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# Computer Modelling of Theory: Explanation for the 21st Century

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# Computer Modelling of Theory: Explanation for the 21$^{st}$ Century[1]

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> It is only now that we have the ability to do complex calculations and simulations that we are discovering that a great many systems seem to have an inherent complexity that cannot be simplified…. Glenn W. Rowe in *Theoretical Models in Biology.*

## Introduction

The words *theory, model,* and *explanation* are used in different ways by different writers. Agreement on their meanings among natural scientists, social scientists, philosophers of science, engineers and others seems unlikely. But meaning depends on context. Appropriate meanings for these words depend on subject matter, and on the purposes of research. A theory, model, or explanation – or a good theory, model, or explanation -- for a physicist or chemist may differ in important respects from a theory, model, or explanation for a biologist, a meteorologist, or a demographer. These differences will be all the greater if one considers models and theories used in practical decision making, as in engineering or policy analysis.

The question of which view of theory, models, and explanation is the 'correct' view seems less relevant than the question of which view promises to be more fruitful for mainstream social science. In this paper I argue the fruitfulness of an approach to theory building, modelling, and explanation with the following features:

1] The first step in explanation is seen as a purely logical exercise, assuming, of course, some empirically described phenomenon to be explained. Something is explained when it can be logically deduced from a set of theoretical propositions, or a model embodying them, supplemented by appropriate factual premises. Whether a logically valid explanation is empirically valid is another matter, a second step. At

[1] . This is a revised version of a paper prepared for the Centre Methodos colloquium and public lecture series on 'The Explanatory Power of Models in the Social Sciences,' 14-17 November 1998, Louvain-la-Neuve, Belgium. Ongoing support for research on modelling theories of fertility has been provided by the Social Sciences and Humanities Research Council of Canada. I am also greatly indebted to Antonella Pinnelli, Department of Demography, University of Rome [La Sapienza], and to James W. Vaupel, Director, the Max Planck Institute for Demographic Research, Rostock, Germany, who enabled me to make extended visits to their respective institutes, and to think about these issues under optimal conditions.

the first step, explanations inherit the hypothetical and provisional character of the theories or models on which they are based.

2] The propositions in a theory or model used in explanation are to be judged more on their clarity, logical coherence and explanatory power, less on their origins. In particular, propositions need not be, or be derived from, primitive axioms [as in economics]. Nor must they be, or be derived from, empirical generalisations or verified empirical laws [as in the 'covering-law' view of explanation]. Theoretical propositions can express hunches, prejudices, or guesses, so long as they are clearly formulated and coherent. Models can contain unobserved or even unobservable variables. This underlines the hypothetical and provisional character of theory, which is generally recognised, but also of models [including computer models and simulations], which is not.

3] In social scientific explanation, theories and models may and often must be more rather than less complex. There will be less emphasis on elegance and parsimony [theoretical aims inherited from medieval philosophy and from physics], in response to the complexity of social and historical phenomena.

4] Given a] more complex theories and models and b] the need for logical rigor in their manipulation [see point #1 above], natural language and logic and analytic mathematics will be supplemented by computer languages in theoretical work. Apart from preliminary or simplified statements or sketches, theories and models will be stated in the form of computer code, or initially pseudo-code, using systems dynamics software, or a general programming language like C/C++, or some other suitable computer software.[2] Entailments, predictions, implications of the theories and models will be calculated by the computer, rather than inferred or eyeballed by the investigator.

In the further development of these ideas, I shall use the following working definitions, slightly modified from Boland [1989]:

*theory*: a coherent set of propositions that can explain a phenomenon or class of phenomena [the *explicanda*], in the sense that the phenomenon follows logically from, is entailed by, the theory;

*model*: a more specific representation of the underlying ideas of a theory, but still general in scope;

*explanation*: the logical deduction of some concrete phenomena or class of phenomena [*explicanda*] from a model plus relevant factual premises.

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[2] . Computer systems for formal logic, with which I am not well-acquainted, may be of service, although they have limited ability to deal with quantitative as well as qualitative reasoning. For an interesting sociological example, see Péli, Bruggeman, Masuch, and Nualláin [1994].

Boland distinguishes 'pure or abstract models, which he describes as representations of the underlying logic of the theory being modelled,' from 'applied models – which are explicit, simplified representations of more general theories and are designed to apply to specific real-world problems or situations' [p.2]. My use of the word *model* is closer to his concept of *pure or abstract model*, and my use of the word *explanation* closer to his concept of *applied model*. In any case, explanation of a concrete event requires a model specific to a real-world situation.

A theory is often general in the dictionary sense of 'involving only the main features rather than the precise details.' A model moves toward specification of details. Market theory says that a rise in price tends to reduce demand and to increase supply. A model of the theory would define the relationships more precisely by characterising the appropriate curves [e.g., convex upward] or by giving specific mathematical functions.

An explanation must be further specified to apply to particular real-world situation. Relevant historical or cultural context must be added in the form of concrete facts. Parameters in mathematical functions or algorithms, already specified in the model, must be given particular values.

But note that explicitness, concreteness, specificity, precision, and detail all are defined in terms of continua rather than sharp dichotomies. The dividing lines between a theory and a model, or between a model and an explanation are not always sharp.

