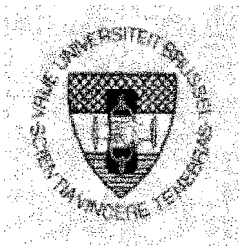

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Imre Lakatos' views on theory development: applications to the field of fertility theories

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1. Introduction

In 1992, Geoffrey McNicoll drew up "An Agenda for Population Studies" in which he warned us about the possibility that demography could be contracting to "its accountancy core", i.e. to its mere technical and statistical home base. Such a fear was obviously inspired by a further "encroachment" of other social sciences such as sociology, history, anthropology and especially economics into the classic domain - largely defined by a small set of dependent variables - of demography. Nathan Keyfitz in 1984 felt similarly that "...demography has withdrawn from its borders and has left a no man's land which other disciplines have infiltrated".

We share no such fears and voice no such complaints. Rather, such "encroachments" are felicitous for the social sciences for they open up new opportunities for advancement. McNicoll too notices these expanding opportunities: "Rounded explanations, cross disciplinary range, awareness of theoretical frontiers and historical contingency, and the critical stance that an outsider can bring to self-regarding disciplinary cultures all can work in favor of demography as serious social science, as well as (not to be scorned) a neat ordering of events on the Lexis plane".

Yet, in surveying several theoretical papers produced in the 1990s, we have found quite an array of reassessments of demographic theories (e.g. Smith, 1989; Burch, 1995, 1996, 1997; Hobcraft & Kiernan, 1995; Kirk, 1996; van de Kaa, 1996; Pollak & Watkins, 1993). In this paper, we shall limit our attention to the subfield of fertility theories, but it is clear that also theories of migration, for instance, are facing similar challenges (cf. Massey et al., 1993). In the reassessments of fertility theories, we detect three types:

(i) *"disciplinary soccer games"* where demography, by virtue of its multidisciplinarity, becomes the arena for opposing theories and interpretations. In itself there is nothing wrong with testing theory H_1 against theory H_2 or empirical evidence E_1 against E_2, provided of course that H_1 and H_2 or E_1 and E_2 are mutually exclusive. But such soccer games deteriorate when opponents settle old scores, when positions become entrenched, and when science becomes a question of belief or commitment. This often leads to the presentation of methodologically seriously flawed research which is supposed to "probabilify" the supremacy of one paradigm or disciplinary approach over the other (see also T. Burch, 1996).

(ii) *"the show room approach"* in which the different approaches and paradigms co-exist and are presented as separate narratives, as distinct "good stories". D. van de Kaa (1996) describes a "good narrative" by the following two characteristics: (a) an easily identifiable central action (such as a statistical account of differentials during a fertility transition) and (b) a setting which allows for an easy interpretation of that action (such as a "wealth flow" reversal). Still according to van de Kaa, fertility theories have not progressed much beyond "anchored narratives" which live side by side. The adjective "anchored" refers to

the plausibility of "probalifying" evidence, and a well-anchored narrative typically consists of a set of sub-narratives that tell the story in greater detail. Such sub-narratives are, according to this author, often nested and they can be ordered hierarchically from the "general" to the "highly precise or detailed". The question is, of course, whether there exist ways of integrating such separate narratives or whether the social sciences, including demography, will never achieve a sufficiently high degree of sophistication that lifts them beyond anchored but still compartmentalized narratives.

(iii) *"the jig saw puzzle" approach*, in which we have no a priori grand view of what the picture represents, but a growing collection of pieces of evidence (E_1, E_2, E_3...) and theoretical insights (H_1, H_2, H_3...) from which we proceed through cumulative learning. Rather than seeing paradigms as hostile species or as living in separate niches, they are viewed as potential complements. Occasionally pieces of the puzzle are (temporally) discarded because they don't fit anywhere, or because they rest on weak evidence or on flawed methodological logic. Several strategies can be followed, ranging from building merely conceptually integrative frameworks to attempts at formal model specification and expansion. In this approach, variables and pieces of evidence are strictly judged on the basis of their merit and never in terms of allegiance, belief or commitment.

The distinctions that we have made so far are in fact nothing more than a popularized version of a great debate in epistemology during the 1960s and 1970s on the subject of "the growth of scientific knowledge". We shall now turn to the key issues of that debate, and assess where we stand as regards the knowledge of fertility change against the background of this discussion.

2. The debate on the growth of scientific knowledge

"For centuries knowledge meant proven knowledge - proven either by the power of the intellect or by the evidence of the senses" writes Imre Lakatos (1970). But this wisdom was seriously undermined by Newtonian physics first and by Einstein thereafter. Few philosophers or scientists today believe that scientific knowledge is only proven knowledge, i.e. that nothing exists without absolute proof. Newtonian mechanics and theory of gravitation constituted one of the best corroborated theories of all times, and yet, they were swept away by relativity and quantum physics. With it, the classic structure of intellectual values had to be replaced. Karl Popper's answer was to abandon the strategy of solely mustering positive "proof" in favor of one's own position, but to look more systematically in the direction of falsification or conditionality as a means of safeguarding intellectual honesty. In Popper's view, commitment to a given position was a crime, and because of ideological school formation being rampant in the social sciences, he had only a scant regard for them:

> "In fact, compared with physics, sociology and psychology are riddled with fashions and uncontrolled dogmas. The suggestion that we can find anything here like 'objective, pure description' is clearly mistaken. Besides, how can the regress to these often spurious sciences help us in this particular difficulty?" (Popper, 1970: 58).

In this sentence, Popper is not only reacting to the mass mobilization of students during the late 1960s by "social science gurus", but particularly to the position of Polanyi and Kuhn on the nature of scientific discovery. According to Thomas Kuhn, there would be long periods of "normal science" during which evidence is mustered as needed for the support of dominant theories, i.e. "science as usual", but these periods would be interrupted by acute moments of upheaval that cause complete paradigm shifts. For Popper, science is in a permanently changing state thanks to scientific criticism. For Kuhn, paradigm shifts are sudden and often connected to extra-scientific events. Kuhn's critics accuse him of establishing "truth by changing consensus" (Lakatos, 1970:92) and of bringing the logic of scientific discovery into the field of sociology of knowledge. Says Lakatos (1970:93):

> "For Kuhn scientific change - from one paradigm to another - is a mystical conversion which is and cannot be governed by rules of reason and which falls totally within the realm of the (social) psychology of discovery. Scientific change is a kind of religious change".

