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'The problem is not with current scientific theories of the world, but with current theories…of what it is to acquire good scientific theories of the world. As is typically the case for individuals, our collective self-knowledge lags behind our collective knowledge of the world.' Ronald N. Giere, *Science Without Laws.*

'Models are to be used, but not to be believed.' Henri Theil, quoted in Thomas H. Wonnacott and Ronald C. Wonnacott, *Introductory Statistics for Business and Economics* [3rd ed.].

Introduction

This paper is as much about the form of fertility theory as its substance. The central argument is that over the last decades, demography's quest for better fertility theories has been hampered by a narrow and constricting view of what theory is, how it is constructed, how it is to be judged, and how it is used. Without detailed evidence -- to the best of my knowledge, the research that would yield such evidence has not been done -- I observe that the discipline has been dominated by the methodological thinking of logical positivism, with intellectual roots tracing back to figures such as Karl Pearson, but most directly influential in the mid-century codifications of Ernest Nagel and Carl Hempel. Demographic thinking on theory has also contained large doses of Karl Popper's doctrine of 'falsificationism,' reinforced by a habitual use of statistical tests to 'accept' or 'reject' hypotheses.

According to this methodological approach, theory is built on valid empirical generalisations. And an otherwise appealing theory can be called into question, or even rejected, in the face of empirical exceptions or counter-examples. Demography's adherence to these philosophical

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doctrines has discouraged the construction and development of new theoretical models, and led to an under-valuation of a large fortune of theoretical capital in the discipline. A central feature of this under-valuation has been the relegation of large parts of formal demography, those dealing with substantive models of population dynamics, to the status of 'techniques' or 'demographic accounting.'

Demography could benefit from a re-orientation of its thinking about theory. A fruitful alternative approach is to be found in what is known as the 'semantic' school of philosophy of science. This view emphasises that all theories are abstract -- and therefore simplified -- representations of some part of the real world. Theories are constructed by the human mind; they do not represent the discovery of natural laws of reality, put there by Nature or by some divine lawgiver. They are not absolute truth. By the same token, they are not false just because they do not accord with certain empirical facts. Since theories are formal systems [whether in words and logic or in mathematics], they are false only if they are logically false. Their empirical 'validity' is a matter of context and purpose. A theory is a good theory if it fits reality a] closely enough b] in certain respects c] for the purpose at hand.

In the sections to follow, I summarise this alternative view of methodology and theory, and discuss its implications for our understanding of demographic theory in general and fertility theory in particular. I shall briefly discuss the relationship of these ideas to Wilson's recent expressions of concern over 'reification' in demography, and to Lesthaeghe's discussion of the ideas of Lakatos and Cohen in relation to demographic theory. In closing, I offer a sketch of fertility theories for the 21st century, with an emphasis on the plural. We may or may not develop a 'unified' or 'unitary' of fertility. But we had better keep our existing theoretical toolkit close at hand, with all our tools sharply honed and well-oiled.

1. Competing Methodological Views of Theory

1.1 Logical Positivism and Falsificationism in Demography

My claim that demographers by and large have been logical positivists in their approach to theory is not easy to document. We tend not to be self-conscious and explicit about scientific methodology [philosophy of science] as opposed to method in the narrower sense of technique. And I know of no systematic work examining the implicit methodology used in demographic research in the last half-century. By necessity today I shall argue by means of illustration and anecdote. Before that, two qualifications are in order. The first is that my remarks do not apply so much to economic demography, since economists have a view of theory somewhat different from that of the other social and behavioural sciences, although certainly affected by logical positivism and the ideas of Potter. But the treatment of economics requires another paper. Second, it may be that many of my younger colleagues in demography have already been liberated from logical positivism. To the extent that is so, the problem may be solving itself through cohort succession.

