For the concept of diffusion to have explanatory value, it is necessary to look at what is being diffused [Mason, 1992] and at the mechanisms by

⁹ It is interesting to note that the late 1960's appointment of Bernard Berelson, a non-demographer, as president of The Population Council was based in no small measure on his background in 'mass communications,' presumed to be a key factor in promoting successful family planning programs.

which some idea or behaviour is transmitted from one place to another. As de Carvalho has recently commented: 'These illusive [sic] determinants [of overall fertility decline] have often been conceptualized in terms of the diffusion of anti-natalist norms, values, and behaviour. Yet the mechanisms at the heart of the diffusion model are poorly understood and inadequately specified. These limitations seriously undermine the predictive power of the diffusion model, and its practical applicability to real contexts...' [1997, pp.2-3].

An important attempt to render the diffusion concept more specific is by Rosero-Bixby and Casterline [1993]. Drawing on a large general literature on the conceptualization and modelling of diffusion, they develop a dynamical model, a system of differential equations, tracking the movement of married women of reproductive age among different fertility-control sub-groups, in effect from uninterested, to interested but unable, to actually controlling. This movement is enhanced by presumed social interaction among the various groups leading to 'contagion,' a function among other things of sub-group sizes. They also develop a model that allows diffusion from one population sector to another [e.g., rural and urban, upper and lower class], along with interaction diffusion within sectors.

Bongaarts and Watkins [1996] develop an even broader concept of diffusion, which they rename 'social interaction.' They discuss three different types of diffusion -- or in their terms, 'three different levels' at which social interaction operates: 'personal networks connect individuals; national channels of social interactions such as migration and language connect social and territorial communities within a country; and global channels such as trade and international organizations connect nations within the global society' [Abstract, p.811]. Their first two levels seem closely related to Rosero-Bixby and Casterline's 'interaction diffusion.' Their last level introduces a new dimension, variously referred to as 'external' or 'point-source' or 'external influence' diffusion [Mahajan and Peterson, 1985; Heckfeldt, Kohfeld, and Likens, 1982, p.30].

Montgomery and Casterline [1996] take a slightly different approach to elaboration of the concept of diffusion: 'The concept of diffusion can be separated into two fundamental components, social learning and social influence' [p.151]. Roughly speaking, the former refers to transmission of ideas, the latter to social constraints on behaviour, modelled in terms reminiscent of Kurt Lewin's field theory or the more recent conceptualizations by Fishbein [1963;1972; see also, Falbo and H. Becker, 1980].¹⁰ Montgomery and Casterline view social learning and social influence as two parts of 'a common conceptual model,' the third part being 'rational individual decision-making' [p.152]. Their model thus integrates social structure, culture and economy.

Kohler [1997a; 1997b; 1998] has explored a number of specific phenomena within the general area of 'social interaction'/fertility interrelationships. His work combines sophisticated

¹⁰. In contrast to 'uncertainty theory,' Montgomery and Casterline's paper makes good use of the enormous general literature relevant to their concerns in sociology, psychology, and social psychology. This is a marked departure from the practice of much earlier work in demography, not infrequently involved in re-inventing conceptual/theoretical wheels.

economic theory and modelling, a broad grounding in relevant literature of other disciplines [for example, the sociological works of Giddens], and serious attention to the revisionist economic ideas of Brian Arthur and others, with emphasis on stochastic and non-linear elements. Given past experience with the work of Becker and the Chicago School on the microeconomics of fertility, it may take a while before non-economist demographers digest more than the general drift of Kohler's work. His active collaboration with social demographers such as Watkins, however, and their involvement in interdisciplinary empirical research projects should help in this respect.

Taken together, these works on diffusion seem to me to constitute a large step forward toward a general theory of fertility, one in which social structure finally plays an important role.¹¹ If one could combine their ideas effectively with the Easterlin supply-demand model and with the rich, historically-based, and flexible synthesis of Lesthaeghe, the progress would be great indeed.