On the other hand, Kuhn (1970a) had accused Popper of "naïve falsificationism", i.e. of proposing that, if scientific testing cannot prove any theory, it can at least disprove it. Kuhn points out that all sciences contain numerous anomalies and that an application of "falsificationism" would leave us completely empty handed. Since there are, in addition, many more false facts than false theories, falsification is equally hazardous. And here we are: we cannot prove theories from facts and we cannot disprove them either. This is a daunting conclusion which leaves scientists, and particularly social scientists, quite perplex. How can we ever progress?

Not being an epistemologist or logician, I have found comfort in Imre Lakatos' notion of "progressive problem shift" (1970:91, 116ff) and L. Jonathan Cohen's strategy (1977) of "inductive knowledge of comparative reliability". The former author derives his notion from studying the great debates and discoveries in physics, the latter is also connected with the standards of proof as applied in courts. And here, there is a link to van de Kaa's "anchored narratives". Such narratives and their supporting facts are indeed the evidence from which courts proceed on the basis of legal procedures. In Cohen's view, the nature of the evidence is similar, but the conclusions are derived on the basis of inductive logic.

3. Theory integration through "progressive problem shifts"

The gist of Lakatos' strategy of "progressive problem shifts" (as opposed to a stalemate war of competing paradigms) is that a scientific theory H_1 can be regarded as falsified only if another theory H_2 has been proposed such that:

(i) the counter-evidence to H_1 is corroborating evidence for H_2;

(ii) if H_2 satisfactorily explains all the empirical successes hitherto explained by H_1 (i.e. partial inclusion of H_1 into H_2);

(iii) if in addition H_2 is capable of explaining or predicting new facts or facts that were unlikely or impossible according to theory H_1 (i.e. enlarged content of H_2).

Suppose that E_1 states all the successful results of performing the most thorough tests on i correlational generalizations $U_1 \ldots U_i$ in the field of theory H_1, then the appropriate support function would be $s[H_1, E_1]$.[1] Suppose now that all these correlational generalizations are also derivable from H_2 and that a further correlation U_{i+1} is derivable from H_2 but not from H_1, and that E_2 reports nothing but the successful results of tests $U_1 \ldots U_i$, U_{i+1}, then we would conclude that $s[H_2, E_2] > s[H_1, E_1]$, and that $s[H_2, E_2] > s[H_1, E_2]$ since E_2 is not fully supportive of H_1. The above outcome typifies Lakatos' "progressive problem shift". By contrast, "degenerating problem shifts" would arise if the counter-evidence to H_1 is not corroborating H_2, or if H_2, although it predicts new facts, fails to account for all the empirical success of H_1. In the latter case $s[H_1, E_2] \geq s[H_2, E_2]$.

The "progressive problem shift" involves more than just superiority of evidential support from known facts. The new theory must also be consistent with or predict new facts. Or, as Cohen (1977: 159) puts it, the discovery of a new regularity on top of the main regularities is in effect the discovery of a new relevant variable, which may well put us on the road toward yet a new discovery based on U_{i+2} and leading to E_3 and possibly H_3. Progressive problem shifts may then result in a cascade of new findings, new avenues of investigation, and ultimately in more comprehensive theories.

Lakatos' framework does not bar the possibility that a research program might remain "progressive" even when confronted with facts that are anomalies for it. The discovery of an anomaly does not automatically mean that an avenue of research is to be cut off. It would be too big a leap to suppose that a theory has always zero support on any evidence that contradicts it. In fact, it is likely that any theory with ample support and no known anomalies may have unknown ones. As Cohen points out (1977:164) it would be rash to suppose that unknown anomalies would simply be non-existent. Hence, a theory H_2 may be decidedly more reliable than theory H_1, without having to imply that H_2 is totally true and has no counter-instances in space or time. Even when

counter-evidence to H_2 becomes available, it may still be that H_2 is more reliable than H_1. In other words, a theory is not immediately killed by the existence of an anomaly if there is no superior theory to replace it.[2]

In the next section, containing demographic examples, we shall not be able to follow Lakatos' or Cohen's schemes with complete rigor, but we shall adhere to the principles as closely as possible. We shall pay particular attention to the principle of partial inclusion of one theory into another and the derivation of more comprehensive theories with enlarged content.

4. Example 1: theories of the "Second Demographic Transition" in the West

The term "second demographic transition" has been coined as a convenient label to describe the various changes in family formation, in union dissolution and in patterns of family reconstitution in the post-war era in Western nations. The changes in family formation operated through the postponement of marriage, the rise of single living, cohabitation, or prolonged residence in the parental household, the "baby bust" and the increased procreation within consensual unions. Patterns of union dissolution were characterized by rising divorce rates and high separation rates of cohabitants. And the forms of family reconstitution were shifting away from remarriage in favor of post-marital cohabitation as well. Although large national differences exist with respect to the incidence and timing of these phenomena (Lesthaeghe, 1995), most changes have consistent international trends. These trends have been explained from three angles: (i) via the *theory of increased female economic autonomy* (G. Becker, 1981), (ii) via the *theory of relative economic deprivation* (R. Easterlin, 1976; Easterlin et al., 1990, 1991), and (iii) via the *theory of ideational shift* (e.g. Preston, 1986; Lesthaeghe & Meekers, 1986; Thornton et al., 1987; Lesthaeghe & Surkyn, 1988; Bumpass, 1990).

G. Becker's neoclassic economic theory posits that increased female education and the resulting opening up of better employment opportunities for women made them considerably less dependent on the traditional forms of household formation for their economic security and dramatically increased the opportunity costs associated with household tasks and childbearing and -rearing. Market facilities were substituted for these tasks. R. Easterlin's theory of relative deprivation points out that the features of the "second demographic transition" are caused by the tension between high consumption aspirations of incoming cohorts, as "learned" during the formative years, and the less propitious employment and career opportunities developing since the 1970s. Linked to this is a demographic factor, i.e. cohort size, which codetermines cohort economic opportunities.

The theory of ideational shifts also links the features of the "second demographic transition" to long term trends toward more individual autonomy in ethical, religious and political domains. Typical expressions of this trend are the steady rise of secularism over the last two centuries, the growth of emancipation movements operating first in the domain of social stratification and then in the area of gender relations, the rise of "postmaterialist" preoccupations in each successive birth cohort since the start of the century (Inglehart, 1990), and more recently the growth of skepticism toward institutions and a weakening of civil morality. Diminishing acceptability of institutional regulation in the sphere of family life is part of the general trend, which means that individuals are allowed to opt out much more freely from any arrangement if the returns are judged to be inadeuate.