Demography's preoccupation with empirical generalisations as the foundation of theory is nowhere more evident than in our treatment of classic transition theory. As more and more cases of transition have been documented in more and more detail, there has been a tendency to question its

status as theory, or even as a rough empirical generalisation. The European Historical Fertility Study out of Princeton was a landmark in this respect. Discovery of cases in which fertility declined at the same time as [or in a few cases even earlier] than mortality, or the absence of strong correlations between fertility decline and standard development variables in others, often were taken as grounds for the dismissal of transition theory as theory. The fact that the older theory could not predict timing or pace of fertility decline was taken to mean it needed to be replaced by a new theory.

In his 'reconsideration' of the demographic transition, Coale [1973] reviews the evidence and then turns to 'the generalization that remains.' His new formulation gives the well-known three preconditions for marital fertility decline, while asserting that one or more of them may occur in the absence of modernisation -- 'in premodern societies.' This would seem to be at odds with classic transition theory, which emphasised socio-economic development and modernisation as the driving forces behind fertility decline. But it is difficult to know just what theoretical implications Coale intended, partly because nowhere in the article [whether in reference to Notestein's earlier work or his own] does call use the word *theory.* He speaks of the 'idea of the demographic transition' or the 'basic ideas of the transition' [p.53] but not 'theory.' At one point he asks whether the basic ideas 'are as valid and germane in the contemporary world as they seemed to be when they first became popular' [p.53]. This apparent throwaway line points to a fundamental distinction on which this paper turns: the distinction between validity and applicability of a theory. I return to this point later.

Another example is from the classic paper by Cleland and Wilson [1987]. In their broadside against economic theories of fertility decline, they offer 'counter-examples of moderate mortality but little or no fertility decline [p.18]. And they conclude 'the evidence for England contradicts most major theories of transition' [p.15]. Since a counter-example can only falsify a truly universal statement, it must be that they are thinking of theory based on universal empirical generalisations. And clearly they are following a Popperian approach to theory testing: in this case, they reject economic theories as invalid, contradicted by the evidence. If I seem critical on this point, I am critical also of myself, since as late as my 1996 paper in *The Sociological Quartlerly,* I argued for formalisation of theory in order to make it falsifiable.

Even in cases where a more eclectic, pluralist view of theory is expressed, the notion of eventual falsification of rival theories lingers. Summarising a review article by Massey *et al.* [1994] on migration theory Weeks comments:

> 'Their conclusion was that each of the theories is supported in some way or another by the available evidence and, in particular, none of the theories is specifically refuted. This serves only to underscore the point…that migration is a very complex process. No single theory seems able to capture all of its nuances' [1999, p.248].

Massey *et al.* comment that 'each theory captures an element of truth,' but they associate this with a 'characteristic defect in research….an absence of evidence, not the presence of contrary evidence' [1994, p.739]. The implication seems to be that with adequate research some of the theories would be rejected.

1.2 Theory as a Collection of Tools

Over against the ideas characterised above, one can pose a different view of the nature and role of theory, and its relationships to empirical reality. The central ideas of this alternative view can be summarised as follows:

1] If theory must be based on valid empirical generalisations, then the quest for social scientific theory is a dubious effort since such empirical generalisations of wide scope [much less universal scope] clearly are few in number. Even in physics, the idea that theory summarises natural empirical laws is outmoded and unnecessary.

2] Theory is abstract and formal. Since it is abstract, all theory simplifies. Since it is formal, its validity is formal validity. It is a valid theory if it contains no logical or mathematical contradictions.

3] With respect to empirical reality, the key question is not whether a theory is valid in itself, but how well it fits a particular piece of the real world. The fact that a theory doesn't fit a particular case does not falsify it.

4] A theory is judged not as true or false, but as useful or not for a particular purpose. Its usefulness depends on how closely it fits some part of the real world in respect to features that are relevant to a given purpose.

5] In this approach, theories that have proven useful in the past are retained in a collection of theories -- a toolkit of theoretical tools.

This is an amateur's summary of what some philosophers of science would call the 'semantic' view of scientific theory. It is interesting to note that this instrumental view of theory parallels in many ways the view of the nature and use of models in traditional mathematical modelling.