4. Attempts to bring biology, specifically evolutionary theory, to bear on fertility decline. Two recent papers pursue this theme. Potts [1997] concludes: 'The argument that development causes fertility decline is flawed because people cannot make choices about family size without realistic access to fertility-regulation technology, and such access is historically recent and remains geographically limited' [Abstract, p.221]. Given the riches of evolutionary theory, this is an anticlimactic conclusion, reverting to earlier 'instrumental' theories of fertility decline that confuse means and motivation and necessary and sufficient conditions. On the other hand, hardly anyone would deny that availability of effective fertility control is an important element in fertility decline processes. Arguments such as that by Robinson and Cleland [1992] that availability of means can affect other factors such as preferences or demand for children tend to heighten that importance.

Kaplan [1994] purports to test Caldwell's 'wealth flows' theory of fertility decline with data from a preliterate nomadic society. As I have noted elsewhere, the test is compromised insofar as a society with a minimum of age-sex stratification is not the ideal test of a theory whose central mechanism rests on patriarchy [Burch, 1996, fn.8]. But there probably is something to the argument that a wealth flow to the older generation of such kind and amount as to lead to exploitation of the young poses survival risks for a population or society -- a fact that developed societies with youth unemployment rates of twenty percent and upwards and total fertility rates of 1.2 or 1.3 might want to think about, for political if not scientific reasons.

My knowledge of biology is not vast. But my guess is that evolutionary theory is not where the cross-disciplinary payoff lies for fertility theory. My approach would be to take a closer look at population biology and ecosystems science [Levin *et al.*, 1997; see also Namboodiri, 1994], where individual-population interrelationships are similar and where great

¹¹. This theoretical work on diffusion, originally stimulated by empirical research [notably the European Fertility History Project], has in turn led to several on-going empirical studies. See, for example, Watkins, Rutenberg, and Green [1995], Stoner-Eby [1998] and Montgomery and Zhao [1998].

strides have been made in modelling of these interrelationships.

To summarize this section on substance, development of the above themes may be viewed as ‘normal science’ rather than as paradigm shifts, with very little that can’t be found in Notestein, Davis, or Coale, if one reads the fine print. The focus on ‘social interaction,’ ‘diffusion,’ and ‘social influence’ might be considered such a shift, but most of the key actors build on rather than reject an underlying microeconomic choice model. It seems to me more like a fleshing out of the microeconomic skeleton, along lines adumbrated by Easterlin from the very beginning [1969]. Uncertainty theory also may be counted an exception [although it too works off the basic economic choice model], along with the attempt to cast fertility decline in evolutionary terms. But neither of these has yet yielded large theoretical payoffs or stimulated much empirical research by demographers.

Methodological Tendencies

1. There is continuing ambivalence about aiming for theory at high levels of generalization and abstraction, and uncertainty about the possibility of one unified theory.

In terms of recent writings, Szreter [1994] stands towards one pole, emphasising the concreteness of fertility decline processes in a way that would divert attention and effort away from general unified theory [even if does not deny its possibility], and calling for ‘...emancipation from the dominance of the abstract idea of “demographic” or “fertility” transition and the associated, too exclusive deference to the covering laws methodology...’ [p.692]. Szreter calls for:

...an accumulation of patient, carefully contextualized, investigative projects on fertility change in specific communities, where the form that fertility change takes is not judged in advance.... Only such studies as these can do justice to the variety of changing fertility behaviours in any community and can examine the ways in which economic and political forces of change are mediated by local, cultural, and institutional forms such as changes in language, values, and roles.... [p.692].¹²

At the other pole stand Friedman, Hechter, and Kanmazawa [1994] who favour very simple and abstract theory. In their view, a theory is to be judged good [that is, ‘fruitful’] ‘...insofar as it minimizes the number of assumptions from which hypotheses are derived, maximizes the number and variety of hypotheses derived from these assumptions, and generates hypotheses about phenomena and relationships that have not yet been observed’ [p.387]. Simplicity, abstractness, and generality are to be achieved by picking a small number of assumptions that may themselves be oversimplified or even unrealistic. They quote approvingly several authors who hold that a theory should be judged more by its hypotheses than by its