Each of the theories can draw on empirical evidence. For instance, premarital cohabitation in many countries started among the better educated segments of the population. This is compatible with Becker's view that educated women would be less likely to opt for a "standard marriage" since they had less to gain from it. The finding is equally compatible with Valerie Oppenheimer's view that premarital cohabitation is merely the result of prolonged dating or a longer waiting time before a suitable partner was found. Increased female education would have increased the standards of what constitutes a "minimally suitable match" (V. Oppenheimer, 1988). It is equally compatible with the rise of new youth cultures and anti-institutional political orientations of the "new left", which typically recruited among the better educated. Also "women's lib" started from the college grounds in the late 1960s.

Also compatible with Becker's thesis is that fertility during the 70s and much of the 80s is strongly inversely related to female employment. This still continues to be so in many countries. Evidently, opportunity costs must weigh quite heavily in the cost-benefit calculus. But this is equally compatible with Easterlin's position in which female employment has become essential to compensate for the weaker earning position of men and to safeguard the material standards of living. With further increasing consumption aspirations dual income earners are far better off to satisfy their material needs. In this fashion the competition between consumption and children is continued. But the link between fertility and female employment is equally compatible with the ideational shift theory. First and foremost, growing numbers of girls were allowed to proceed with their education during the 1950s and 1960s, inter alia, because their parents were no longer subscribing to classic patterns of gender discrimination and because of the weakening of religious influence that had hitherto strongly defended the position of women as homemakers and procreators. Secondly, the ideology stressing individual fulfillment saw female employment as a means toward this more general end.

Rising divorce rates and declining remarriage rates draw equally on all three theses. In the neoclassic view, female economic autonomy allows for easier opting out and for subsequent financial independence. In the relative deprivation theory more precarious employment situations would equally foster marriage instability. The ideational shifts theory predicts higher divorce and subsequent postmarital cohabitation not only on grounds of weakened institutional support, but even more so because ethical autonomy leads individuals more directly into a more "naked" cost-benefit calculus and because the "quality standards" of marriage have increased. Individual fulfillment or self-actualization and the give and take requirements of marriage are not easily reconciled.

Up to this point, we seem to have three theories which draw support from a common body of empirical evidence. Hence we are faced with a collection of facts and three different narratives of how these facts have come about. How do we decide between them, when the situation is as follows: $[H_1, E_1]$, $[H_2, E_1]$ and $[H_3, E_1]$?

There are several paths of investigation that can be followed, each leading to a specific diagnosis. The following outcomes are possible:

a) H_1, H_2 and H_3 can be brought together in a *new overarching multi-causal theory*. In this instance, the mechanisms operating in H_1, H_2 and H_3 were never independent to start with, but causally connected. For instance, a mechanism specified in H_1, when operative, could also trigger another mechanism described in H_2, which in its turn activates a mechanism in H_3. Any such configuration would be the hallmark of the existence of H'. Under these conditions the three theories could be rolled into one, so that $s[H', E_1]$ is superior to either $s[H_1, E_1]$, $s[H_2, E_1]$, or $s[H_3, E_1]$. H' then includes all relevant successes of its parts, and it is a new theory with an enlarged content. It is also likely to alter the predictions that were made by its three predecessors separately thanks to the introduction of causal connections between them.

b) The evidence E_1 on which H_1, H_2 en H_3 are based is not refined enough. Upon closer inspection, E_1 contains distinct sets of correlational generalisations $[U_1, U_2...U_n]$, $[U_{n+1}, U_{n+2}...U_t]$ and $[U_{t+1}...U_z]$. Each of these sets specifies distinct mechanisms that may be operating independently, either in separate subpopulations or "niches", or in a parallel or simultaneous way over time. The failure to recognize heterogeneity or simultaneity had led us to believe that the three sets of correlational generalizations were only one set, which, upon closer inspection, they are not. In this instance, the outcome is that H_1, H_2 and H_3 remain *distinct but not mutually exclusive theories*, and they need to be represented as $[H_1, E_1]$, $[H_2, E_2]$ and $[H_3, E_3]$. They are applicable to specific and more limited contexts.

c) The third solution is an intermediate one. Causal connections between the three theories can be made, so that there is an overarching general part. The mechanisms described in each are not fully independent or autonomous. But, in some specific context, a mechanism specified in H_1 may be a better predictive tool than other mechanisms specified in H_2 or H_3. The result is a *multi-causal theory with strong contextual variations*.

d) The deadlock can also be broken by the "discovery" or introduction of a fourth (or subsequent) theory H_4 which specifies a *"missing link" mechanism*. Via this missing link, either new connections between H_1, H_2 and H_3 become visible, or the contextual domains, in which each of them operates differentially, are being brought together. Models of diffusion and imitation often specify such missing links. Their introduction alters predictions because the hitherto segregated contextual niches weaken or vanish altogether. But also the opposite may occur: new niches may develop in a population as a reaction to increased homogeneity. In this instance, we must again redirect our conclusions away from solution a) and prefer solution c).

In what follows, we shall try to document that the three theories of the "second demographic transition" have stronger connections than admitted by their "founding fathers", so that we have indeed a multi-causal theory. But solution c) is more likely to be the appropriate outcome than solution a), because of the existence of contextuality or path dependency.

To document these points we shall use a few examples. The first pertains to the role of female education. In the post-war era, the continued education of daughters was not only a function of their fathers' income but also a function of fathers' education. Social classes form subcultures (e.g. Kohn, 1977; Kohn et al., 1986) with different "tastes" as to what are the appropriate ultimate goals in life (cf. Rokeach, 1979). Among women, daughters of educated secondary elites (e.g. school teachers, government employees) were the first large groups to move into higher secondary and college education. Such secondary elites were of course the first ones that could benefit from increased opportunities if the channel of education was available as a means of upward intergenerational mobility. Further education for *sons* would not have been an anomaly. Hence, especially prolonged education for daughters cannot be seen exclusively as a human capital investment but also as a cultural good in its own right. This activated both the neoclassic economic mechanisms (i.e. subsequent increased female economic autonomy and rapidly rising opportunity costs) as well as the ideational response (expressive individualism). This joint occurrence constitutes a major interaction term with considerable predictive power: for as long as we have equal education standards for both sexes and for as long as the highly educated also form a cultural elite in tandem with being an economic one, chances are that fertility will remain below replacement level and that the other outcomes of the second demographic transition will be continued. But there is also a link to Easterlin's model. The increased female labour supply has contributed to the shrinking of opportunities for men and hence to an increase of their level of "relative deprivation". This labour force supply factor is at least as powerful as the effect of increased cohort size, which featured so prominently in Easterlin's mechanism. In fact, cohort size may not have been that decisive after all (cf. Pampel, 1993; Pampel and Peters, 1995). Also several European countries never had "baby booms" comparable in size to the American one, yet they experienced the features of the second demographic transition to a higher degree. More basic to Easterlin's explanation is the factor of rising consumption aspirations. Relative deprivation is measured against the rise in the standards of living of the parental household, and it is therefore in part also an outcome of the socialization process. But, much more than consumerism is being transmitted during the "formative years". For instance, the content of educational values to which the "baby boomers" were submitted had equally been altered, and quite dramatically so. Ever since the Lynds had measured the socialization values in their Middletown studies, there had been a trend away from values stressing conformism (religious faith, obedience, order and neatness, hard work) in favour of values stressing autonomy and expressive individualism (thinking for oneself, imagination, independence, being interested in "how and why", etc.) (cf. D. Alwin, 1990). Hence, both the utilitarian (consumerism) and the expressive forms of individualism were simultaneously activated. Also this joint operation calls for a major interaction effect in the equation. Hence, the ideational shifts, which were already emerging prior to the post-war period, form synergistic relations with mechanisms identified by both Becker and Easterlin.[3] Moreover, the two economic theories are interconnected as well. Not only was the increased female labour supply an element contributing to increased relative deprivation of men (and vice versa), but if there is increased consumerism (see Easterlin and Crimmins, 1991), then the calculus of material advantage must occupy a more prominent central place. In other words, if money and consumption matter more, then so do opportunity costs.

