

Relevance, Realism and Rigour: A Third Way for Social and Economic Research

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1 Conventional alternatives

The model based approaches to economics and the verbal and philosophical approaches to sociology normally coexist without much traffic between them. The concerns of the two research communities are, by and large, simply different. When it comes to management research, however, there is a substantial overlap in the subjects of interest and, as a result, the respective approaches of the economist and the sociologist are frequently in conflict. Economic modellers and sociologically oriented, qualitative students of management both claim that their research is relevant. The stereotype economist claims rigour and asserts its importance while the stereotype sociologist claims realism and denies that realism can accommodate rigour.

The purpose of this lecture is to address that conflict, to assess the validity of the arguments of each side and to draw some conclusions and recommendations for the future of social research. Much of the discussion will focus on management research in particular because of its close relationship to practice and its problem and policy oriented concerns.

The third way indicated in the title is not just the ageing buzz word. It is an important approach recognising that social structures and institutions are products of individual behaviour and action. In order to live and work together, people develop norms of behaviour, common beliefs, notions of responsibility, right and duty. While this has been recognised both in the political arena and in the academic literature, social scientists have not yet developed the concept of the third way as a useful approach to policy analysis. In addressing the conflict between economics-style and qualitative social research, a means of developing the third way into a set of practical tools of policy analysis will be defined.

The lecture is organised around the following four points:

- Economics, as developed over the past half-century and more, is not useful for the analysis and support of formal policy; it should simply be ignored by serious social scientists

- Social scientists developing and using qualitative research methods, particularly in management research, have rejected any kind of formal modelling because, understandably but wrongly, they identify all formal modelling with economics
- While qualitative social research, particularly in the management disciplines, can be too introverted to be relevant for general support of social policies, the detailed understanding of social processes developed by researchers working in that area can usefully inform the development of social models and a model based science of management and social policy
- Methods developed by social simulation modellers in particular are well able to reflect the detailed evidence of qualitative social and management research and, moreover, to do so in a way that captures the evolution and development of social institutions in a manner that is entirely consistent with the third way.

2 Economics: irrelevant, unrealistic, rigorous (sometimes)

Generally speaking, verbal, qualitative description and analysis is more expressive than formal models. The formal models rely on a clear and prominent syntax to avoid ambiguity in the specification of relationships among entities which are frequently but not exclusively mathematical symbols. Qualitative analysis relies on semantics to convey an impression and to share understanding in a looser but richer way than is possible by syntax alone.

Economic analysis is frequently neither expressive nor rigorous. But when it is rigorous, it is surely not expressive. This proposition is demonstrated by means of three examples.

2.1 The Humbug Production Function

In 1957, Robert Solow published "Technical Change and the Aggregate Production Function" in one of the leading economics journals, *The Review of Economics and Statistics*. In this article, Solow adopted the well-established theory of production captured by a *production function*. The production function states that aggregate output (denoted Q) for the whole economy is determined by inputs of capital (K) and labour (L). Now, if you regress gross domestic product (GDP) against

a measure of domestic fixed capital and employment, it turns out (as would be expected) that realised GDP in any year lies to one side or the other of the value indicated by the production function. Solow interpreted this difference as a result of technical change and took that difference explicitly into account in his statistical analysis. By making a whole host of assumptions – Solow was completely honest about this – he virtually invented a series of values for capital¹ and assumed a particular (and peculiar) characteristic of technical change and then separated out changes in outputs due to that technical change and changes in outputs due to changes in the amount of capital employed per unit of labour. Figure 1, showing the relationship between output per worker and capital per worker after eliminating the effects of “technical change”, is reproduced from Solow (1957).

¹ “The capital time series is the one that will really drive a purist mad,” wrote Solow. “Ideally, what one would like to measure is the annual flow of capital services. Instead one must be content with a less utopian estimate of the stock of capital goods in existence.” Moreover, “Lacking any reliable year-by-year measure of the utilisation of capital, “ assumes unutilised proportion of the capital stock is the unemployment rate.