¹². In a recent conversation, Szreter suggests that I had earlier read too much into these statements, and that he does not rule out the possibility of some kind of general theory. In his most recent work [1996] he focuses on changing perceptions of the costs of children as a key **general** element in fertility decline.

assumptions: Jasso [1988], Stinchcombe [1991], and especially Friedman [1953]: ‘...the more significant the theory, the more unrealistic the assumptions...’, that is, assumptions that are ‘wildly inaccurate representations of reality...’ [p.14].

The bulk of theoretical writing on fertility has taken a middle course: general theory is needed and possible, but it should be true to the realities of historical experience and to our intuitive sense of how human beings behave. This methodological predilection for ‘realistic’ theory may be one of the reasons mainstream social demography has tended to hold Beckerian microeconomic theory at arm’s length, using it only ‘heuristically,’ not for purpose of rigorous statement and inference [McNicoll, 1992].¹³ This strain toward realism lies beneath the general tendency to enlarge content of theories and make them more complex, as described earlier. It is being reinforced by the current interest in ‘qualitative methods,’ and a concomitant weakening of exclusive reliance on standard analytic methodologies [the general linear model] for the analysis of typical data [large-sample surveys].

The same tendency has lead many to hedge their bets as to whether there can be **one** theory of fertility decline. Caldwell’s classic work [1976] started with the view that one theory was needed to explain the start of fertility decline, another theory to explain its continuation. The theme is echoed in the masterful review of migration theory by Massey *et al.* [1993], and by Feeney [1994] in his summary of East Asian fertility trends. Mason [1997], by contrast, looks for a general theory that will explain not only fertility transitions, but other changes and differences as well.

Closely related is the suggestion that the phrase *fertility decline* is ambiguous, and that different theories may be needed to account for different specific meanings of that phrase: marital or overall fertility; the fact of fertility decline, as opposed to its timing or speed; fertility decline over different time scales or at different geographical levels; etc. [Burch, 1996; Mason, 1992; 1997]. Bongaarts and Watkins in fact speak of three different dependent variables [1996, pp.641-642].

Hirschman [1994] notes that ‘The vast body of empirical evidence on the origins, speed, and correlates of fertility declines in different historical and geographical settings shows more diversity than a single theory of fertility change would predict,’ and poses the challenge ‘...to develop a common theoretical framework that will accommodate the diversity of historical paths from high to low fertility’ [p.203]. Caldwell [1997] has recently posed a similar challenge in his Beijing IUSSP address ‘The Global Fertility Transition, the Need for a Unifying Theory.’

¹³. Another part of the explanation is that most non-economist demographers simply do not know enough economic theory or mathematical economics to master the details of the relevant literature. In other words, our stance towards Becker has been due partly to tastes and partly to constraints. The latter provide a strong argument for more cross-disciplinary collaboration.

2. There is continuing absence of rigorous, ‘formal’ statements of theory. This tendency is closely related to the one described above as point #1. The simpler, more abstract a theoretical structure, the easier it is to state and manipulate it in terms of analytic mathematics or formal logic. The more realistic a theory -- the more it tries to be faithful to complex, concrete reality -- the more intractable it becomes using these tools. This is why abstract theorists and mathematical modellers typically urge that we start with ‘the simplest case’ that is relevant to the problem at hand, and why they use the word *tractable* so often.

But as theories become richer in content and more complex, the dangers of more informal modes of theoretical discourse come into play, chief among them vagueness and logical errors. Mathematics and newer computer languages yield not just numerical precision but also – and perhaps more important – logical rigour.