In this paper the word *model* is used in a different sense than in statistics or traditional mathematical modelling, in which the emphasis is on fitting a particular statistical or mathematical equation or set of equations to a specific set of empirical data. The emphasis here is on an initial process of fleshing out theoretical ideas, representing them in a more precise and more explicit form. Modelmaker, a systems modelling software package, makes the relevant distinction, between *empirical models* and *simulation* or *mechanistic models* [Walker, 1997, pp.7-10]. Empirical models describe variation in 'some observed data for a phenomenon which shows how it varies in relation to other factors' [p.7] . 'Simulation models…try to describe a number of sub-processes which can be combined to represent the behavior of a larger more complex system' [p.9]. The latter might also be termed *theoretical modelling*. More simply, it is a question of modelling a theory versus modelling a specific data set.[3]

The description of empirical models by Edwards and Hamson [1989] is particularly instructive and worth quoting in full:

> An empirical model is one which is derived from and based entirely on data. In such a model, relationships between variables are derived by looking at the

[3] . In demography, the notion that simulation is a way of working out the implications of theoretical ideas has been emphasised by Wachter and Hammel. See, for example, Wachter, 1987, Hammel, 1990.

> available data on the variables and selecting a mathematical form which is a compromise between accuracy of fit and simplicity of mathematics…. The important distinction is that empirical models are *not* derived from assumptions concerning the relationships between variables, and they are *not* based on physical laws or principles. Quite often, empirical models are used as 'submodels,' or parts of a more complicated model. When we have no principles to guide us and no obvious assumptions suggest themselves, we may (with justification) turn to the data to find how some of our variables are related [p.102].

Confusing empirical with 'non-empirical' models in social science has been a source of considerable misunderstanding. Both proponents and critics of computer simulation have often tended to forget the hypothetical character of simulations, the former assuming without adequate tests that their models are empirically valid, the latter judging them by the inappropriate standards for empirical statistical models.

Clearly, theories, models and explanations crafted using the above approach remain hypothetical structures, and must be empirically tested in some way. The 'validation' of computer models or simulations is a thorny issue, to which I turn after a brief comment on each of the four points sketched above.

None of the key ideas in this paper is novel, although the approach they suggest in combination is not commonly used in day-to-day work in the social sciences, where 'theoretical work' is largely qualitative, discursive, and verbal, and 'empirical work' consists largely of statistical modelling, often only loosely linked to theory.

The equation of explanation with strict logical inference from *explanans* to *explicandum* is common enough in methodological and philosophical writings of those who adhere to an empirical science model of social science, as opposed to those who favour 'understanding' or 'intuition.' But it is encountered more in statements of methodological principle than in everyday work.

The notion that the origin of theoretical ideas or hypotheses is not so important so long as they are eventually tested is classic Popper. A particularly forceful development of this view [as well as the equation of explanation with formal inference] is found in an early work of Meehan [1968] – *Explanation in Social Science: A System Paradigm,* a stimulating book I have only recently encountered[4] -- arguing against the 'covering law' model of explanation. I refer to Meehan often because his ideas seem to me to clarify so many of the issues posed in the present volume.

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[4]. I do not recall ever having come across reference to Meehan in works in sociology or social or economic demography. Nor does a perusal of indexes in several economics and sociology texts on methodology encounter his name. It would be unusual, of course, for someone in these disciplines to turn to a political scientist for methodological guidance. Like most distinctive human groups, social science disciplines are class-conscious and not a little bit snobbish.

The move towards complexity in models and theories is well under way in many scientific fields. With the 'discovery' of chaos, it has become fashionable, but that does not mean it is mistaken. The quote that opens this paper is representative of a changing outlook on simplicity as a scientific virtue [see also Waldrop, 1992].

The idea of computer simulation or modelling of theoretical ideas is now commonplace in the physical and biological sciences, and would need no special attention were it not for the fact that it is still looked on with some suspicion in many social science circles.

The 'systems dynamics' approach to modelling has a special history in social science, and has been roundly and justifiably criticised on more than one occasion. My suggestion that we make more use of the software emerging from this tradition does not imply that we must accept the underlying philosophy of 'general systems theory,' or should imitate some of the early misuse of the software. I view the software simply as a practical tool for the more rigorous statement and manipulation of theoretical ideas, a tool that goes beyond analytic mathematics in power and flexibility , and in any case a tool that is accessible to the average social scientist, who does not have now, and is unlikely to have in the future, the fund of mathematical expertise of the average physicist or engineer, or the programming sophistication of the average computer scientist. My thinking on these matters has been heavily influenced by Hanneman's *Computer –Assisted Theory Building: Modeling Dynamic Social Systems*, a work that deserves more attention than it has received.

## Explanation as Logical Inference

The influence of multivariate statistical methods has been so powerful in empirical social science that for many the word *explanation* tends to be equated with the phrase *accounting for variance*: X helps 'explain' Y if it has a large and significant regression coefficient; a 'good explanation' has a large $R^2$. This is a valid use of the word, but in many respects an impoverished one [see Abbott, 1988; Lieberson, 1985]. Such an approach limits explanation to variables that are not only measurable but have actually been measured, if only indirectly [e.g., latent variables models]. It tends to discourage, or at least often does not require, deep thought about process or mechanisms. It easily leads to atheoretical analysis. Or, at best theory is pared down to fit statistical models, in what might be called Procrustean empirical modelling.

The idea of explanation as inference of the *explicandum* from a set of premises is common enough among some social scientists of a theoretical bent – mathematical economists, exchange theorists [see Homans, 1967], and all who have subscribed to the 'covering law model of explanation,' à la Nagel and Hempel.