In social science, an early statement of this approach to science -- specifically directed against the logical positivists -- is a little-known classic by political scientist Eugene Meehan: *Explanation in Social Science: A System Paradigm* [1968]. Meehan argues [against the so-called 'covering law' doctrine of explanation] that explanation is based on formal systems [which he equates with theory, although not liking the latter term]. Explanation occurs when some real-world phenomenon to be explained can be logically inferred from a system that is sufficiently 'isomorphic' with the real-world system in which the phenomenon occurs. Sufficiency is a matter of degree, and depends on purpose. 'Systems are formal logical structures, sets of variables and the rules governing their interactions. One of the basic elements in any explanation, therefore, will be a system. However, since explanations must have objects, must be relevant to something in human experience, each explanation will also involve a *description* which contains the events to be explained. Construed as a process, explanation is the application of a logical system to a description' [p.31]. Note that a system may not work well with respect to a particular description, but, as emphasised above, this does not invalidate or falsify the formal system.

Meehan's early work does not draw explicitly on philosophers of science other than the logical positivists whom he rejects, but later work refers to philosophers of the 'semantic' school. A convincing recent statement of this view of science is *Science Without Laws* by Ronald Giere [1999], a philosopher of science with a physics background.

For Giere, a theory or theoretical model is an abstract representation of reality, one of three kinds of models, the other two being physical models and visual models [maps or diagrams]. Models are 'abstract objects, imaginary entities whose structure might or might not be *similar* to aspects of objects and processes in the real world' [p.5, emphasis in original]. What are called 'laws of nature' may be regarded as 'principles,' but

> 'As such they are not even candidates for being truths about the world. They are not statements, but general rules for the construction of models. Incorporated into the characterization of particular models, however, they do function as true statements, but not statements about the world. They are then truths only about an abstract model. In this context, such statements are true in the way explicit definitions are true' [p.6].

The empirical question is how well the model fits the intended aspects of the real world, 'And here my central claim is that the fit is always partial and imperfect…. That does not, however, prevent models from providing us with deep and useful insights into the workings of the natural world' [p.6]. In the context of the current 'science wars,' Giere argues that one can have 'realism without truth…. Science does not deliver to us universal truths underlying all natural phenomena; but it does provide models of reality possessing varying degrees of scope and accuracy' [p.6].

The reason why models never fit perfectly is that models are abstract, the real world indefinitely complex: '…any law of nature refers to only a few physical quantities. Yet nature contains many quantities which often interact with one another, and there are few if any truly isolated systems…. Consequently, understood as general claims about the world, most purported laws of nature are in fact false' [p.24]. Giere is speaking about physical systems. His comments would seem to apply all the more strongly to social systems.

It is noteworthy that Giere blurs the distinction between theory and model. Sometimes he refers to a theory as a collection of theoretical models [e.g., classical mechanics in physics], but a theory and a model are not fundamentally different.

> 'In this picture of science, the primary representational relationship is between individual models and particular real systems, e.g., between a Newtonian model of a two-body gravitational system and the Earth-Moon system. But similar models may be developed for the Earth-Sun system...and so on. Here we have not a universal law, but the *restricted generalization* that various pairs of objects in the solar system may be represented by a Newtonian two-body gravitational model of a specified type. Restricted generalizations have not the form of a universal statement plus a proviso, but of a conjunction listing the systems, or kinds of systems, that may successfully be modeled using the theoretical resources in question...Newton's equations of motion and the formula for gravitational attraction' [1999, p.93].

He notes that some pairs of objects, for example, the Earth-Venus system, cannot be well-represented by the same sort of model, but this does not lead to a rejection of the model – only to the recognition that the model works well for some systems but not others.

The acceptance of a particular model is not a matter of pure reasoning or logical inference from facts, as in logical positivism, but a matter of 'making a decision': 'Effective decision making requires strategies for reaching desired goals…' [p.7], and thus the rationality is 'conditional and instrumental.'