Lack of formalization has not been as nearly complete as it was in earlier decades, when many influential theoretical pieces took the form of ceremonial addresses to the Population Association of America. The models by Rosero-Bixby and Casterline [1993] and most recently by Lesthaeghe [1997] have introduced considerable formalization into the specification of processes moving people among states defined with reference to their orientation to fertility control, processes that lie at the heart of aggregate fertility decline. Rosero-Bixby and Casterline use differential equations, Lesthaeghe uses a mathematical statistical approach, but the underlying conceptualization is similar. Economists have always been more inclined and more able to state their theories in mathematical form, a tendency, as noted earlier, that has limited the influence of the details of their theoretical systems on demographers with other disciplinary backgrounds.

Eloundou-Enyegue and Stokes [elsewhere in this volume]] continue in this tradition, proposing a matrix framework involving nine parameters and a child quality production function. The framework is put forward not as ‘substitute to current theories,’ but as ‘a device to quantify and integrate their influences.’

Montgomery and Casterline [1996] offer several equations which express their central concepts of social learning and social influence, integrated with ordinary microeconomic notions such as the utility function, but stop short of the systems of equations to which these formulations seem to be pointing. Nor have they yet specified which elements in potentially indefinitely large vectors of influences are the crucial ones, theoretically speaking. As noted earlier, Kohler has explored notions of social interaction and social influence using a high degree of economic theoretical formalism.

Burch [1997] has begun an attempt to synthesise several leading theories and to express them in terms of systems dynamics language for computer simulation.

Montgomery and Zhao [1998] have developed a dynamic individual decision model which is then embedded in a model of social network structure. Simulated results of the model are

compared with field data from Ghana, with promising results.

The demographic tradition of explanation through decomposition or the partitioning of variance continues in papers by Bongaarts [1992; 1997] and by Lee, Galloway, and Hammel [1994], discussed in detail earlier. While useful for summary descriptions of what has happened - decomposing the explicandum -- these formulations seem to me to fall short of explanatory models dealing with process and mechanisms of change, even when the decomposition is embedded in a multivariate model. The analysis is more formal than behavioural.

Overall, formalization of fertility theories has been limited, dealing with only bits and pieces of our most comprehensive theories. We are a long way from a complete, rigorous, and formal statement of theoretical systems as rich as those of Montgomery and Casterline [1996], Bongaarts and Watkins [1996], or, especially, Lesthaeghe.

3. Some confusion remains about the relationship between abstract theory and explanations of concrete historical events. Max Weber [1949 ed.] made a sharp distinction between 'social science' and 'sociology.' The one sought abstract social laws, general regularities in behaviour or structure. The other sought to use those laws, among other things, to explain concrete historical processes. The distinction has been repeated over the years. Homans [1967], for example, discusses it under the heading 'psychological explanations' in history, showing how explanations of concrete events involve a mixture of general behavioural premises and specific historical facts [p.44]. Anderson has summed it up nicely [1986]: 'What is needed to understand marital fertility decline is not a unique theory or explanation...for each different time and place, but particular knowledge of the culture and history and detailed data for small areas, combined with the discipline inherent in a theoretical approach' [p.313].

The development of abstract theory, including microeconomics, uncertainty theory, exchange theory, diffusion theory, etc., is simply a different intellectual game than the development of an adequate explanation of fertility decline in, say, India in the period 1948 to date, or in England since the 19th century. The two games are or should be related, but they are different. To a certain extent they will move on separate tracks, with empirical research and more realistic theorizing posing a constant challenge to the highly abstract, simplified representations of a Becker, or of Friedman, Hechter, and Kanmazawa, and with abstract theory and associated research giving detailed guidance as to the variables, functional forms, and parameters.

E.O. Wilson has recently written of biology:

Much of the history of biology can be expressed metaphorically as a dynamic tension between unit and aggregate, between reduction and holism. An equilibrium in this tension is neither possible nor desirable. As large patterns emerge, ambitious hard-science reductionists set out to dissolve them with nonconforming new data. Conversely, whenever empirical researchers discover enough new nonconforming phenomena to create

chaos, synthesizers move in to restore order. In tandem the two kinds of endeavors nudge the discipline forward [quoted in *Science* 265 [23 September 1994] p.1901].