A particularly striking development of the idea is to be found in the works of the political scientist Eugene J. Meehan, notably in his 1968 book *Explanation in Social Science: A System Paradigm* [see also 1981]. Meehan dismisses the 'covering law' approach to explanation [he cites Braithwaite, Hempel, and Nagel] and offers as an alternative what he terms the 'system paradigm of explanation.' Proposed at a time when computers and computer modelling [simulation] were still in their infancy, his approach provides a convincing methodological foundation for computer modelling as a powerful tool for the statement and manipulation of behavioural theory

Meehan characterises the 'covering law' or 'deductive' paradigm of explanation as follows:

> An event is explained when it can be related to an established 'empirical generalization' or 'general law' according to the canons of formal logic; generalizations in turn are explained when they can be deduced from sets of 'higher' generalisations or theories. The structure is held together by the rules of formal logical inference. The elements of the structure, the empirical generalizations or laws, must be available *before* explanation is possible. If the relation is to be deductive, generalizations must take the form 'all A is B,' or in some few cases 'n percent of A is B.' Other forms of generalization are not amenable to deductive inference. The generalizations, in other words, are established independently of the explanation; they are subject to 'empirical verification' or test [1968, p.9].

Meehan's characterisation of the covering-law approach to explanation agrees with that of Miller [1987] who comments: 'Covering laws in the deductive nomological pattern must be, not just general, but empirical, subject to disconfirmation by observational data' [p.19].

Meehan's criticism of the deductive approach is twofold: a] '…the paradigm collapses or merges the logical and the empirical aspects of explanation…' [the classic problem of induction]; and b] '…the definition attaches no weight to the purposes for which explanations are sought or to the manner in which they are used'[1968, p.10].

In practice, Meehan finds adherence to the deductive paradigm of explanation severely restricting for social science, since there are so few 'empirical laws' or 'nomic empirical generalizations' of the sort the paradigm requires, which leads to a pessimistic view of the explanatory capacities of the social sciences. Meehan sees the situation not so much as a reflection of 'the weakness of social science' as of 'the limited usefulness of the deductive paradigm' [1968, p.3].[5]

Simply stated, Meehan's notion of explanation of an observation or event involves: a] creation of a logical structure of variables and their relationships, a

[5] . Meehan claims that the 'deductive paradigm' of explanation is in fact not actually used in the physical sciences, but has largely been made up by logicians. See 1968, p.3-4.

structure which logically implies or entails the event; b] demonstration that there is correspondence or 'isomorphism' between the logical structure and the real-world context in which the event is embedded.

In its emphasis on a 'formal logical structure,' Meehan's approach resembles traditional mathematical modelling, the axiomatic structure of modern economics, and the covering law model of explanation.[6] The difference lies in the origin and character of the propositions in the formal structure, to be discussed below.

The following summary statement by Meehan captures the spirit and essence of his approach:

> The instrument that makes explanation possible is here called a *system.* It is defined as a formal logical structure, an abstract calculus that is totally unrelated to anything in the empirical world. The system, as a system, says nothing whatever about empirical events; it generates expectations within its own boundaries [p.48].

Expectations are generated through strict logical inference:

> Since the instrument used for explanation of empirical events must contain timeless or general propositions, and since it must generate expectations that can be warranted or justified, there is really no choice of instruments involved. Of all the structures that [one] can create, only a formal calculus can create warranted expectations. Given the axioms of a formal logical structure, certain conclusions are inescapable; if the axioms are accepted, the conclusions can be denied only by self-contradiction…. Barring errors in calculation, the entailments of a logical system are necessarily and indefeasibly true [p.48].

Explanation is a form of applied mathematics or calculation, using a closed formal system [1968, pp.62,125].

Meehan's 'system' might be called a theory or a model as I have defined the terms above, depending on the degree of specificity.[7] In either case, it remains abstract. The system must be further specified [Meehan speaks of the formal calculus as being 'loaded'] in order to apply to and explain a concrete event or class of events.

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[6] . Given the central place of deductive reasoning in his system paradigm of explanation, it is a bit awkward that his second name for the covering law approach to explanation, which he rejects, is the 'deductive paradigm.'

[7] . Meehan systematically avoids use of the word *theory* 'because of the ambiguity of common usage.' But he notes that in his approach, systems perform the same explanatory functions as theories, and comments that 'well-established systems should probably be called "theories" if the concept is to be used at all.

The notion of a computer template provides a contemporary analogy. A spreadsheet program for making a cohort-component population projection is an abstract algorithm. It must be 'loaded' with data for a particular county before it can be applied to predict or to explain past demographic dynamics. But first and foremost it must be a logically and mathematically correct template. A similar idea is found in Miller's [1987] distinction between theories and 'event explanations': '…a theory is a description of a repertoire of causal mechanisms, a theoretical explanation, an explanation appealing to instances of such a repertoire' [p.139].

A theoretical explanation that does not logically entail its explicandum, or a theoretical 'prediction' that is not logically implied by its theory, are non-starters. If an explanation is not logical, it is not an explanation. Many, perhaps most, social scientists would agree with this view in principle. But as Platt pointed out many years ago [1964] in his classic paper on 'strong inference,' it often is neglected in everyday scientific work as researchers 'feel their way to conclusions' or investigate hypotheses 'loosely based on' or 'suggested by' theory. And, as noted above, explanation often is equated with explaining variance.