In physics, 'theory' includes simple mechanical models [gravity, pendulums, etc.] as well as more general models such as relativity or quantum theory. But the earlier and simpler models are not in this view discarded. Newtonian mechanics is alive and well in university physics courses – not because it is of historical interest, but because the theoretical models therein have permanent validity as scientific models.

2. Demography Reconsidered

2.1 Demograhy in General

If Ohm's law, restated as *voltage = current x resistance*, is a theoretical model in physics, then the demographic principle *number of births = birth rate[s] x population at risk* can be seen as a theoretical model in demography.

If $d = ½\ gt^2$ is a theoretical model, then so is $P(t) = P(0)e^{rt}$. Both state basic principles that are valid though seldom observed in the real world. Granted the term g is approximately constant on earth [in fact, it varies by geographical location and altitude] whereas r is seldom constant for long, differing in time and place, g is not the same everywhere in the universe. The difference in the two expressions is one of degree not kind. Both are formal models.

The cohort-component projection model, to use a more complex example, cam be viewed as a theoretical model of population dynamics involving the variables size and growth, age-sex composition, fertility, mortality, and migration. It is a technique, an accounting device, and a computational algorithm. But it is more than that; it is a valid theoretical model of how populations change.

Interestingly, the term *theory* is seldom used with respect to the projection model, whereas it often is used with respect to the stable population model. It would be interesting to know whence this usage arose. Was it because stable theory was due to Lotka, a mathematical biologist? Or, in demography, was it due to the fact that the stable model had equations, whereas the projection model had only procedures?

In this view, which I think would accord with Giere's, a large part of 'formal demography' can be thought of and presented [this is especially important for students, policy makers, and the general public] as substantive theory --not behavioural, but still substantive. To characterise it as our 'accountancy core' [McNicoll] or as our 'statistical and technical home base' [Lesthaeghe] is to underrate its stature as knowledge.

2.2 Fertility Theory

What is the status of classical transition theory? I doubt that many demographers today would claim it as valid theory? But in the view sketched above, it is or at least could be. I say 'could' because unlike many older models in physics, demography has never developed a standard version of transition theory. We have not been able to agree precisely what it says. There is the well-know diagram showing fertility and mortality with respect to time, with mortality declining first. But this often is dismissed as a 'schematic diagram,' with the accompanying comment that transition theory doesn't fit all the facts.

But if transition theory is stated clearly, it fits a large number of facts rather well: in the face of 'development' [industrialisation, rising income, modernisation, urbanisation], mortality and fertility fall, first mortality and then, after a lag, fertility. A recent graph [see Figure 1] from INED shows that at the level of collections of nations, the qualitative fit is perfect. For both developed and developing nations collectively, mortality decline is followed after a time lag by fertility decline [INED, 1994, Fig.3]. As another example [see Figure 2], Bongaarts and Watkins [1996, Fig.2] show that no nation very high on their development index has high fertility, and no nation low on their index has low fertility. This is a very good fit indeed; some measures of association on this table would approach 1.0.

The value of transition theory as a theoretical model for the last 150 years of demographic history may be contrasted with the logistic model of the biologists, in which population growth equilibrates due to falling fertility and *rising* mortality. The latter model remains a valid theoretical model of population dynamics; it just doesn't fit well with recent human demographic history. In the long run, at a very different time scale, it still could.

In Giere's terms, what we need to do is to state transition theory more rigorously and to indicate clearly the empirical instances to which it applies – to develop what he calls a 'restsricted generalization.'

The list of other good theoretical models of fertility is a long one: Coale's fertility indexes and Bongaarts' proximate determinants [but seen as theoretical models, not just statistical decompositions]; the many versions of microeconomics of fertility; Rosero-Bixby and Casterline's formal model of diffusion and fertility decline; Coale's model of the three preconditions and its recent formalisation by Lesthaeghe; to mention just a few. But many of us, myself included, have viewed this plurality of models as a weakness, and pined for the unified general theory of fertility.