This could be a description of the development of demographic fertility theory, especially over the last two decades.

But there is a growing body of opinion to the effect that the days of the dominance of extremely abstract analytic theory may be numbered -- or at least that its status as the norm and goal of all scientific theory will diminish -- due to the advent of the computer with its ability to handle more complicated theoretical systems and models. Others argue that what has worked for physics or for economics [maybe] may be less appropriate for biology or for behavioural and social sciences, in which history and extreme complexity are characteristic. No doubt computer simulation or modelling is destined to play a much larger role in theoretical work in the coming century.

The practice of working with smaller models, models which the human mind can comprehend, seems unlikely to disappear entirely, however, if only for aesthetic and psychological reasons. It remains one path to insight into how things work. Besides, we can never rule out in advance the possibility that someday someone will come up with an elegant theory that explains the phenomena of interest. Empirical verification of highly abstract, simplified models will always be hampered, however, by our virtual inability to perform strictly controlled experiments.

Some Concluding Comments

Demography as a discipline has tended not to be self-conscious about methodology [as opposed to technique] -- the epistemology and logic of scientific procedure and explanation. Lesthaeghe's recent [1998] exploration of demographic applications of the work of Lakatos is a notable exception. As a multi-disciplinary field, demography lacks consensus on methodology, with practitioners bringing different methodological ideas from different disciplines. Even the best possible theory of fertility decline imaginable might fail to command acceptance in all quarters. It is difficult to think of a theoretical statement that would satisfy Szreter [history] or Greenhalgh [anthropology] and at the same time followers of Becker, or Friedman, Hechter, and Kanmazawa. Over the long term too much self-consciousness about scientific procedure, including theory building, is not healthy, but at the moment demography might benefit from a period of self-examination on these matters. This is especially so as 'computer modelling' [or 'scientific computing'] is establishing itself as a new way of 'doing science,' with its relationships to traditional experiment, non-experimental statistical analyses, and theory still to be worked out.

As a practicing demographer, I am not sure where to look for guidance on an approach to theory, explanation, and computer modelling that might command widespread acceptance among demographers. Perhaps methodologists and philosophers of science, or practitioner in other fields, have set forth the directions to follow, but I cannot claim a systematic knowledge of the relevant literature.

I have recently encountered, however, a slim and somewhat old book on explanation [Meehan, 1968. See also 1994] that seems to me to have anticipated our current situation and to point the way forward. Meehan rejects the 'covering law' approach to explanation, arguing for a 'systems' approach in its stead. The system is a rigorously stated formal structure which in strict logic entails the phenomenon to be explained. A good system can predict and suggest ways in which at least in theory outcomes might be controlled. Verification or validation of an explanation of a particular phenomenon or class involves showing that the system is more or less 'isomorphic' with respect to the real world system to be explained. The focus is on explaining specific events, and on prediction and control, rather than just 'understanding,' as sensible aims of explanation.

Meehan wrote when computers and computer modelling were in their infancy, and speaks several times of the difficulties of manipulating systems, of being able to work out a 'formal calculus' for logical inference when systems become complex. Modern programming tools would seem to provide the solution.

In its emphasis on an explanatory system [theory?] as a purely formal structure, Meehan's ideas somewhat resemble standard economic theory, but his systems are not derived from a limited number of axioms -- propositions in the system can be derived from axioms, but also from intuitions, empirical regularities, etc., that is, from any source that might give insight into how the system works. Meehan also insists that just predicting an outcome is not enough; the system must allow us to see into the black box of process and mechanisms -- there must be an overall isomorphism, not just agreement in terms of outcomes.

Pending more research into these ideas and their background in methodological writings, I sense, as noted above, that they are pointing us in the right direction as to the form of the unified theory we seek.

