MODELLING: FOR REAL OR FOR FUN?*  

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ABSTRACT. Models are reconstructions of nature. Their extrovert potentiality is the light they may throw on the real world, while their introvert attraction lies in exploring and transforming them without reference to the real world. Fashions in model construction and use oscillate between the extrovert and introvert poles. Because models in network analysis are basically scientific, the movement of fashion is not entirely repetitive but has a forward cumulative component as well.

Although the literature on models in science is vast, many social scientists query its relevance. Some of us, myself included, are sufficiently old-fashioned to think that the real world should remain the main focus of scholarly attention in social science. The philosophy of the social sciences, of which these discussions of models form an important part, seems to add very little directly to our understanding of that real world. If so much has been said already, and to such little use, why then add to the jungle of words?

Yet two features of this literature may justify a modest attempt to add to it. First, there is little evidence that network analysts spend much time browsing through this literature. It doesn't get cited in Social Networks and Connections. The word 'hermeneutics' never appears there, nor do the authors of these articles speculate independently on the subtle issues that dominate the discussions of the philosophers of science and others who write professionally about models. By and large network analysts press on regardless. They produce a profusion of models uninhibited by any doubts about how these might fit into some typology and unconcerned about the ontological and epistemological status of the connection between the world and the model. Indeed, there is a striking and refreshing contrast between the essays that I read continually in Cambridge and the corpus of network analysis. In our student essays the words epistemology and ontology luxuriate, largely at the expense of any data from the real world, whereas in the writings of network analysts these words are rare birds indeed.

The second relevant feature of the literature on modelling in social science is that it is almost completely classificatory, morphological and normative, and not at all sociological, even when written by card-carrying sociologists. There are learned and fascinating discussions on topics such as the difference between models and paradigms (Hesse 1976), between emic and etic models (Pike 1967), and between analogue, iconic and symbolic models (Miller 1967). There are also historical accounts of how the key model in scientific thought has, through the centuries, shifted from society to the wheel, the balance, clockwork, the organism and so on (Deutsch 1951). But there is almost nothing on what we might call the sociology of modelling in the social sciences, at least not at close range. Kuhn's well-known Structure of scientific revolutions (1962), with its later modifications, and competing proposals from Popper, Lakatos, Feyerabend and others, provide large-scale accounts of how in natural science one constellation of concepts, axioms and propositions gives way to another. But these accounts cannot easily be modified to allow for the intrinsic differences between the natural and the social sciences (cf. Putnam 1978:55-65). Furthermore, their attention is directed at paradigm shifts, the major changes in scientific thinking, rather than at the waves of intellectual fashion that animate the scene between one major change and the next, during periods of so-called routine science. Certainly no-one so far seems to have looked at network analysis from this point of view. Yet the sub-title of Sam Leinhardt's collection of articles, Social networks: a developing paradigm, invites a sociological scrutiny of how routine social science, using the network paradigm or model, does actually develop.

But as Harary and I have pointed out recently (Barnes and Harary n.d.), there are good structural reasons why network analysis, unlike graph theory, has not yet acquired an orthodox pedigree. I doubt if it ever will. All I want to do here is to throw into the ring some ideas about how modelling develops within a given intellectual and academic context, and to suggest how a knowledge of this development might affect future praxis. In other words I propose to offer a meta-model, a model of the modelling process. In doing so, I am reminded of the sentence with which Barry Wellman ends his excellent guide to network analysis: 'the capacity of network analysis to pose questions would profit from an enhanced capacity to provide answers' (1982). The same cri de coeur can be made with meta-modelling.

We can begin with the observation that everyone seems to be in favour of models. As May Brodbeck says: 'Models are Good Things'. Then she goes on to say, more questionably, 'And if models are good, mathematical models', needless to say, are even better' (1959:373). But she questions this latter statement herself so severely that she says we should abandon the term 'mathematical model' completely. What we have to ask is this: if models, mathematical or otherwise, are good, what are they good for?

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This question leads us at once into the treacherous field of definitions. There are dozens of definitions, and there is no point in adding to the roster. The best one for our present purpose is that given by Levins, a biologist, who says simply that a model is 'a reconstruction of nature for the purpose of study' (1968:8). The main point of my thesis in this paper hinges on the word 'study' in Levins' definition. What are we studying when we use a model?