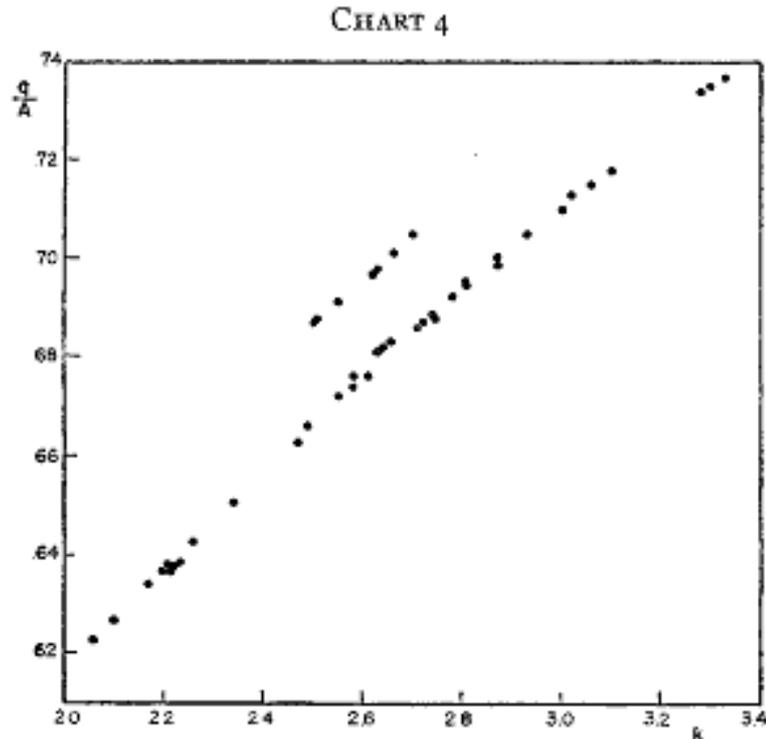


Figure 1: Solow’s production function – “technical change” removed

Note that seven points are above and to the left of the main body of points forming a slightly curved line. This set of points stumped Solow until Warren Hogan (1958) pointed out that they were due to an arithmetical error in calculating those data points. Hogan looked for the arithmetical error because he knew what is now common knowledge to those who care to look at the literature: the closeness of the relationship between output and capital, as calculated by Solow’s technique, depends only on the extent to which the distribution of income (*i.e.*, the fractions of income going to wages and profits, respectively) is constant over time. During the period for which Solow took his data, the share of wages in income in the USA was virtually constant at about 65 per cent.

This point was argued verbally by Hogan, who concluded:

The plain fact is that we could insert any set of random numbers in the capital stock series and still get a production function, net of technical progress, with the same close fit.

Hogan’s critique was followed 16 years later by Anwar Shaikh (1974) who proved Hogan’s verbal demonstration algebraically. He then took an invented set of points spelling out the word “HUMBUG” to relate output to capital. Shaikh’s diagram of this “data” is reproduced as Figure 2. He added the assumption that the

profit share was constant at exactly 35 per cent and applied Solow's technique to the data. The resulting "corrected" production function is reproduced as Figure 3.

FIGURE 1. — THE HUMBUG ECONOMY

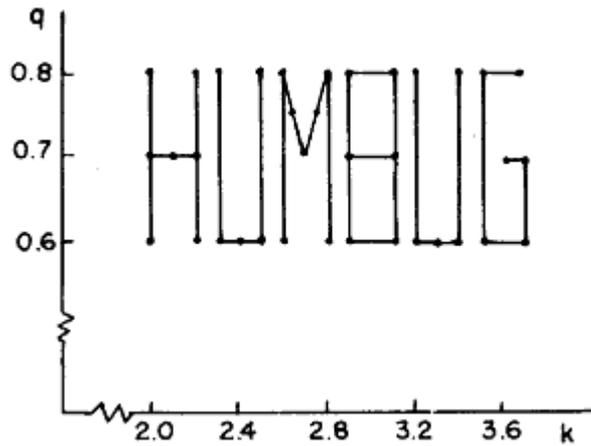


Figure 2: Shaikh's "data"