In any event, I believe we are close to achieving a comprehensive integrated theory of fertility decline. We probably have all the pieces for the puzzle [I say 'probably' because 'uncertainty theory' must give one pause: no doubt there are still other new ideas waiting in the wings]. Indeed, we may have too many pieces for one puzzle. There are at least three reasons for this.

First, some of the elaborations and detailed distinctions made in recent writing may turn out to be unnecessary, adding nothing to explanatory or predictive power. Montgomery and Casterline's return to a 'value times expectancy' concept of the actor's orientation to behaviour may be a case in point. Falbo and H. Becker [1980], for example, in a test of Fishbein's similar conceptualisation, suggested that the 'value times expectancy' elaboration of attitudes was redundant. Respondents tended to think about and mention rewards or punishments only if they thought them highly likely; unlikely outcomes or utilities tended not to be salient.

Second, we may have some duplicate pieces, different names for virtually the same concepts. As noted above, Lesthaeghe's and Montgomery and Casterline's three categories of people with respect to their orientation to fertility control are similar, except for terminology and for the fact that Lesthaeghe allows more flexibility in the temporal order in which people move through the categories.¹⁴ They are close enough that some consolidation is possible.

Third, there may be pieces to more than one puzzle. That is, some theoretical propositions found in the literature may be essentially contradictory. For example, a theory that assumes unvarying tastes [preferences] is incompatible with a theory that attributes a large part of fertility decline to declining preferences [as distinct from demand] for children.

In any case, I believe we should try to assemble the puzzle from the pieces at hand, to try to create a synthetic theory based self-consciously on the existing body of literature. Apart from the relatively few contradictory elements just noted, it will be one theory, but also one admitting different levels of generality. This process of refinement and synthesis will require a degree of clarity and precision in the definition of concepts and in the statement of theoretical propositions that goes beyond what has been commonplace in theoretical writing, with the notable exception of economics. There will be explicit attention to things like 'scope conditions,' that serve to distinguish different theoretical levels, or, if you will, sub-theories. How the process of fertility decline works, for example, clearly depends in some way on whether it takes place in the presence or absence of modern contraception and modern state-financed family planning programs. A general theory of fertility will have to incorporate these and similar facts.¹⁵

My personal view [see Burch, 1996] is that the complexity at hand and the needed rigour in theoretical statement and manipulation will push us beyond ordinary language, traditional logic, and even analytic mathematics, to a greater reliance on computer modelling, using 'languages' such as 'systems dynamics' or C/C++ [object-oriented modelling]. This rigour will be sought not for its own sake, but because it will be needed if we are to derive valid implications from our theory, and if the theory is to be tested.

The assembled puzzle may be pretty and persuasive to some, especially those who have built it, but in empirical science we still have to ask if it is true in the limited and qualified sense that the term is used in science. Testing a theory can occur only if we can state implications derived from the theory by strict logic. A 'rubber-band' theory, one that can be stretched to

¹⁴. This is an important difference insofar as differences in temporal order may be a key to differences in the rapidity of fertility decline.

¹⁵. Paradoxically, it would seem that a general theory of fertility will have to be either very abstract, or very detailed – bordering on the concrete. This seems to take us back to Weber's classic distinction mentioned earlier, and to the idea that constructing a general theory and applying it in order to explain a concrete historical event are two distinct tasks. In Meehan's terminology, a formal system must be 'loaded' in order to explain a specific event.

explain any outcome, can be neither disproven nor verified. But our synthetic theory will be too complex to work out its implications in our head, to intuit them. The age of 'eye-balling' theoretical predictions is over.

Thus, new methodologies will be required. Reliance on statistical estimation of parameters in multivariate models may have to move over to allow room for new approaches to theory validation, if only because the number of parameters will be so large, and the functional forms highly non-linear. The work of Jacobsen and Bronson [1995] points in at least one promising direction.

Preoccupation with these tasks of theoretical synthesis, methodological advance, and empirical testing will carry demographic science into the twenty-first century at a new level of scientific maturity and of policy relevance.

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