The examples that we have just given illustrate that there are definite connections of a causal (inclusing recursive causation) or interactive nature between the mechanisms specified in H_1, H_2 and H_3 respectively. The narratives of the three witnesses are not fully distinct. When put

together, they argue in favour of a stabilization of the features of the "second demographic transition", and not in favour of a cyclical return to the patterns of family formation and dissolution that the West has known till the 1960s.

The next example pertains to the issue of heterogeneity in the population and the fact that theories may address specific mechanisms that are more recognizable in one context than in another. In such instances, the claims of universality of a theory could be leading to erroneous predictions. A good example of this can be found in the topic of union formation. Becker's theory posits a positive correlation between female education and premarital cohabitation: persons with more human capital have more economic autonomy and have less to gain from marriage. Easterlin explicitly expects a negative correlation: lower educated segments are most deprived in the job market and they would be constrained to opt for a longer stay in the intermediate states such as cohabitation. The ideational theory is less differentiated by social class or education, and predicts cohabitation more on the basis of value orientations (religiosity, forms of expressive individualism, etc.). But, if pushed it would connect expressive individualism to higher education, and predict cohabitation to spread out starting from the better educated.

Confronted with data on cohabitation for the 1990s, education may not be a very helpful independent variable anymore, as was for instance found in the analysis of data for France, Germany, the Netherlands and Belgium (Lesthaeghe and Moors, 1994). There was no clear patterning in the logistic coefficients that connected educational categories for men or women to the alternatives of cohabitation and marriage. The conclusion could have been that the two economic theories are not very helpful. But the test is too weak and the evidence too rough. Over time, there is other evidence showing that premarital cohabitation often started among the college educated in many Western societies, but not in all (not in Sweden, for instance, where the pattern had survived in other niches of the population). In the four countries concerned premarital cohabitation did originate among the better educated, and specifically among those with political preferences for the "new left", the "countercultures", the Greens etc. Why then had education become a weak predictor by 1990? A historical reconstruction helps. C. Villeneuve-Gokalp (1990) shows for France that cohabitation at first rose steadily for children of upper class fathers, but that shortly thereafter, the same trend materialized for children from working class and farming backgrounds. By the end of the 1980s, the second group had caught up with the first. A similar conclusion was reached with the 1991 census data for Belgium (Surkyn and Lesthaeghe, 1996). Looking at income structures and housing characteristics of cohabitants, it became clear that there are really two types of cohabitants. At one end of the scale, there were cohabitants who relied quite heavily on unemployment benefits as their sole or as a supplementary source of income, and who, compared to the 1981 census, were disproportionately pushed into the lower echellons of the housing market and renting old appartments. These were often persons under age 30 who had not established themselves economically. Clearly, the Easterlin version works here, but the link between education and the finding of a suitable job is now less obvious than two decades ago. At the other end, there is also a group of wealthy cohabitants, typically without children, who had progressed most as owners of new houses between 1981 and 1991. In fact their ownership rates of newly built homes had risen more than among the married couples, with or without children. This is obviously the pattern that Becker had in mind. Moreover, the poorer group of cohabitants was overrepresented in Wallonia, and the prosperous group in Flanders, which matches the current economic situation in the two parts of the country. From this, one would no longer conclude that the economic theories are useless, or that by virtue of opposite predictions based on education, they would be mutually exclusive. Rather, they were not incompatible to start with, but respectively describe distinct mechanisms that operate in different population segments. Moreover, the use of both theories explains the evidence better than the use of just one. In this instance we have no overarching multi-causal H' in which the mechanisms of H_1 and H_2 are causally or interactively connected, but two distinct and not mutually exclusive explanations that operate in different contexts. As a result, it is not because of one theory predicting a positive correlation and the other a negative association that one of the theories must be false.

At this point, one can also introduce the argument of diffusion. Patterns starting among economic or cultural elites often spread from top to bottom along the social stratification scale. Hence, Easterlin's explanation for cohabitation among the more economically deprived could get a competitor: cohabitation has spread to the lower strata as a result of imitation. But, an equally feasible model is the one with an interaction term for the synergistic operation of economic

constraints and cultural imitation. These two are again not mutually exclusive, but can operate together and reinforce each other.

The examples that were elaborated here show that Lakatos' three point strategy for theory integration may still be too simplistic or that social science evidence is too weak, but it still sets us firmly on a much more promising road than the one offered by the "disciplinary soccer games" In addition, it also illustrates that the originally segregated narratives can be dissected in such a way that causal or interactive links between their mechanisms become visible. Yet, the outcome may not exactly be the overarching H' that one may have hoped for, but still a multi-causal configuration based on empirically grounded complementary explanations.

5. Example 2: looking back at the European fertility transitions

The European fertility transitions have been a major source of inspiration for empirical and theoretical work alike. Along with several refinements[4], there has also been output that does little more than continuing a "disciplinary soccer game". Invariably, the Princeton project (EFP for short) is the soccer ball that is being kicked in all directions. To some, the EFP would have shown that industrial development was not a prerequisite for a fertility decline, and consequently, that family planning programmes in LDC's could achieve what coïtus interruptus had done in the 18th Century France. To others, the EFP was some misguided conspiracy convened for the sole purpose of promoting cultural explanations. In the process, an image is being created, as typified by the following quotations (italics added):

"While cultural factors are not to be ignored, *relegation of economic variables to interpretative limbo, per Princeton project*, appears unjustified" (Galloway et al., 1992: 19).

or even stronger:

"The EFP formulated *narrow* propositions that were supposed to be *generally* valid, in support of an *exclusive* focus on cultural ideational change as *the* determinant of the timing of fertility transition" (Friedlander et al., 1996: 32).