## The <u>Origins</u> of Theoretical Ideas Are Irrelevant

One of the best definitions of *theory* that I have encountered was in a small English dictionary in the library of The Netherlands Institute for Advanced Study in Wassenaar:

> Conceptions, propositions or formula [as relating to the nature, action, cause origin of a phenomenon or group of phenomena] formed by speculation **or** deduction **or** by abstraction and generalisation from facts [emphasis added, exact reference unknown].

The definition properly does not limit theoretical ideas to one source or origin. In particular, it does not limit them to valid empirical generalisations, as in the covering law approach to explanation. Theoretical propositions arrived at 'by abstraction and generalisation from facts' are included, but others sources of theoretical propositions are not excluded.

In fact, it doesn't matter where ones ideas come from in science, so long as they are reasonably clear and coherent, relevant to the matter at hand, have explanatory power, and are subject to empirical test.

This is a central theme in the work of Popper, who emphasises the imaginative and creative character of theorising and hypothesis formation, counterbalanced by a strict program of attempts at falsification. 'Bold ideas, unjustified anticipations, and speculative thought, are our only means for interpreting nature…. And we must hazard them to win our prize' [1959, p.280].

This is also a central theme in the work of Meehan, as described earlier. If explanation in social science must rely on empirically valid generalisations ['covering laws'], its scope will be severely restricted at the outset and the explanatory enterprise will barely leave the ground. In his system paradigm of explanation, 'timeless or general propositions are assumed to belong to the logical rather than the empirical world' [1968,p.32].

If an explanatory system does not require valid empirical generalisations as premises [along with relevant factual premises], it of course cannot contain empirical statements which are clearly false. But one should be careful not to throw out the baby with the bath water. In demography, for example, the documentation of several instances [both nations and provinces] in which secular decline in aggregate fertility was **not** preceded by substantial mortality decline [Coale, 1973] invalidates classic 'transition theory' or any other explanatory theory that assumes such a time sequence is universal, and that mortality decline is a necessary condition for fertility decline. But the generalisation remains valid for the vast majority of historical cases in Europe and for virtually all recent non-European cases. And it should find a place in theories or explanatory models pertaining to the cases to which it applies, or with the insertion of *ceteris paribus* clauses or scope conditions. It is too powerful a finding to discard because of counterexamples.

This probably is the motivation behind past efforts to introduce probabilistic generalisations into covering law explanations. This is a difficult technical issue. But suffice it to say that probabilistic statements do not support the kind of strict logical inference that Meehan favours. If A and B are positively but less than perfectly correlated and B and C are positively but less than perfectly correlated, it does not necessarily follow that A and C are positively correlated. Or if A causes B with moderately high probability, and B causes C with moderately high probability, the occurrence of A will lead to the occurrence of C with much lower probability – that is, there is a pretty good chance that C won't happen at all in a given case.[8]

It is not clear how to reconcile the notion of explanation as strict logical inference with the introduction of non-universal, probabilistic premises, since explaining that something had a high probability of happening is not quite the same as explaining that it happened. One approach might be to keep theoretical statements deterministic and therefore subject to strict logical inference, but to introduce stochastic elements into models and explanations. Meehan finesses the problem by urging resort to *ceteris paribus* assumptions. Perhaps something deeper is at work, namely a backing off from the explanatory and predictive standards of 'celestial mechanics,' which, when all is said and done, are not quite suited to biological and human systems [Ekeland, 1988].

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[8] . For an interesting probabilistic formalisation of Coale's ideas on the 'necessary preconditions' of marital fertility decline, see Lesthaeghe and Vanderhoeft, 1997.

**Towards More Complexity**

The influence of classical physics on our notions of good science is nowhere more evident than in the commonplace view that theory should strive for elegance and simplicity. The physicist Steven Weinberg has written [1980]: 'Our job in physics is to see things simply, to understand a great many complicated phenomena in a unified way, in terms of a few simple principles.' A hundred years earlier, J. Willard Gibbs had written: 'One of the principal objects of theoretical research in any department of knowledge is to find the point of view from which the subject appears in its greatest simplicity' [quoted in Tanford, 1978]. The idea has philosophical and theological origins with William of Ockham – after all, God is the one explanation for everything. It pervades physics from Newton right up to the present.

In social science, the self-conscious quest for elegant models is most pronounced in mainstream economics, based as it is on three basic axioms. The classic methodological reference is to Milton Friedman [1953], who not only favours explanation with as few axioms as possible, but with human behavioural axioms that are counter-intuitive. That the quest for parsimonious explanation continues to influence thought is to be seen in a recent paper on fertility theory [D. Friedman, Hechter, and Kanmazawa, 1994], in which they argue that their theory of fertility is better than the standard microeconomic theory because it is based on only two axioms rather than three.

In sociology, Jasso [1988] holds to the reasonable view that other things equal, a theory that can explain many things with relatively few assumptions is better than one that requires more assumptions to explain the same things.

There is a certain common sense to this way of thinking -- why use a shotgun to kill a fly? But a reasonable notion of efficiency in explanation may become an obsession, with as much emphasis on simplicity as on explanation. Moreover, what will work in one field of study may not work in another. Only time will tell, but it may well be that biological and human systems are indeed more complicated than those studied by physicists and chemists. It already is clear that many natural systems are more complicated than those designed by engineers.