Levins himself is quite definite about what he means. He refers to the study of nature. Nature is reconstructed and simplified in a model so that nature can be studied more effectively. But his definition can also be read in another way. The 'study' in question can be the study of the model rather than of the nature from which it has been derived. For as an intellectual object the model has two attractions for us, and they compete with one another. Let's call them the extrovert and the introvert potentialities of the model. On the one hand we can use models to increase our understanding of the real world; on the other we can manipulate and transform them so that they tell us more about themselves. The latter point is well stated by Lave and March, in their remarkably didactic textbook, An Introduction to Models. They say 'Much of the power, beauty, and pleasure of models comes from inventing and elaborating them and that 'playing with ideas is fun' (1975:vii, 3). Power, beauty, pleasure and fun: these are attractive qualities, but in general they stand in sharp contrast to those of understanding, knowledge, wisdom and utility which we would expect to spring from a study of the real world.

I am reminded of Einstein's remark that 'the most incomprehensible thing about the world is that it is comprehensible' (Frank 1950:118) and would place that alongside another comment of Lave and March: 'God has chosen to give the easy problems to the physicists' (1975:2). If the world that Einstein had in mind includes the social world, and if this is truly difficult but comprehensible, then its study, whether by means of models or by any other means, is necessarily an austere and arduous task. It is therefore not surprising if those who should be struggling with that task are sometimes diverted from it by the pleasure and fun offered by the study of models. On the other hand, given the chronically sorry state of the real world, there is continual pressure - political, financial, moral, and intellectual too - for social scientists to turn their attention outward from the ivory tower, to leave the introverted fun of manipulating models and turn to the extroverted task of manipulating real data, or even the real world itself.

In other words, there seems to be a dialectical relation between problems posed by the real world, whether these are generated by social causes or by intellectual curiosity, and the content of the intellectual armoury we collect and elaborate within the ivory tower (cf. Dahrendorf 1968:256-278). As with armours of other kinds, the weapons we construct are not necessarily intended for any specified target, or indeed for any target at all. Both terms in the relation have their internal dynamic. The real world continually generates new problems without much regard to our ability to solve them, while in the ivory tower the model-makers and other builders of analytic tools beaver away, largely absorbed in their own scholastic debates. Yet the relation between the real world and activity within the ivory tower persists, and is dialectic or two-directional. Each term impinges on the other. To some extent our perception of problems in the real world is influenced by our ideas about what aspects of the world are changeable, at least in principle, and what are immutable. Likewise the development and elaboration of intellectual weapons is not entirely an autonomous enterprise but responds in part to pressures from outside.

There is thus an oscillation of attention by social scientists, including network analysts. Sometimes the emphasis is on the real world and its troubles. We have to provide solutions, either practical or intellectual, to problems we have not chosen, making the best use we can of existing analytic tools. At other times attention is directed more towards elaborating the armoury, without bothering too much about whether the weapons might, or even could, ever be used, in anger or in earnest, on real world problems. This pleasurable activity, however, eventually attracts the critical attention of colleagues who are frustrated by their inability to understand what the model-makers are doing, or who see support for the pursuit of their own extroverted interests threatened by this flagrant introspective scholasticism. Thus we have for example a complaint directed specifically at the work of some of us here today:

... a considerable number of them are almost completely involved in technical problems. They are busy refining existing concepts and enlarging the arsenal; they try to make elaborate classifications, and they attempt to inject network analysis with mathematical concepts and procedures in order to give it a more 'scientific' tone. Evidently for these network 'technicians' ... there is not much time for realizing that network analysis is meant to solve anthropological problems (Bax 1977:3).

Price (1981:308) makes a more pertinent criticism when she complains that 'Assumptions about the social rules and resources employed in the production and reproduction of social patterns are seldom explicitly discussed in sociological network studies. ... many network studies ... take as their point of departure a view of culture as a coherent system of symbols and meanings. Apparently, a passive, adaptive, receptive view of human agency predominates. ... Esoteric network analysis can be profoundly misleading when transposed into applied research agendas without specification of the assumptions on which such work is founded'.

A well-aimed methodological criticism is made by Piddington in an article about irregular marriages made by Australian Aborigines. Australian Aborigines, as I am sure you know, have themselves produced models of such elegance that Lévi-Strauss (1956:143) was moved to credit them with the invention of
sociology. Yet they have suffered more than most ethnic groups from mis-modelling imposed on them by outside observers. Piddington (1970:341-342) writes that the construction of foreign models 'leads to endless discussions about hypostatized symbolic systems. Though these may provide an entertaining diversion for frustrated mathematicians they have the fatal weakness that they do not lead back to the operations which alone could test their validity. This last requirement is an essential characteristic of a valid scientific theory'.