The more careful reader who checks his sources might come up with some different opinion. Firstly, he would notice that all EFP monographs were trying to create ecological proxies for changes in *the costs and in the utility of children*, which, incidentally, is the core of the Easterlin-Crimmins framework. The first sets of variables used in the EFP were typically indicators of industrialization, urbanization and educational advancement. These processes were assumed to alter the cost-benefit calculus quite dramatically. Wherever possible, operationalizations were further refined by taking more specific indicators of changing occupational structures into account. For instance, we find indicators that target the survival of familial modes of production by also incorporating employment in cottage industries. Other indicators contrast the employment in the newly developing sectors to that in the pre-industrial sectors by taking logarithms of their ratio. Forms of land tenure, inheritance patterns, real wages and income distribution are also occasionally introduced or discussed. Via literacy levels, or wherever possible, via proportions reaching secondary education, connections were made to the "*child quality*" argument and to the role of education in promoting upward *social mobility* in settings characterized by *new structures of opportunity* (cf. Arsène Dumont's "social capillarity" or the newly styled "quality-quantity swap"). Via infant mortality rates and their declines links were studied to the "*supply of children*" variable, which is another Easterlinian input.[5] Even breast-feeding patterns are discussed in this connection. *Diffusion patterns*, when introduced, are studied in function of linguistic demarcations and the degree of language heterogeneity. Not many others had done this prior to the EFP. And, finally, only in the studies of M. Livi Bacci and of the present author, attention is being paid to patterns of *political organization and ideological mobilization*, and hence to the role of *secularization*. Is such a broad script really "a relegation of economic factors to limbo", or "an exclusive focus on cultural-ideational change"?

Secondly, the connections, both historical and statistical, between the various independent variables were often studied in the EFP with far greater sophistication than has been the case in several later studies.[6] Mechanistic multivariate analyses that leave the issue of multicollinearity unresolved were systematically avoided. Any correlation between two predictors is being interpreted in its historical context. In short, the various narratives were often closely connected, mostly via descriptions of historical path dependency first, and by introducing patterns of hierarchical causality between the independent variables subsequently.

Thirdly, the EFP frequently follows the strategy of falsification rather than solely relying on "probabilization". The strategy of playing the devil's advocate is frequently adopted. For instance, the non-redundancy of the secularization dimension is checked by loading the dice explicitly in favour of the structural economic explanation (Lesthaeghe and Wilson, 1986). Strategies based on the use of partial and part correlations, typical for the EFP, are seldomly encountered in subsequent work.

Fourthly, the EFP is at times disconcertingly empiricist. There were no "pet explanations" to start with, and the authors involved in the project were often dabbling in very different waters. If anything, the leading "theoretical" guideline that emerged was that simple monocausal explanations would fall considerably short of the intellectual standards needed to come to terms with complex and intertwined historical processes.

Of course, the EFP has its limitations. The most serious one is that so much of its evidence is solely based on ecological correlations. But, have other types of studies offered a solution? Studies of demographic transition of a single village or town population have definitely contributed to a better "ground level" understanding of local systems. But "systemic understanding" was also the aim of the EFP; only, changes in nation-states were addressed and this is not less relevant. Another feature of village studies is that they only possess fragmentary individual-level measurements. More often than not, they too have to rely on *aggregates* such as social class (or more rarely ethnic group or religious denomination). And since information at the individual level on demographic phenomena (parity, birth intervals, infant deaths, marriages) are at most only matched by similar data on occupation and literacy, explanations must be in terms of social stratification. The rest disappears in the large pool of unobserved heterogeneity. Hence, the price that village or town studies had to pay was that of *reduced variance*. There are two aspects to this problem. First, other villages or towns are rarely introduced as points of comparison, and consequently, the "no control, no conclusion" outcome is just around the corner. Second, statistical artefacts of specific "ecological clustering" will emerge. By this we mean that a reduction to a single smaller geographical aggregate not only means a loss of variability in general, but also that the variance of some variables will shrink more than that of others. Variables with a high degree of homogeneity will simply drop out from the analysis. Hence, the choice of a particular setting, such as a village or town, is definitely not neutral in terms of the outcomes. A "battle of conflicting outcomes" is quickly generated depending on the chosen settings. Incidentally, that also holds for countries, and not just for villages or towns.[7]

A second criticism addressed to the EFP is its use of simple cultural markers such as ethnicity, language or religious denomination (cf. Hammel, 1990; 1993). These are indeed easy to come by in the published 19th Century censuses or related data sources. Says Hammel (1980:2) (italics added):

> "Over the last 40 years, anthropological theory has moved away from the institutional, structural-functional approach it has long presented to its sister social sciences, toward the elucidation of *local, culture-specific rationalities*, in the building of which actors are important *perceiving*, *interpreting* and *constructing agents*. This change in direction has been ignored by the "economists", who continue to insist on a universalistic individual rationality, and by the sociologists, who continue to underplay the *agency of actors* in the *construction* of culture".

Obviously, ethnicity, language or religious denomination as dummy variables tell us nothing about these ongoing processes of interpretation, perception and recreation of rationalities. But, just throwing them away would be an opportunity foregone. If cultural construction by actors is not merely an individual affair but also a *group activity*, chances are that new interpretations are

growing within contexts demarcated by ethnicity, language, religion, or for that matter social class as well. The "perceiving, interpreting, and constructing agents" are not little atoms popping up on the screen at random or flying all over. They come in *clusters* to start with, and seek to form larger *organized clusters* subsequently. Moreover, Hammel leaves no room for *institutional agency*. This is just as serious as underplaying individual agency in processes of rapid social change. Consequently, we must equally introduce state and state ideology, communities of believers or churches, rival political parties, power and influence, and forms of political patronage. In short, there is a definite place for political history.[8] Probably, the EFP should be congratulated for having introduced the "institutional, structural-functional approach" (be it in a more Mertonian sense) and its political-ideational derivatives, since otherwise, it would have neglected a salient dimension of 19th Century history. In short, a major narrative would have been missing.

There is much more to this story. The EFP-monographs on Portugal, Belgium and Italy do not just stop with simple cultural markers such as language or religious denomination. Behind these fronts, they identify and operationalize the growing importance of individual ethic and moral autonomy in decision processes, summarily called "secularization". And they place this against the backdrop of political fragmentation and mobilization in these countries. *Here is a "new piece of culture" being "constructed", "interpreted" and "perceived" by individual and institutional actors alike.* An it is also being "reacted to" by institutional rivals with considerable power. Family limitation was to become an explicit topic in this political arena.