But if the reality is more complex then perhaps our theories and explanations also must be more complex. This is the force of the quote from Rowe at the beginning of this paper. It is the theme of Wunsch's paper [1995] 'God gave the easy problems to the physicists.'

There seems little doubt that many fields of science will in future work with theories and models of greater complexity, and indeed this already is happening in many quarters. The ultimate ideal of theoretical elegance no doubt will remain, based as it is on human aesthetics and common sense notions of efficiency. And deliberately oversimplified models will be studied as sources of insight. But useful theories and models – for explanation, prediction, and practical application – will be more complicated than we are accustomed to.

The greater complexity will arise on several fronts. There will be more variables in models, including theoretically important variables for which empirical data are sparse or non-existent. The functional relationships will often be non-linear. The models will be inherently dynamic, with feedback processes.

## Manipulating Complex Systems

This greater complexity will strain the analyst's ability to derive logical implications of model assumptions using ordinary language and logic. Similarly, model manipulation will often exceed the capacity of analytic mathematics. The obvious tool for the manipulation of such complex models is numerical simulation by computer.

In an introduction to an issue of *Science* on computers and fluid dynamics, the authors comment:

> Efforts to understand the formation of stars, the motions of ocean and atmosphere that control our weather, and other fundamental processes on Earth and in space face a double challenge. Data on these phenomena are often sparse, and they are governed by complex fluid motions. Thus they are tailor-made for study with a computer[Hanson and Appenzeller, 1995, p.1353].

I have argued elsewhere [Burch 1997a] that demography – and by implication other fields of social science -- faces a similar challenge, and need to pay far more attention to computer modelling, in both research and training, than has been the case up to now. Computer modelling is the only way to work with complex models while preserving logical rigour. The alternatives are rigorous simple reasoning or less-than-rigorous complex reasoning.

Meehan is cautious as to how complex effective explanatory systems can be. His acquaintance with early examples of computer simulation apparently only underlined this caution.

> …logical limits preclude the development of large and cumbersome theories that contain a great many variables. Most of the complex structures that have been produced in the computer era are actually very weak and unreliable, and their internal operations simplistic. Good theories are likely to contain only two or three variables, with an elaborate set of limiting conditions. If the system is characterised by interactions among variables [feedback], those limits must be maintained. Calculi containing four variables with feedback

> are for all practical purposes impossible to calculate. If that number is increased to five, calculation becomes impossible in principle. This argues strongly against the development of theories containing dozens of interacting variables [1981, p.123].[9]

Such a view of early attempts at computer simulation are understandable given the over-ambitious character of some early models, notably, macroeconomic models of whole economies and 'world models,' as in *The Limits to Growth.* With greater experience, we now understand that there are limits to the amount of substantive complexity that can be effectively dealt with even with the most modern computer hardware and software. There are limits to the intelligibility of overly complex models, limits to control over internal processes [we don't really know if the model is behaving the way we intended it too], and even limits to error-free programming.

But all that said, it is true that computer modelling does allow for the rigorous manipulation of systems considerably more complex than those that can manipulated by means of traditional logic and analytic mathematics. These more complex systems can be dynamic [rather than static or based on equilibrium assumptions]; they can contain non-linear relationships; and they can contain feedbacks. Practical limits on the number variables no doubt exist, but they are much higher [tens, dozens?] than the two or three variables that Meehan speaks of. It seems that modern computer modelling greatly extends the potential power of Meehan's 'system paradigm of explanation' – essentially by greatly expanding our ability to deduce, in strict logic, our explicanda from formal explanatory systems.

## Confirmation and Testing: The Greater Challenge

Meehan's explanatory system is a formal system in the same sense as plane geometry. It consists of well-defined variables and well-defined relationships between or among them [propositions], such that the system can be rigorously manipulated to infer implications or entailments. The variables do not need to relate to observables. The propositions do not need to have been empirically verified. They can themselves be formal; they can result from hunch or intuition; they can, but need not be, inspired by propositions generally taken as empirical generalisations in a particular field. The first requirement for an explanation is that the thing to be explained follows logically from the formal system.

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[9]. Meehan quotes W. Ross Ashby's *Introduction to Cybernetics* [1963] to support this assertion. Ashby seems to be speaking of systems in which every variable directly affects every other: 'When there are only two parts joined so that each affects the other, the properties of the feedback give important and useful information about the properties of the whole. But when the parts rise to even as few as four, if every one affects the other three, then twenty circuits can be traced through them…'[ p.54]. It is uncharacteristic of contemporary systems modelling to posit direct causal links from each variable in the system to every other.

The second requirement is that the formal system, when 'loaded' or further specified to relate to a given empirical situation, is adequately 'isomorphic' with respect to that situation. It is not enough that the model's predicted outcomes match the explicandum; in some sense the whole model must match the whole empirical situation.

> When a system is applied to an empirical situation it is not enough to show that one particular entailment of the system can be found in the empirical situation…. The aim is to match the total explanatory system with an empirical situation so that all of the entailments of the system have empirical counterparts in observation. The goal in explanation is a perfect match or fit between a complete system and a description rather than a logical fit between a single event and a general proposition, as in the deductive paradigm' [1968, pp.50-51].