In response to this kind of criticism, and to pressures from the real world, there is thus an irregular temporal alternation between one emphasis and the other. Not all social scientists move in synchrony, and there are plenty of reverse eddies. Nevertheless there are also discernible tides, at least on a continental scale. In periods of fervent analytic inventiveness the real world is neglected while new techniques are developed, often as ends in themselves. Some of these techniques are later found to have practical applications, but membership of this sub-set cannot be predicted in advance. At the other end of the pendulum's swing, in periods of coping pragmatically with urgent practical issues, existing techniques are mobilized and employed with positive effects; the world gets changed as the result of applied social research, and changed for the better, at least from some points of view and in the short run. But unforeseen consequences, and new practical problems, soon upset the applecart of social engineering. The pundits begin to call for better analytic tools, and the pendulum starts to swing back again. In the intervening periods, between tides at slack water, when political pressures for immediate answers to social problems are moderated, there is sufficient leeway for a fruitful link between empirical research and intellectual innovation. It is then possible to hoist the famous banner of the Tavistock Institute: 'No research without therapy, no therapy without research'.

Thus the relation between model building and the real world can best be seen as oscillatory or cyclical, rather than as stable and static. Maybe this oscillation would be sustained by social processes even if the models we build fitted the data from the real world rather more closely than they actually do. In fact the fit, as we all know, is never as close as we would like it to be. Our dissatisfaction with our models, at least whenever the pendulum departs from the introspective end of its swing, generates an additional force for instability. Levens, the biologist I referred to earlier, puts the point well when he says 'There is no single, best all-purpose model. In particular, it is not possible to maximize simultaneously generality, realism, and precision' (1968:7). All three qualities are desirable, but since we cannot achieve all of them, the way is open in model-building for those cyclical instabilities so beloved of political scientists in their study of multi-cornered contests (Kramer and Hertzberg 1975:371-374). Indeed, nearer home, Anatol Rapoport (1959:371) has cautioned social scientists that they 'should not demand realism from the mathematician's models but only pertinence'. Max Black (1960:45) is rather more pessimistic when he writes that 'more commonly the mathematical treatment of social data leads at best to "plausible topology" . . . qualitative conclusions concerning distributions, of maxima, minima, and so forth. This result is connected with the fact that the original data are in most cases at best ordinal in character' (cf. Boulding 1952:73).

Should we accept as inevitable this continual ebb and flow of intellectual style or should we try to restructure our activities so that routine social science, or at least routine network analysis, can proceed in a more straightforward manner? Before tackling this question I would like to refer briefly to physics, that discipline whose image casts so long a shadow over the whole of scientific activity. In physics there is an accepted division of labour between the model-builders, the theoreticians as they are called, who still work with blackboards, pencils and paper, and the experimentalists who wear white coats and devour enormous research grants. This division seems to work, at least as shown by both the enhanced intellectual understanding of the physical world and the enhanced ability to manipulate it which physicists have achieved in the last hundred years or so during which this division has existed. Would a similar division of labour between theoreticians and applied social scientists yield equally spectacular results? Would each branch then be able to pursue wholeheartedly its own objectives, the introvert and extrovert aims of social science as I have labelled them, undistracted by the swing of the pendulum of intellectual fashion? I doubt it. It is always hazardous to rely on analogies with natural science in prescribing for social science, so we should in any case think twice before trying to model ourselves on the physicists. But there is another piece of evidence nearer to hand that also points against adopting this division of labour. Mathematical psychology is a specialism which provides a much more plausible picture than does physics of what might happen if network analysts were to partition themselves into pure and applied. The writings of mathematical psychologists are virtually unintelligible to most of their colleagues in other branches of psychology, so that the division of labour generates not greater productivity but merely greater segregation; the ivory tower becomes a ghetto (Barnes 1972:1422). I think there is a danger that this may happen in sociology, even in network analysis. In any case we should remember that what looks pure in one context becomes applied in another. In my own university, for example, we have a department rejoicing in the name of 'Applied mathematics and theoretical physics'. The purists even in the physicists look askance at the eyes of the mathematical Brahmins. It seemed to me essential that pure and applied should in social science remain a continuum along which individual socialists are encouraged to wander, moved by their curiosity and their conscience and talking to everybody they meet on the way.

The evidence for the existence in social science of the oscillatory model I have sketched must, as they say, wait for another occasion, less light-hearted than this one. In brief, I think that in
network analysis the initial impetus came from a desire to understand the real world. We had only very crude analytic tools. Enthusiasts then started to develop tools for their own sake, and the link with the real world was weakened. Some of these tools looked very powerful, but not until a great deal of spadework had been done on both the hardware and software of computers could they be used. My guess is that the tide is beginning to turn again, and that the next few years will see much more attention given to questions of application and falsifiability as we confront powerful analytic tools with more data from the real world.