As in the case of transition theory, we have often failed to take these models sufficiently seriously, to collect and codify them, and polish them into clearly stated models stated in a standardised way. But that is precisely one way in which theory cumulates.

In short, demography has more and better theory than we let on.

2.3 Wilson and Oeppen on Reification

In a recent, provocative paper, Wilson and Oeppen [1999] have expressed concern over reification in demography. Reification [or the fallacy of misplaced concreteness] amounts to confusion between our mental abstractions and concrete reality. Given human weakness, including the individual and collective vanity of scientists, it is unavoidable – there is a little bit of Pygmalion in all of us -- although this does not mean that we should not try to avoid it.

I believe one way to help is to emphasise, as do Meehan and Giere, that **all** human knowledge is abstract, that none of it represents The Truth. Human knowledge is just a tool which we use to adapt to the world around us in order to predict, explain, and control [see Meehan]. This will be easier to see and teach if we think not so much in terms of one theory, but in terms of many theories and theoretical models, each one with its strengths and weaknesses. It is the claim of near absolute truth for one very powerful theory that causes real trouble, in science and in the larger community.

2.4 Lesthaeghe on Lakatos

The recent paper by Lesthaeghe [1998] on theory development according to Lakatos is one of only a few papers by demographers on issues of philosophy of science and scientific methodology – as opposed to techniques [but see also Wunsch, 1995 and earlier]. It is of interest to compare briefly Lesthaeghe's ideas, with those sketched above, especially those of Giere.

It appears that Lakatos adheres to the underlying logical positivist approach, including falsificationism, whereas Giere drops both. He would disagree with Lesthaeghe's statement that 'Newtonian mechanics as elaborated in the theory of gravity constituted one of the best-corroborated theories of all times, yet it was swept away by relativity and quantum physics' [p.3]. Giere would counter that it was not swept away at all, but continues to be taught in introductory physics, as containing simple models of limited scope, but valid theoretical models. An old but classic physics text [Gamow and Cleveland, 1960] uses the appealing sub-title *Foundations and Frontiers*, and comments '…the foundations laid down by Galileo and Faraday and others cannot be slighted. The laws of Newton are still vital in every satellite and rocket' [p.*v*]. Sometimes the simpler models have the advantage over more modern theories. Another physics text comments 'Our ability to describe and predict the behaviour of systems that may be too complicated to analyze in terms of atomic and molecular processes is one of the beauties of thermodynamics' [Hewitt, 1993, p.306]. Giere is less interested in trying to falsify theories than in testing to which real world systems they can apply.

The basic thrust of Lakatos is to develop ever more inclusive theories, that is, theories that can account for more empirical observations. Lesthaeghe speaks of the 'partial inclusion of one theory into another and the derivation of more comprensive theories with enlarged content' [p.5] . Giere has no objection to this so long as it is feasible and leads to useful theories, but he is more willing to accept usable partial models [e.g., an Earth-Moon model as opposed to one that applies to all body-body relationships, and is less tractable].

Lakatos would appear to be fundamentally opposed to Giere's view of theory acceptance as a decision insofar as his explicit criticism of Kuhn would seem to apply even more strongly to Giere:

> For Kuhn scientific change – from one 'paradigm' to another – is a mystical conversion which is not and cannot be governed by the rules of reason and which falls totally within the realm of the *(social) psychology of discovery*. Scientific change is a kind of religious change' [quoted by Lesthaeghe, p.3, emphasis in original].

Giere emphasises decision and choice as the basis for acceptance of a theory precisely because this leads to use of social psychology in studying science. But he would not accept that the process is thereby irrational – but is not just a matter of logic. It is a matter, not of discovering truths through logical inference from facts, but of deciding that some models fit better than others. This process is at best instrumentally logical, conditioned on finding an appropriate means to some end.

The emphasis on a plurality of formal theoretical models has something in common with Lesthaeghe's 'show room approach,' dealing with van de Kaa's 'anchored narratives.' But the latter are more concrete 'stories' arising from empirical data than they are abstract models arising from the scientist's imagination, disciplined by data. There is plurality of models, but their form is different.