Meehan is firm in his rejection of 'black box' explanations. An explanation must contain an account of the causal mechanism producing the outcome. In particular, it is not enough to show than an explanatory system can predict outcomes – Meehan makes a sharp distinction between explanations and forecasts. An explanation must relate to causes, and it must fit empirical reality in a broad sense:

> The nature of the isomorphism required is immutable and unambiguous. *The whole structure must fit the observation.* It does not suffice to show that some of the implications of the calculus appear in the observation. That result can be produced using false assumptions. But assumptions that are known to be false and to be imperfectly isomorphic to observation cannot be incorporated into theories. They are only useful for producing forecasts or predictions. The point is vital, particularly for criticism of much of the work in economics and econometrics [1981, pp.89-90, emphasis in original].

Unlike a prediction, which enables us to anticipate an outcome and adjust to it, explanation, given its clear causal structure, also provides a basis for **control** of the outcome, at least in principle.

But there is no easy way to demonstrate isomorphism. This problem of how to test complex simulation models empirically ['validating,' 'confirming'] has plagued the practice of computer modelling from the beginning and has yet to be adequately resolved. It is one of the chief reasons why mainstream empirical social science has tended to hold simulation at arm's length.

Insofar as they are supposed to refer to the real world, and insofar as they contain contingent propositions, explicit or implicit, computer models or simulations need to be empirically tested and justified. This seems an obvious point, but it often has been overlooked by proponents of simulation, especially enthusiasts, of which there have been more than a few. Just because a computer model is complex and convincing, and produces plausible results, it is not therefore valid. That it produces results using a computer or produces results that are numerically precise [in table or graph] -- even less do these facts guarantee validity of the model or the more general theory underlying it.

A strong tradition of computer modelling that has claimed special relevance to social science problems is the "systems dynamics" school, originating at MIT in the late 1960's and early 1970's] and associated especially with the names of Jay W. Forrester [author of such works as *Urban Dynamics*, 1969], and of Dennis L. and Donella H. Meadows -- famous or infamous, depending on one's view, for *The Limits to Growth*. The systems dynamics school has generated a large literature, both general works and simulations of particular systems, and has helped foster the development of software specifically designed for the modelling of dynamic systems with feedback.[10]

It is characteristic of much of the literature of the system dynamics school that more attention is paid to the building of models than to their testing or validation, at least in the traditional statistical sense. A basic hardback text from the MIT group [Roberts *et al*., 1983], for example, a work of over 500 pages, contains no chapter on testing, validation, or parameter estimation; indeed, these words don't even appear in the index. This exclusion apparently is deliberate. The authors include "model evaluation" as one of the phases in the model-building process, and comment:

> ...[N]umerous tests must be performed on the model to evaluate its quality and validity. These tests range from checking for logical consistency, to matching model output against observed data collected over time, to more formal statistical tests of parameters used within the simulation. Although a complete discussion of model evaluation is beyond the scope of the book, some of the important issues involved are presented in the case examples.... [p.9].

Whatever the intent, it is hard for the reader to avoid the impression that testing a model against real world data is less interesting and less important than model building.[11]

An earlier work from the same group [Richardson and Pugh, 1981] makes clear that the emphasis on model building rather than model estimation reflects a deep-seated attitude towards scientific and policy analysis, one somewhat at odds with traditional statistical methodology:

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10. The original language developed by the MIT group is called Dynamo. More recent programs in the same genre include Stella II., Modelmaker, and Vensim; and there are still others. All share the common feature that they are designed to make it relatively easy to build and run models of complex dynamic systems with feedbacks. They all work in essentially the same way, that is, as numerical solvers of difference/differential equations, and feature output arrayed by time.

11. This impression would be reinforced by the use of software such as Dynamo and Stella II which reflect a similar emphasis on building over testing both in the software itself and in the accompanying manuals. Modelmaker, by contrast, features utilities for testing goodness of fit of a model, and, along with Vensim, for parameter estimation from actual data.

> The systems dynamics approach to complex problems...takes the philosophical position that feedback structures are responsible for the changes we experience over time. The premise is that *dynamic behavior is the consequence of system structure* [p.15, emphasis in original].

That is, if one has the structure right, the details [e.g., specific parameter values] don’t matter so much. And later:

> ...experience with feedback models will convince the reader that model behavior really *is* more a consequence of structure than parameter values. One should therefore be more concerned with developing the arts of conceptualization and formulation than finding ultimate parameter selection methods. Our advice for beginners would be to estimate parameters with good statistics [data] but not Statistics [mathematical methods]. In the systems dynamics context the latter are a collection of power tools that just might cut off your intuition [p.240].

In general, they are skeptical about the value of “correlational approaches” and standard regression techniques [ordinary and generalised least-squares] when dealing with dynamic models with feedback [pp.238-39].[12]

Validating a model, in this tradition, is achieved primarily by comparison of model output of key variables with ‘reference behavior modes,’ essentially actually observed time-series measures of the phenomena of interest. But still the greater emphasis is placed on “causal understanding” -- how does the process really work? Regression equations, with coefficients attached to a set of distinct factors to reflect their relative importance, are viewed as uninformative, at least as a representation of process in an underlying system. In Abbott’s [1988] words, they reject a “representational” approach to linear regression models in favour of an approach that they feel accords better with our intuitions of how a system actually works.[13]

A late example in this tradition [High Performance Systems, 1996] criticises an econometric analysis of milk production, expressed as a function of GNP, interest

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[12]. They acknowledge development of more advanced statistical techniques that show “...promise for statistical estimation of parameters in system dynamic models...,” but in general seem to prefer a ‘bottom up’ approach to parameter estimation as opposed to attempts to estimate parameters from data on the dependent variable, that is, the variable whose dynamic behaviour is being modelled. One might view the increasing use of path analysis, simultaneous equations, and other structural equations modelling approaches as a move on the part of statistical modelling towards the systems dynamics tradition.