However, the forward glance I want to make is directed not only at what should be our policy for the next five or two years but also at what consequences there might be for policy in the longer term if the oscillatory meta-model of social science has any validity. If we realize that there is this alternation of intellectual fashion, should we continue as before, naively letting the pendulum swing to and fro but pretending not to notice the periodicity in its movement? Or, following in the footsteps of Maynard Keynes, should we aim to influence the course of events by trying to damp down the swings, by swimming against the tide? In periods of heightened introspective activity should we exhort our colleagues to leave the fun and games and start collecting data from the real world? And, in periods when there are urgent calls for prescriptions on how to deal with, for example, declining morale in inner cities, or excessive use of energy, or inefficiency in the use of social services, should we then demand time to elaborate our intellectual and analytic armoury rather than base our policy recommendations on findings we know to be inconclusive? I suppose there is also a third possibility to consider, of trying to increase the amplitude of the pendulum’s swing, but I am unable to think of arguments that would support this kind of strategy. However the other two possibilities are, if I may be allowed to call on the language of the United States Navy, strategies rather than tactics, and we are well advised to have our plans ready for action at both levels even if, at the strategic level, we decide after due deliberation that we should content ourselves with nature or history taking its course without trying to change it.

We should also be aware that there are other long term movements in science that are likely to impinge on our praxis as social scientists in ways that are difficult to predict. One of these movements particularly relevant to network analysis is that from positivism to epistemological populism (Barthes 1981:22). Almost everything published in the field of network analysis is written from a point of view that many of my sociological colleagues, and I think many social anthropologists as well, regard as naively positivist. Network analysts confidently propose to model social behaviour in all its details, adopting the natural science paradigm without reservation and undeterred by any of the warnings sounded by philosophers of science against such superhuman, hubris risking, ambitions (cf. Putnam 1978:65; Barnes 1980:25-35).

Boldness, naivety and even brashness have a necessary place in the quest for scientific enlightenment, and I think that, at the tactical level, we should probably continue to press on as before. Positivism still has plenty of life left. But we should at least realize that much of the resistance to network analysis springs from a philosophical objection to what is seen as the reification of relations between individuals, and as an attempt to count or measure qualities which are inherently problematic, contestable or negotiable, and therefore uncountable. Maybe at the strategic level we ought to explore the possibility of developing a model of social action that is less a direct import from the natural sciences and which takes adequate account of the reflexive and self-correcting qualities of human behaviour.

There are also other processes that we have to treat as unidirectional and which ensure that the pendulum does not simply swing to and fro endlessly in the same trajectory. Mathematical reasoning is pre-eminently cumulative, so that the mathematically inspired models we construct become steadily more sophisticated with each swing. There is a good example of how an advance in mathematical thinking leads to an advance in model building in a recent paper by Seidman and Foster (n.d.). They propose a new way of looking at what they call, somewhat oddly, 'social events' and 'pseudo-events'; these are the partially overlapping sets of neighbours in social space that are mobilized for a variety of local tasks. They give a discursive account of their analytic procedure and then say 'In a more formal treatment, we adopt the concepts, terminology and notation of the mathematical theory of hypergraphs. ... In fact, the hypotheses that were developed in the preceding paragraphs would have been very difficult to conceive and state without the hypergraph formalism'. As you know, it is only fairly recently that Berge's work on hypergraphs (1973) has become accessible in English. What I find interesting is that, although they claim to be dependent on the formalism of hypergraphs, Seidman and Foster are able to present a lucid description of their analysis without once invoking the technical terms hypergraph and hyperedge. We should defend our right to use jargon when necessary, while remembering that we preach to our colleagues, numerate and innumerate alike, not to mystify them but to enlighten them. As Fraser Darling (1947:77) once defined it, 'good research is orderly thinking plainly said'. That so much activity in many branches of social science consists of egression is a sad comment on our failure to follow that precept.