To sum up, the introduction of secularization and its operationalization through voting behaviour - itself a product of "new culture", called democracy - proved to be a little nugget for the EFP, not because it would relegate the structural-economic explanations to limbo (it does not at all do that), but because it added a *non-redundant* narrative. Even in the instance of the French fertility transition, which so prominently preceded any industrialization or mass education, the non-redundancy of the secularization factor emerged again.[9]

And so, we're back to Lakatos:

* if H_1 is a structural-economic account of European fertility transitions based on non-refuted correlational evidence E_1 (and the EFP does not refute E_1),
* and if H_2 is a political-ideational account based on equally non-refuted additional evidence E_2,
* and if a new theory H' is available connecting H_1 and H_2 and not excluding parts of E_1 and E_2,
* then H' is a superior theory to either H_1 or H_2.

Coale's expression (1973:69) for a success (S) of a fertility transition depending on "readiness" (R), "willingness" (W) and "ability" (A), i.e. S = RWA, provides the elementary ingredients of a H' theory. This Boolean expression simply states that neither one of the three conditions on its own is sufficient but that the combination of all three is necessary. It therefore provides a starting point for dealing with a fertility transition in a specific context. For instance, R or the economic rationale for fertility control in the French case prior to 1830 may have been conditioned by very different structural factors (such as declining real wages throughout the 18th Century, effects of different agricultural production structures...) than those operative from the second half of the 19th Century onward in the same country or in the other nation-states. The legitimacy of fertility control, W, definitely depended on the political and religious demarcations and evolutions in these European national contexts. Only the history of ability A, i.e. that of contraceptive methods and especially of "regulation costs", has remained too fragmentary so far; but then, this has not been the issue that inspired much of the debate, at least not in the European historical case.

The three narratives of R, W and A cannot just be told side by side. For their inclusion in a H' theory, more specific mechanisms need to be formulated at first within each narrative, and these in their turn need to be linked by additional causal and/or interactive mechanisms. It is this nesting

of the models for R, W and A that is needed to generate an overarching multi-causal theory. If S = RWA is a good starting point, much of the work consists of specifying R⇄W⇄A.

The problem in the social sciences is that we may be able to specify more than one set of models or mechanisms that are all compatible with the evidence. One reason for this, as already pointed out, is that the models and their dynamics are not necessarily mutually exclusive, but can either operate simultaneously or even synergistically, or differentially in separate contexts or niches. A second reason is that our data and proxies do not much more than hinting at relevant variables, but fail to provide insights in the actual dynamics. These dynamics are mostly supplied *exogenously* by motivation-action paradigms (e.g. the Easterlin-Crimmins model) or by models of diffusion, resistance or imitation (e.g. models of cultural mobility or of "contagion"). In many instances, our data are also too parsimonious, and basic descriptive information for one or more of the narratives may be missing.

All of this creates new challenges, but at the same time the simple expression S = RWA warns us against "soccer game" statements about the primacy of one of the conditions when the narrative on any of the remaining conditions is inadequately documented or not even researched.

6. The challenge ahead: the dynamic integration of S = RWA

The integration of the narratives subsumed by the joint conditions of readiness, willingness and ability can be in the form of a verbal elaboration or it can also proceed via a more formal representation. T. Burch (1996), for instance, has tried to link the Easterlin-Crimmins model to that of Rosero-Bixby and Casterline (1993) and even made a Dynamo Plus application for the data of the Taiwan, spanning the period 1957-63. The challenges inherent in the construction of a H' theory clearly lie in this direction, and if computer modelling can help, all the better.

There is obviously plenty of work ahead, and in this last section of the paper we can do no more than present a discussion and a few hints.

First, we shall make a distinction between *instrumental* and *procedural* rationality. In the definition of Hargreaves Heap (1989:39) "instrumental rationality equates the 'rational' action with the choice of the means most likely to satisfy a given set of ends". The instrumental account of rationality has obviously proved to be a powerful generator of explanations in economics, and the core of the Easterlin-Crimmins model is a perfect example of it. Fertility control (means) is adopted when the *motivation* for control (itself a function of income, costs of children and utility of children all relative to other goods) exceeds the *costs of fertility regulation*. The motivation for control clearly belongs to Coale's R-condition, and the costs of fertility regulation to the A-condition. But Easterlin equally emphasizes that the key variables in the cost-benefit calculus are *subjective* perceptions of couples. They have to anticipate what the future supply of children would be. They have to formulate expectations about the costs of each additional child and about its economic and psychological utility. And all this is being evaluated against prospective income and material consumption aspirations. Clearly, we have a striking case of Leibenstein's "bounded rationality" (1975), since nobody ever possesses enough information to come up with a reasonably watertight cost-benefit calculus of this sort. Under circumstances of limited information, actors commonly resort to *existing scripts as shortcuts*. From this point onward, procedural rationality equally applies. In Hargreaves Heap's definition (1989:116): "procedural behavior is defined as action which emanates from the use of procedures or rules of thumb". In their environments, actors may have only one dominant script, supported by tradition and institutionalized norms, or they may have a variety of alternative scripts at their disposal. Hence, the *density of alternative scripts* as beacons for procedural rationality needs to be introduced as well, since plain instrumental rationality based on full information is an illusion. Several of these factors and concepts will be used again at a later stage.

We shall now turn to the matter of diffusion. The "adoption-diffusion" debate is another one based on the misunderstanding that one excludes the other. Any diffusion can take place once at least one group in the population has adapted to new circumstances. But from there onward, both adaption and diffusion are likely to occur simultaneously and to reinforce each other. Hence, we

take it that diffusion models are equally non-redundant ingredients of the narrative. The Rosero-Bixby and Casterline model (1993) of "interaction diffusion" is a start. It addresses only one component in the Easterlin model, i.e. that of the cost of fertility regulation. According to the model, couples move through three stages, (i) the "natural stage" in which they have no interest in controlling fertility for whatever reason, (ii) the "latent stage" in which they want to control fertility but are still not doing so, and (iii) the control stage in which they had become contraceptors. The proportions that move through these stages are dependent on (i) the intensity of interactions with controllers and the probability of "contagion" per interaction, and (ii) on the number of couples already in each of the three stages. The Rosero-Bixby and Casterline model is, as said, only applied to the diffusion of fertility regulation and it has only one basic mechanism, i.e. that of contact with growing numbers of "infected" couples. Of course, diffusion is not necessarily linked to a variable that comes at the end of the story. Ideational change has its diffusion too, and so do changes in preferences, changes in the legitimacy of a cost-benefit calculus, changes in perceptions etc. In other words, *all enabling conditions* subsumed in Coale's R and W *are subject to diffusion as well*, and these would in their turn largely determine the diffusion probabilities of the final action outcome, namely fertility control. Secondly, the contagion model based on contact and networks needs to be complemented by *other axes of diffusion*. The axis of power, influence and political mobilization is definitely one we cannot skip. Experiences in European historical settings and from some contemporary Asian countries quite clearly show the importance of such factors. And thirdly, we cannot divorce diffusion from adaptation altogether. Diffusion will be facilitated in population segments that are ready given their structural economic and ideational conditions.