[13]. I have not yet encountered a discussion in the systems dynamics literature of what Abbott termed the ‘entailment approach’ to regression, that is, the use of regression analysis to test whether linear relationships predicted by a behavioural theory or model actually obtain, making no claim that the linear equation ‘represents’ the system at work.

rates, etc., on the grounds that the model nowhere mentions cows; and a model of human births [as a function of birth control, education, income, health, religion, etc.] on the grounds that the model nowhere mentions mothers [pp.25-28]. The recent text by Hannon and Ruth [1994] takes a more balanced and sophisticated approach towards blending dynamic modelling and more traditional statistical approaches.

The intellectual history of the systems dynamics tradition remains to be written.[14] Based on an incomplete review of the literature, I would hazard the guess that most proponents are theorists at heart, more interested in ideas about how and why things work the way they do, and less interested in the technical details of measurement, statistical modelling, parameter estimation -- the lifeblood of contemporary empirical research.

The work of the systems dynamic school seems to me to support my view that computer modelling is a highly theoretical exercise. If they do not emphasise model validation, they do not deny its necessity. At most, they often seem lulled -- and lull the reader -- into a semi-conscious acceptance of rich, complex, and plausible computer models as reality – one colleague speaks of students of this approach as being 'seduced' by their models.

A central part of the problem is that there seems to be no clearly defined or 'neat' processes for testing simulation models, processes analogous to goodness of fit measures for statistical models, of tests of significance and magnitude for coefficients attached to particular variables. Part of the difference arises from the fact that computer models may often contain variables for which there are no empirical measures. Another difference is that the computer model often assumes complex, non-linear functional relations between variables, and must postulate some value for key parameters, values which may not be firmly established through empirical research.

A consequence is that it is often possible – or even easy – to modify a given model until it agrees with some empirical result – after the fact. That the model can yield predictions in close accord with empirical data is an important fact. But it does not validate the model: 'correct' predictions can result from a model with incorrect assumptions and inputs. In any case there may well be other models which predict the same empirical results as well or better.

My approach to this problem is to view the validation of a complex computer model of the sort I have been discussing as being closer to theory confirmation than to the estimation of statistical models, or the testing of one or a small set of specific hypotheses, as in many contemporary multivariate analyses. Except in rare cases, one will not validate or disprove a model by means of one or a small number of experiments or empirical statistical analyses. The process will be more like that described by Miller in his account of 'confirmation':

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[14]. An early and thoroughly negative assessment is by the mathematician Berlinski, 1976. The economic and demographic critiques of *The Limits to Growth* are well-known.

> Confirmation, I will argue, is the fair causal comparison of a hypothesis with its current rivals. A hypothesis is confirmed just in case its approximate truth, and the basic falsehood of its rivals, is entailed in the best causal account of the history of data-gathering and theorizing out of which the data arose…. In arguments for hypotheses, as against textbook expositions of findings, the best scientists sound like honest, intelligent lawyers and like principled, mutually respectful people in engaged in political controversy' [p.155].

The overall process is thus a far cry from a chi-square or t test of a statistical model. In theory validation, unlike some specific empirical analyses, definitive conclusions and closure is harder to come by. Indeed, any theory and models derived from it will always remain provisional – the best available until something better comes along.

But validating a simulation model and estimating a statistical model are not at all mutually exclusive endeavours, even if they are different. The tension between the two approaches described above can be and needs to be reconciled, so that computer modelling leads to better empirical research and so that standard statistical methods may be of service in the validation of complex computer models. Several approaches can be mentioned.

A complex computer model or simulation can be tested in the classic way using the 'hypothetico-inductive' method. If the model is applied to a particular empirical situation, it predicts that certain empirical outcomes should be observable, perhaps a time series or a linear structure among empirical observed variables. The failure to observe these outcomes leads to weaker acceptance if not outright rejection of the model as formulated. Here the multivariate model is being used in the 'entailment' sense [Abbott, 1988]. There is no thought that the statistical model represents the system or its mechanisms. Indeed, this use of statistics to test complex computer models may help avoid the very reification that Abbot is concerned with in the 'representational' use of the general linear model.

An extension of this approach used by Jacobsen and Bronsen [1995] to test a systems model of deviant behaviour, might be called the 'multiple entailment' approach. They compare model predictions of deviant behaviour in Israel not just with one or two empirical examples [time series of deviant behaviour] but with as many as fifteen. That model predictions are in close accord with empirical time series in twelve of the cases studied is taken as strong evidence for the plausibility of model of deviant behaviour and the more general theory underlying it. Jacobsen and Bronson do not claim 'proof,' expressing a view that would be consistent with that of Miller sketched just above.