Technology is also, in perhaps not quite so strong a sense, a cumulative branch of knowledge and practice. We are all well aware of how formidable the impact has been on our professional activities by advances in the technology of information processing. Most of the models that have been produced in network analysis in the last ten years or so would still be mere toys, suitable for use only within the ivory tower, were it not for the computers through which alone they can be put to work.
From this point of view, network analysis, and a good deal of the rest of social science,
characterized as they are by a combination of oscillatory and cumulative movements, show themselves to
be indeed sciences; they belong to the tradition of Euclid rather than of Plato. There is a
significant cumulative movement in philosophy but, at least to the outsider, the oscillations seem to
outdistance the forward advance. In the creative arts the great masterpieces are never superseded.
Even at this moment, classical Greek tragedies are being acted before full houses more than two
thousand years after they were first performed. The plays of Aeschylus and the philosophical works of
Plato remain therefore permanently in print, an enduring source of employment for a thriving exegesis
industry. The books of Euclid have disappeared long ago from the reading lists of students. One of
the discritical characteristics of science, in contrast to the humanities, is that landmark books go
out of print, for they are superseded by later work in which their once new findings are incorporated
and surpassed. We should remember too that we, in particular among social scientists, follow in the
steps of Galileo rather than those of Plato's countryman, Aristotle. For it was the switch from the
study of attributes, as practised by Aristotle and characteristic of the classic and medieval worlds,
to the study of relations which led both Kurt Lewin (1933:5-10) and Levi-Strauss (1963:33, 101, 307) to
see in Galileo the pioneer of modern structuralism (cf. Mach 1960:168). Yet the works of Galileo, like
those of Euclid, are today sadly but appropriately neglected as compared with those of Aristotle. Thus
as our own modest contributions to knowledge slide into early oblivion, gathering dust unsold and
uncited, we can console ourselves that this is evidence that we are indeed scientists and not litterati.
In such a context we may surely be excused for forgetting that instant obsolescence is a necessary but
not a sufficient indicator of scientific writing.

One aspect of the oscillatory model that has been implicitly recognized by many writers on network
analysis is the shift from the use of the notion of network as a metaphor to its employment as a precisely
specified model. I think that most writers, myself certainly included, have seen this shift in Wiggish
terms, as a forward and irreversible step in the right direction, a cumulative rather than an oscillatory
movement. There is some support for this view from Max Black, when he says that 'Perhaps every science
must start with metaphor and end with algebra; and perhaps without the metaphor there would never have
been any algebra' (1960:64). However I now believe that we would be foolish to think that our specialism
has become so mature that we can dispense with metaphor. The recent appearance of notions of charm and
colour even in the world of particle physics should alert us to the heuristic power of metaphor.
Klovdal (1981), for example, has drawn attention to the importance of visual imagery in achieving
comprehension of network phenomena and has shown how computer graphics can be put to good use to generate
images that it would be impracticable to construct by any other means. The metaphors of graph theory have been of enormous benefit to the analysis of social
networks (Barnes and Harary n.d.). Nevertheless we should avoid becoming hooked inescapably even on nodes
and arcs. Max Black again has something pertinent to say: 'The more persuasive the archetype [the model]
the greater the danger of its becoming a self-certifying myth'. Yet he goes on to say that 'a good
archetype can yield to the demands of experience', which may perhaps be read as support for the oscillatory
meta-model. As Braithwaite (1953:93) reminds us, echoing Edmund Burke, 'The price of the employment of
models is eternal vigilance'.

If, ready to pay this price and remaining appropriately vigilant, we accept for the moment the
validity of the oscillatory meta-model of scientific activity, how should we classify it? Clearly, the
glaring absence of any real data in this paper places my model firmly in the introspective category; it is
an exercise in meta-modelling just for the sheer fun and pleasure rather than the contribution it makes to
solving the troubles of the world. But if I have tried to live up to Lave and March's dictum that playing
an exercise in meta-modelling just for the shear fun and pleasure rather than the contribution it makes to
validity of the oscillatory meta-model, how should we classify it?

To answer that question, we obviously have to turn to Douglas Hofstadter, whose Gedel, Escher, Bach
(1979) has thrown so much light on self-referencing and allied phenomena. Hofstadter has recently made
a distinction between what he calls healthy and neurotic sentences. A healthy sentence is one that,
so to speak, practices what it preaches, whereas a neurotic sentence is one that says one thing while
doing the opposite (1982:14). The single word 'Terse' is an example of a healthy sentence, whereas a
neurotic sentence is one such as this: 'Proper writing - and you've heard this a million times - avoids
exaggeration'. Hofstadter's contrast between healthy and neurotic sentences can easily be generalized
to larger entities, such as learned articles or even unlearned keynote addresses. Clearly what we are
now reading is a neurotic address. I have constructed for fun a model which generates the
instructi: thou shalt model for real, at least for the next five years or so. I am hoist by my own
petard.

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