At this point a conceptual scheme is necessary. Figure 1 is an attempt in this direction. The scheme has four blocks dealing respectively with:

1. The initial conditioning and controlling factors operating at the macro-level, in which we distinguish again between ideational changes and their own diffusion, the institutional setting and its alterations, structural economic change, and alterations in other variables of the demographic regime (e.g. in mortality, migration, nuptiality or household structures). These four factors are linked between themselves: for instance, institutional settings control ideational change and structural economic change and they are themselves conditioned by them. The same control/conditioning relationship equally applies to the pairwise relations between ideational change, demographic alterations and structural economic change. In this section we essentially write a *political-economic and demographic history* of the case at hand.

2. The areas of adaptation, in which we distinguish 5 areas operating at the micro-level. Adaptation is to be understood as changes in these 5 areas resulting from changes in the macro-level conditions.

2.a.The first area is that of *changing preferences*. In this respect we would adopt the Maslowian model of hierarchical needs. If structural economic change leads to increased real incomes, there would be a gradual shift from the lower echellon to the next higher. In this situation children would compete with different goods, and more and more with luxury goods, forms of conspicious consumption, or with the "higher order needs" for greater self-realization of adults. Similar shifts in preferences could be equally fuelled by ideational changes, as we have argued before. But also in the instance of stagnating real incomes, consumption aspirations above the levels of "subsistence" and "security" can be accentuated as a result of strong impulses in this direction from outside. Preferences for children would then decline relative to those for new consumption patterns. "Reference group behavior" is a classic mechanism capable of generating preference shifts.

2.b.The second area of adaptation is that of *increased legitimacy of pure instrumental rationality* (see Coale's W-condition). Here we deal with the ethics of solely focussing on the cost-benefit calculation. Much of the secularization argument made in the context of the European fertility transition is based on this adaptive mechanism. The same also holds for the further growth of individualism, both in its utilitarian and expressive forms, during the second demographic transition. In all these instances, institutional moral control retreats. But also changing economic circumstance may have the effect of reducing ethical obstacles. In the instance of a crisis-led fertility transition, for instance, economic deterioration may push couples to a defensive plain economic calculus with respect to fertility irrespective of ethical considerations, traditions or religious fears.

1.Conditioning & controlling factors	2. Adaptation	3. Diffusion	4. Action on outcome variable
Macro-level	**Micro-level**	**Micro to Macro**	
* Institutional settings and alterations * Structural economic change * Structural demographic change (in mortality, migration, nuptiality, density) * Ideational change & diffusion of secularism	a. Changing preferences (hierarchy of needs) b. Increasing legitimacy of applying instrumental rationality to new domains c. Changing perceptions of : *costs, benefits *opportunities, constraints d. Growth of density of procedural scripts e. change in the OBJECTIVE terms of the cost-benefit calculus	Diffusion of the social-psychological factors a through d. * contact density circles, boundaries * power, influence, mobilization, propagation * latent readiness	Fertility control * Ability : fertility regulation costs * Ability : innovation and diffusion of contaceptive methods. * Adaptation : increase of users, fertility decline, spacing, postponement of parenthood, stopping ....

FIGURE 1 : CONCEPTUAL REPRESENTATION OF "RWA-FACTORS" INVOLVED IN FERTILITY CHANGE

2.c.The third area of adaptation is the one of *changing perceptions*. Here we are dealing with the *subjective* image that actors form about the elements of the economic calculus. A first example is that a higher communication density may alter a couples perception of advantages and disadvantages of having (additional) children. Another example deals with the "discovery" of opportunity costs for women during the 1960s and subsequent years. For as long as few women were employed in Western Europe or America, the majority of home makers would not be aware of the value of their human capital or of the cost of time. Only when the numbers of working women increase is there a growing awareness of these factors and of relative deprivation for those still staying at home. The ideational changes too produce alterations in the way people perceive their social and economic situation. Political mobilization in fact often aims at altering these perceptions.

2.d.The fourth area of adaptation is that of *enlarged diversity of procedural scripts for action*. As societies grow in complexity, both structurally and ideationally, traditional scripts are mostly being replaced by more instrumental ones. However, one should not assume any mechanistic relation here. Traditional scripts may continue to exert their attraction for much longer than anticipated. If in the face of rapidly growing diversity, a population segment decides to maintain its authenticity or identity, older scripts may be given a new life. Moreover, scripts can also be imported via the media (see for instance Caldwell's Westernization) sometimes with "fundamentalist" reactions as a result.

2.e.The fifth area of adaptation deals with the *objective terms of the instrumental cost-benefit calculus*. Here we are dealing with the core of the micro-economics of fertility. A crucial model in this respect is the "quantity-quality swap" as it affects the costs and utility of children. Of course, this economic core of individual economic rationality is largely determined by the structural changes at the macro-level.

Ideally, a plausible narrative needs to take all 5 areas of adaptation into account. It also needs to translate the effects of the four initial factors, operating at the macro-level, into these various patterns of adaptation specified at the micro-level. As this is difficult to do with classic statistical data and techniques, additional suggestive evidence can be gathered via qualitative techniques using more in-depth interviewing and via the analysis ofhistorical documents. Despite all the shortcomings of such techniques, they are still a welcome alternative to armchair types of ex post interpretation.

3. The axes of diffusion. The third block in figure 1 deals with the spreading of new preferences, the growth of legitimacy of instrumental rationality, the perpetuation of altered perceptions, and the increase in density of procedural scripts. In other words, these four social-psychological areas of adaptation (a through d in block 2) are subject to diffusion mechanisms as well. The first issue in the context of diffusion is that of *boundaries*. If diffusion takes place via the density of contact, then demarcations between contact density circles will also become natural boundaries for diffusion. Linguistic areas, ethnic groups, social classes, religious groups etc., are all typical cases of such high density contact circles. Patterns of intermarriage would probably be quite useful in demarcating such circles. The second issue is that of *agency of diffusion*. In this respect, individual agency operating via "contagion" or "demonstration" needs to be complemented by institutional agency. By this we mean *organized* form of stimulating diffusion through *mobilization*. A second axis connected to power and influence is therefore essential. The third axis is that of *latent readiness*. By this we mean that structural economic change leading to altered *objective* terms of the cost-benefit calculation may not be directly leading to altered behaviour. Yet, the diffusion of the enabling social-psychological factors, possibly supported by institutional mobilization and propagation, will be considerably enhanced if there is such latent readiness or interest. At this point a readiness for adaptation to altered objective conditions and the diffusion generated by both institutional agency form a synergistic pair. A minimal model of diffusion therefore needs (i) a set of contact density circles (with parameters for contagion or learning) and their demarcations, (ii) parameter adjustments allowing for effects of organized mobilization and propagation, and (iii) extra parameters capturing the degree of latent readiness in each of the contact density circles. Ideally such models need to be applied *first* to the social-psychological factors identified in block 2 of the figure. We then have *models of diffusion of "readiness" (R) and of "willingness" (W)*.