Note that both of the above approaches focus on comparing predicted output with empirical output. More comprehensive approaches to model validation are emerging, approaches that express the flavour of Meehan's use of the word 'isomorphism.' In general, they are as concerned with the relation to empirical reality of the inputs and internal workings of the model as they are with the relation to empirical reality of outputs.[15]

Each element of the model is examined separately for its empirical or logical validity. In my simple model of fertility [combining ideas from Easterlin's 'socio-economic theory,' and from Rosero-Bixby and Casterline's diffusion model; Burch 1997b], for instance, many of the inputs are of empirical data from surveys, and deal with well-defined concepts. Others [e.g., natural fertility] can be estimated based on well-developed procedures in demography. Relationships among variables are often logically true [surviving children equals total children times survival probability], or seem to represent common-sense behavioural assumptions [e.g., that behaviour is more apt to result from strong rather than weak motivation, or that behavioural responses to perceived external conditions are often delayed]. At the end, only a few of the elements in the model are questionable or arbitrary, notably, the conceptualisation and measurement of 'costs of fertility control.' But overall, the model is close to reality at most points, as well as predicting empirical time series closely. Again, it is not 'proven,' but its credibility is strengthened by a process of 'triangulation,' or what Miller might refer to as a process of 'causal, comparative, and historical confirmation.'

Hammel and Wachter have validated their SOCSIM model [a microsimulation model of household, family, and kinship] by showing that it can produce current population figures when run from 1900 to the present with the best available data for demographic input [see, for example, Wachter, 1997, and Wachter, Blackwell, and Hammel, 1997]. This is similar to the approach taken by climate modellers, who try to 'predict' current climate from long-term historical observations the presumed determinants, a task which has recently proven successful [R.A.K., 1997].

Recent versions of systems modelling computer software incorporate elements of this broader approach,[16] in important steps towards a reconciliation of the simulation and statistical approaches. Modelmaker and Vensim, for example, provide procedures for estimating a limited number of parameters in a model, given empirically observed output. Not all parameters in a complex model can be meaningfully estimated in this manner, and the procedure is not as cut and dried as least-squares. But, as in the case of my fertility model described above, when only a few parameters are unknown or in doubt, the procedure can be of great help.

Vensim also has a program feature which it calls 'reality check.' If we know certain things that must be true in the modelled system, we can specify them as conditions and have the computer check whether they hold in model runs. As a

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[15] . Note that this approach is at odds with the view of theory testing expressed by Friedman [1953] in economics or by Jasso [1988] in sociology, in which a theory is to be judged by its predictions not by its assumptions.

[16] . Such elements were not completely lacking from the earlier systems dynamics literature. See, for example the quote from Roberts *et al.* [1983] above, p.9.

simple example, a production model should produce zero widgets if the number of employees falls to zero. Some quantities in a model cannot increase without limit; others cannot drop below zero. If they do so, because of some complex set of interrelations within the model, then something is wrong with the model, and it must be re-worked. The model remains subject to common sense and sound scientific intuition.

**Concluding Comment**

Modern computer simulation or modelling has been developed largely by mathematicians, statisticians, computer scientists, and engineers. It requires numerical inputs and specification of functional relations, and produces seemingly precise numbers and graphs. Not surprisingly, many social scientists associate computer modelling or simulation with quantitative, empirical social science, with 'number crunching.'

Of the many types of models that have been constructed and used, many justify this association. But, I would argue, the association is not a necessary one. And, for 21st century social science, the most fruitful application of computer modelling technologies will be to the statement, manipulation, and testing of our more promising complex behavioural theories. This application does not represent as sharp a departure from past practice as may appear at first. Computer models of theories can be used to generate empirical predictions [implications, entailments] from our theories in order to subject them to statistical tests. Computer models can be used to explain, even if one takes the narrow 'covering law' view of explanation, but even more powerfully if one takes a broader view of explanation similar to those of Meehan or of Miller, as sketched earlier.

Computer models can be used to generate precise numerical outputs. But working with them also tends to heighten one's awareness that the precise numbers are largely the result of one's assumptions. This can lead to a greater emphasis on broad qualitative results, an idea that has always characterised that quintessential tool of the hard sciences, differential equations.

The key to all of this is that the computer and associated software has extended much more than our ability to do numerical computations. It has in effect extended our powers of logical inference and reasoning. We are able to deduce the strict logical consequences or entailments of systems of propositions much more complicated than can be dealt with using logic or even analytic mathematics.[17]

These systems will be richer and more realistic than those of modern economics, for example, based on mathematical deduction from a limited number of axioms, some of them behaviourally questionable. They will be more flexible and intuitive than those permitted by the 'covering law' approach to explanation, requiring

[17] . For a an interesting illustration of the relative advantages of formal mathematical solutions and computer models, see Timpone and Taber [1998].

verified empirical generalisations before one can even begin. Such theoretical systems have always existed in social science, but in the past their statement has often been less than clear, their logical manipulation somewhat less than rigorous, and their systematic testing problematic.

The newer approach will lay to rest the notion that one must ‘let the facts speak for themselves’ – an empiricist bias that can be traced to no less a figure than Newton – *Hypotheses non fingo.* It also will break free from an excessive concern with simplicity -- a reductionist bias that can be traced to William of Ockham and has been perpetuated by the dominance of a physics model of science. There will be less concern with where the assumptions in a theoretical system come from – empirical evidence, intuition, even fantasies – so long as they can be and eventually are subjected to rigorous empirical test. If the theoretical systems become complex rather than simple and elegant, so be it, if that is the character of the reality being studied.

In short, the computer will enable 21st century social scientists to match the breadth and depths of their insights with expanded powers of logical inference, leading to a true marriage of theory and empirical research.

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