There is also some similarity with Lestaheghe's 'jigsaw puzzle approach,' in which 'Rather than seeing paradigms as hostile species or as living in separate niches, they are viewed as potential complements' [p.2]. Giere would appear to value the pieces of the puzzle as well as the assembled puzzle. Imitating Lesthaeghe's colourful rubrics, we might refer to the pluralist approach as 'the one hundred flowers' approach, with due credit to Chairman Mao Zedong.

There is opposition between the two approaches to theory, but that does not mean that one has to be rejected, a point to which I shall return in closing.

3. The Future of Fertility Theory

3.1 Heterogeneity in Demographic Dynamics

The law of falling bodies and the exponential growth curve for population are formally the same. The difference lies in the fact that the gravitational constant g is in fact nearly constant, at least on earth, whereas the proportionate growth rate r is seldom constant, across time and place – for large human populations, it ranges from below zero to above 4 percent per annum, and tends to change from decade to decade, if not year to year. I take this as a metaphor for human science -- there simply is more variability due to history and culture.

In the case of fertility decline, for example, Hirschman [1994] has emphasised 'the diversity of historical paths from high to low fertility' [p.203]. An interesting specification of this diversity can be made using Lesthaeghe and Vanderhoeft's [1997] formalisation of Coale's three preconditions. They assume, following Coale, that the movement from being neither 'ready,' nor 'willing,' nor 'able,' to being all three can occur in any temporal order. This implies at least six distinct pathways from traditional high to modern low fertility. In addition, Coale explicitly states that modern development is not a precondition for the preconditions, so that there are many

different kinds of demographic transitions – not just in regard to quantitative features such as timing and pace, but in regard to qualitative features.

How can we have a unified or unitary theory of such diverse processes. Two ways are possible.

3.2 Elegant Theories

We can strive for very general elegant theories. In fact we already have one, namely, Coale's statement of the three preconditions for marital fertility decline. Note, however, that its generality – its ability to apply to virtually all fertility transitions – is as result of its being nearly tautological. One further step in that direction would assert that 'A population will undergo a transition to low fertility when it is ready to undergo a transition to low fertility.' But Coale's statements are not quite tautological. Indeed they are true and may even constitute a useful theoretical model. Clearly, the preconditions tell us virtually nothing about quantitative features of fertility decline. Generality is achieved through inattention to concrete detail.

As an aside, we need to examine the meaning of *tautological* in regard to our theories. The British physicist Arthur Eddington criticised Newton's first law of motion on the grounds that we can tell whether a force is acting on a material body only by the fact that the motion of the body deviates from uniform motion or from a state of rest. He suggests the reformulation: 'Every body continues in a state of rest or uniform motion in a straight line except insofar as it doesn't' [Gamow and Cleveland, 1960, p.35]. Gamow and Cleveland comment 'Newton's first law should not be considered so much a law of nature as the definition of a force,' a view that parallels that of Giere quoted above.

In this category of elegant theories, I would include the several versions of the microeconomic theory of fertility, and 'uncertainty theory' [Friedman, Hechter, and Kanmazawa, 1994] – a recent effort that foundered in the face of 'rejectionism.' The difference between the economic approach to fertility theory and that of Meehan and Giere, as I understand them, is that the former emphasises the construction of theory on the basis of a few standard 'axioms,' whereas the latter are more relaxed about the where the content of theoretical models comes from.

3.3 Inelegant Theories

Another way of achieving more general theories is greatly to enlarge their content. This is the route favoured by Lesthaeghe, and most recent writing on fertility theory [see Burch, 1999; Mason, 1997]. The process of enlargement has been encouraged recently by greater attention to 'culture' and to diffusion processes and to social control or influence [Bongaarts and Watkins, 1996; Montgomery and Casterline, 1996; Kohler, 1997].