4. Action with respect to the chosen outcome variable. Evidently, any behavioural outcome variable can be chosen here and applications to health or to migration can readily be imagined. But we shall proceed with the issue of fertility control. From the figure it is equally obvious that fertility control is

now a function of "readiness" (R) (change in objective terms of the cost-befefit calculus; latent readiness) and of "willingness" (W) (change in the subjective adaptation factors and their diffusion). The third condition, i.e. "ability" (A), needs to be introduced in the last step. In the instance of fertility control we then focus on fertility regulation costs as influenced by availability and knowledge of contraception, the impact of family planning programs, marketing and distribution techniques etc. At this stage, also diffusion models of contraceptive use provide a logical step in the analysis. The only problem, however, is that diffusion models operating at earlier stages (i.e. in block 3 of the figure) are often being overlooked. In such instances one risks of course to give too much credit to the effectiveness of family planning and marketing programs. But the matter is not as simple as that. Specific action programs operating at the level of the outcome variable may at the same time be forms of institutional agency promoting diffusion of more general changes in preferences, in perceptions, or procedural scripts, and equally contribute to the enhancement of the legitimacy of rational calculus. In other words, action aimed at enhancing "ability" may also produce an increase in "willingness". Such feed-back mechanisms have, to my knowledge, not been studied. The question then is whether family planning or health programs just promote contraceptives and medications or whether they also contribute to increasing overall perceptions of greater self-control, promote higher material consumption aspirations, create new instrumental scripts, and help to legitimize individual freedom of choice.

7. Conclusion

The debate among philosophers of science (mainly T. Kuhn, K. Popper and I. Lakatos) during the late 1960s is of substantial interest to demography. Many issues that were then discussed have not lost their relevance. In this paper, we have made use of Lakatos' principles of theory integration, and applied them to theories of fertility. Both the historical European fertility transition and the contemporary "second demographic transition" have been discussed in the light of Lakatos' principles. We have tried to demonstrate that current demographic theories are largely complementary and not mutually exclusive. The mechanisms they describe are either operating jointly and synergistically or to a differential degree in separate contexts or subpopulations. Hence more fruitful work can be done than continuing "disciplinary soccer games".

The final section of the paper contains a specification of Coale's principles of "readiness", "willingness" and "ability". Here, specific attention is being paid to:

(i) The links of control/conditioning operating at the macro-level between institutional change, structural economic and demographic change, and ideational shifts.
(ii) The socio-psychological mechanisms operating at the micro-level and affecting changes in preferences, the increased legitimacy of instrumental rationality, changes in perceptions of costs/benefits and opportunities/constraints, and the growth of alternative procedural scripts.
(iii) The diffusion of new preferences, perceptions, legitimacy and procedural scripts via mechanisms of "contagion" or "learning" and via mechanisms of mobilization/propagation.
(iv) The innovation and diffusion aspects of "ability"-related variables.

The contextual scheme therefore combines various approaches or narratives which hitherto have largely led their own independent lives.

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NOTES

[1] We are following Cohen's (1977:155 ff) notations from now onward.

[2] The issue of anomalies plays a critical role in the Popper-Kuhn debate. Here we adopt Lakatos' solution to the issue in accepting the theory with the least anomalies as the most reliable one available.

[3] Maslow's needs hierarchy, for instance also fully accepts that shifts in needs are contingent on increased economic prosperity, a feature which is fully evident in Inglehart's cohort profiles of "post materialism". However, the ideational shifts need not be fully synchronized to the economic rise in real income, nor is there an economic threshold beyond which ideational changes are occurring (see French secularization in the 18th Century). Finally, countries can differ quite a bit with respect to their values structures and have only minimal differences in their standards of living. For instance, Switserland scores very "conservative" on many ideational dimensions and on the actual demographic characteristics of the "second demographic transition", and yet it has one of the highest per capita incomes of the world.

[4] Among these refinements we would definitely cite the David, Mroz and Wachter (1984) contribution showing that the parity (or age)-specific control function of fertility needs not have the steadily accelerating from suggested by Coale. This introduces the notion of "anticipatory spacing". Also the diffusion of controllers to non-controllers is taken up explicitly. Other contributions are made by village studies, where the focus is much more explicitly on households, their composition and possible rationalities for further fertility control (e.g. J. and P. Schneider, 1992; D. Kertzer and D.P. Hogan, 1989; G. Alter, 1988), or by studies that target specific occupational groups or social classes (e.g. M. Haines, 1992).

[5] At this point it should be recognized that Chesnais' advocacy of using the decline in childhood mortality ($_4q_1$) should equally be taken into account. The EFP relied too exclusively on infant mortality ($_1q_0$).

[6] The projects on Prussia and Croatia of the Berkeley Group could make considerable improvements in this respect. Particularly the Prussian study is beseiged by large problems of multicollinearity between its numerous indicators.

[7] It suffices to compare the EFP monographs on England, with very little regional variations in demographic outcome variables, and the Belgian monograph with large variations in every single respect.

[8] See for instance S.C. Watkins (1992) or E.A. Hammel (1993). In his Slavonian study (Croat-Serb border), Hammel introduces five major social distinctions (linguistic and religious affiliation, political status as military colonists and civil serfs, urbanity and type of village (wage labour versus agricultural peasants), wealth depending on land ownership, and locality. In fact, Hammel uses cultural markers in the way the EFP did for larger regions. One can only wonder about the "Economics 1, Culture 0" outcome if diversity would also have included Muslim Bosnian villages, or if a new Yugoslav study would be made of villages located along the axis from Slovenia to Macedonia or Kosovo.

[9] For the French fertility decline, see especially D. Weir (1985), and for the statistical analysis incorporating both the economic structural variables and the degree of secularization, see R. Lesthaeghe (1992). Incidentally, it is still amazing that no large volume on the French fertility transition has been compiled.