FIGURE 3. — UNDERLYING HUMBUG PRODUCTION FUNCTION

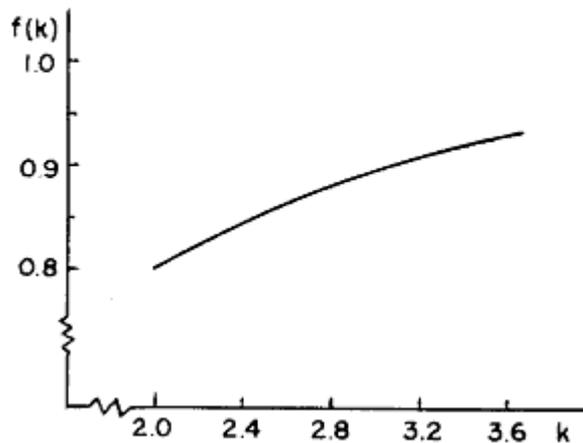


Figure 3: Shaikh's "production function"

The conclusion is inescapable: Solow's technique for distinguishing between the effects of technical change and the effects of capital investment does no such thing. At best, it provides a complicated measure for the constancy of income distribution.

So was that the end of Solow's analysis of technical change and the aggregate production function? Not a bit of it. Since 1981 – the start of the Social Sciences Citation Index data – there have been 454 articles citing Solow's 1957 article, 319 of them since 1990. During the same period, Hogan's paper was cited twice and

Shaikh's eight times. All of the citations to Shaikh's papers were by economists I recognised as heterodox (Sam Bowles and Geoff Hodgson, for example) or there they were in fringe journals (*Review of Radical Political Economy*, *Cambridge Review of Economics*). It is safe to say that the critique, accepted in substance by Solow and never successfully refuted, has not prevented the paper and the technique from remaining hugely influential. During the last UK General Election, the present Chancellor of the Exchequer was reported to have said that his policies would be guided by neo-classical endogenous growth theory. That theory is nothing more nor less than an elaboration of the Solow's technique for measuring technical change.

2.2 Information and Markets

Conventional wisdom among economists has it that, if prices are sufficiently flexible, then supplies and demands will be equal in all markets. Frictions and disturbances of one sort or another might cause there to be some excess supplies and demands, but these divergences are temporary.

If there is a theoretical basis for this view, it is general equilibrium theory. In the most general versions of the theory, all transactions are agreed at the start of time. Each transaction is contingent on a set of events pertaining at the time the transaction is to be completed. For example, an individual might contract to buy an umbrella in Duluth, Iowa on January 12, 3944 provided that it is raining and the individual is in Duluth on that day. Since all of these models require there to be a given set of individuals defined by utility functions and probability distributions for the occurrence of every possible event at every date, any individual around at the Creation is assumed to be around forever – including on the date January 12, 3944 (my 2000th birthday).

The first of these models was due to Arrow and Debreu (1954).

The first economist to pursue the consequences of assuming that individuals in such a system could agree and complete transactions at every one of a sequence of dates was Roy Radner (1968). The implication found by Radner for the existence of a general equilibrium when there are spot markets (*i.e.*, markets in which goods are exchanged for money at the same time as, for example, in a shop) is this:

Agents can have rules to determine their supplies and demands for the various goods and services available to them in different circumstances. In the original general equilibrium model of Arrow and Debreu (1954), such rules were not

necessary because all the information that would ever be available was conveyed by prices. But when trading takes place and can be revised over time, then the ways in which other individuals behave will affect the consequences to you of your own behaviour. Moreover, the amount of information about other agents and their rules of behaviour grows without limit. Unless individuals all have sufficient computational capacities to calculate their own best rules of behaviour by identifying the rules used by the other individuals (and the effects of those rules on themselves), there can be no general equilibrium. If, however, computational capacities are limited, then eventually there will be more information available than the individuals can use. Consequently, general equilibrium cannot exist unless individuals have unlimited computational capacities.