A theory of this sort tends to be complex, with many variables, many parameters, and many functional forms. It achieves generality in the sense that a template is general – plug in enough of the right numbers and the model can be made to fit almost any concrete situation. But this probably is the kind of theoretical model one needs in order to account for or predict features of fertility change such as timing, pace, initial and 'final' levels, etc. And the abstract model moves towards greater concreteness, becoming more similar to one of van de Kaa's anchored narratives.

It can be noted in passing that if such a theory is to be anything more than the personal possession of one or a few authors, it will have to be stated in much more rigorous form than has been the case until now [see Burch, 1996]. For Meehan and for Giere, theoretical models have to be completely clear and unambiguous. This is barely discussed since it is taken for granted. But many demographic theories are neither.

3.4 A Two-by-Two Table

The concepts of the size of content of a theory and of the generality or comprehensiveness of a theory suggests an simple table:

	Larger content	Smaller content
More general	Lesthaeghe, Mason	Coale's preconditions
Less general	Anchored narratives	Caldwell's wealth flows

Note that the dominant methodological approach in demography would urge us to focus on more general models, the top row. Giere would place more value on the bottom row, while not disparaging the top.

This table also suggests why the prospects for a generally accepted unitary or unified theory of fertility are not so good: different demographers will want it to fall in different quadrants of the table. It is hard to imagine a theory that would satisfy Becker or Friedman, Hechter, and Kanmazawa, with their emphasis on the fewest possible premises, and at the same Lesthaeghe or Mason.

It can also be mentioned that emphasis on the bottom row is in keeping with the current substantive shift in fertility research away from a preoccupation with fertility transitions to at least equal interest in extreme low fertility. Many of our existing theories have been shaped by the earlier preoccupation, and may serve as very blunt tools for understanding total fertility rates of only slightly above 1.0. But the same token, good theoretical models of extreme low fertility may be of little relevance to historical fertility declines. The point is that we need different theoretical tools for different tasks.

4. Concluding Comments

Giere counts himself as a 'realist' in the philosophy of science – he tries to develop his ideas from empirical reality, not solely on the basis of logic. By this view, his ideas about science are in fact theories, and are to be judged in terms of their usefulness in understanding or doing science. Although he does not favour some of the older ideas of logical positivism and Popper's emphasis on falsification, it is hard to see how he could reject them outright as an alternate abstract model of science.

Perhaps logical empiricism does apply to some sciences or some scientists, telling us how they worked and how they thought they ought to work. It must apply to much of social science, because so many of us accepted for so long the gospel according to Nagel and Hempel. It is even possible that some parts of social science can in fact be built on universal empirical generalisations, with competing theories tested in terms of their ability to explain all known phenomena, that is, to explain the empirical regularities.

By the same token, I may have constructed a straw man. Perhaps a careful reading of a representative sample of demographic literature would show more theoretical inference along the lines suggested by Giere than I have suggested. My impression is that demographers have been more willing to accept and use simple formal models than simple behavioural models. Apart from impressions, we need more critical-historical work to answer that question.

Part of the difficulty lies in the fact that there is often a subtle difference between saying 'This theory doesn't fit this empirical case or this collection of empirical cases, and is therefore not valid for them,' and 'This theory doesn't fit this empirical case or collection of cases and is therefore false and invalid.' A related distinction is between a theory considered as an abstract model, and a theory-based explanation of a concrete historical case. Theories are abstract; concrete cases are concrete. A theory cannot represent the full historical detail of a concrete case – as in a 'thick description -- or else it ceases to be a theory.

These differences are important even if they seem small. The alternate view does not militate against the search for theoretical models of wide applicability. And the preferred model, other things equal, will be the one that applies to the largest range of empirical examples. In this Giere and Lakatos are not too far apart.

The key difference lies in the attitude toward and treatment of the lesser models, those that apply to a smaller set of empirical instances, but nonetheless have other virtues. Does the discipline cherish and care for them, or reject them? It seems to me that the former course would lead to a much richer body of theory and a stronger science of demography. It also would lead to a more effective presentation of the fund of demographic knowledge to students and to the general public.

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