This would seem to be an important result since it states formally that the theoretical basis of economists' views of markets requires buyers and sellers in all markets to have unlimited computational capacities.

Though by no means as influential as the Solow paper discussed above, the Social Sciences Citation Index records 68 citations of the Radner paper since 1981. None of those papers address Radner's conclusion that unlimited computational capacity is a necessary condition for equilibrium when spot trading takes place over time. Instead, they address a more restricted problem identified by Radner relating to the existence of equilibrium when individuals have different information about the environment but there is no spot trading. In that case, he proves that general equilibrium can exist.

2.3 *Rational expectations*

A significant set of the citations were in papers developing the theory of rational expectations.

The fundamental idea behind (and justification of) the rational expectations theory is that it would be wrong to assume that econometric modellers were smarter than the individuals who make the decisions that generate the time-series data used to specify and estimate the econometrician's model. If those individuals are really as smart as the econometricians, and if the econometricians' model of the economy is correct, then the individuals will also have specified the correct model of the economy and it will be the same as that of the econometricians.

One consequence of this line of reasoning is that, if all agents know the correct (econometricians') model of the economy, they therefore have the same model. By using this model to form their expectations, they must all have the same expectations and there is nothing analytically to distinguish one agent from another. The point can be put in two ways. Either there is a single representative agent or agents are homogeneous. In either case, individuals have nothing to learn about one another and the basis of the Radner problem vanishes.

All of the main models used for forecasting features of the UK economy incorporate the assumption of rational expectations or its close cousin, consistent expectations. Evidently, we can expect these models accurately to reflect the economy if we are either all alike or if there is only one of us.

Tom Sargent, one of the most influential and prolific contributors to rational expectations research, noted in 1993 that (for reasons that are not relevant here) rational expectations theory actually required individuals to know a great deal more about the economic system than the econometricians. He therefore proposed to substitute for individuals with rational expectations individuals with cognitive capacities represented by an optimising algorithm involving the evolution of successful rules of behaviour. These are called *genetic algorithms*. He then reported a number of simulation models incorporating such agents that converged to a rational expectations equilibrium. This work has given rise to a minor industry among economists that seems to work in the following way:

1. Write down a rational expectations model.
2. Determine the equilibrium configuration of that model.
3. Replace the rational expectations agent with multiple agents represented by genetic algorithms.
4. Simulate the model devised in step 3.
5. If the simulation converges to the corresponding rational expectations equilibrium, write up the results and send to a journal.
6. If the simulation does not converge to the corresponding rational expectations equilibrium, revise the model and/or the genetic algorithm and go to step 2.

Steps 5 and 6 of this procedure have been specified inductively on the basis of questions asked by me at seminars and workshops where such papers have been read.

2.4 *Economics and the real world: two quotations*

Quotation 1:

“I’m an economist – I don’t believe in anecdotal evidence”

This was actually and recently stated by an economist colleague. It is not an uncommon view among economists. One consequence of this methodological stance is that observation more direct than statistical analysis of a questionnaire survey or published data is not to be used to formulate the problems to be addressed. I am not saying that direct observation is never systematically applied to specify a problem, only that I know of no such cases and the applied economics literature is dominated by examples of statistical analysis without “anecdotal evidence”.

Quotation 2:

“For (the sake of) simplicity, we assume that ...”

A full text search of *The Review of Economic Studies*, one of the leading economics journals and available on the JSTOR database of journal articles at least five years old, turned up 33 (out of a total of 67) articles in 1994 and 1995 using the above quotation in one or the other of the forms given above. Taking them in context, all but one that I investigated asserted a special form of some mathematical function to represent behaviour and then asserted without proof that a more general form of the function would yield substantially the same result. The most common assumptions “for simplicity” place suitable mathematical bounds on the representation of the space over which individuals find their optimal behaviour.

In every case that I investigated, the purpose of the simplification was to recast the problem being considered into a form that could be analysed using conventional economic modelling techniques. *In no case was the technique modified to make it more suitable for analysing the problem.*

The combined effect of the reluctance to use “anecdotal evidence” and the willingness to make assumptions “for the sake of simplicity” is that, in general, economists do not specify problems in a manner that can be shown to address specific, real issues faced by actual decision makers (thereby to avoid anecdotal evidence) and, once a problem is specified, it is a legitimate practice among economists to recast the problem into a form that makes it amenable to analysis using the chosen technique of analysis.

2.5 *Conclusion*

A small, selection has been presented of the voluminous evidence available to demonstrate that economics is, *as if* by design, remote from historical reality and common observation. The reason is that only a narrow range of modelling techniques is acceptable to the economics profession and none of these techniques correspond to any observed manifestations of behaviour, social structure or social processes. This range of techniques is not static, but any new technique must amount to some variant of optimisation procedure.

When it comes to the analysis of serious issues of social policy such as public investment in education, neither formal rigour, nor logical coherence, nor any considerations of realism are criteria. No other conclusion is possible from the importance in the economics literature of Solow's analysis of technical change and the aggregate production function.

When analytical rigour is maintained and the most rigorous of economic analytical forms is used to extend the foundations of economics in a realistic direction, then fundamental problems thrown up by such developments are ignored. How else can we interpret the way in which economic theorists have ignored the Radner proposition that decision makers must have unlimited cognitive if markets are to function as economic theory requires?

In summary, when economists are concerned with rigour, they cannot be concerned with relevance and when concerned to be relevant, then rigour is not a criterion. Moreover, assumptions for simplicity and the rejection of anecdotal evidence ensure that realism is not an issue.

3 Qualitative research: Not rigorous, realistic (sometimes), relevance uncertain

The foregoing discussion of economics supports the rejection of *economic* modelling by the qualitative management research community. But it does not follow that, just because economists use models to do bad science, modelling is bad science. The deep problem with economics is the commitment of economists to a narrow range of analytical techniques. It is not entirely clear that the qualitative management research community wholly escapes that problem. At the very least, that possibility warrants some assessment.

A top-down approach to the question of whether technique dominates substance begins with the distinction made by the qualitative research community between positivism and phenomenology. A clear initial statement is offered by Easterby-Smith, Thorpe and Lowe (1991, p. 24):

The starting point ... is the idea that reality is socially constructed rather than objectively determined. Hence the task of the social scientist should not be to gather facts and measure how often certain patterns occur, but to appreciate the different constructions and meanings that people place upon their experience. One should therefore try to understand and explain why people have different experiences, rather than search for external causes and fundamental laws to explain their behaviour. Human action arises from the sense that people make of different situations, rather than as a direct response from external stimuli.

Qualitative research starts from the presumption of a socially constructed reality that, in principle, cannot be represented objectively. Not surprisingly, there are some more, and some less, extreme views of the social construction of reality and its incompatibility with objectivity. Indeed, Easterby-Smith et al. point out how the proponents of each of these approaches frequently adopt the practices of the other. At the same time, whole journals and books are devoted to questions of how it is appropriate to apply qualitative research techniques to the social sciences in general and management research in particular.

A recent book on information systems and qualitative research (Lee, Liebenau and DeGross, 1997) is a good example of the way in which technique can come to dominate over substance in methodological discussion even in apparently applied academic work. There are chapters on the main approaches to qualitative research. In one, Michael Myers elaborates three broad approaches to critical ethnography: holist, semiotic and behaviouristic with semiotic ethnography subdividing into “thick description” and “ethnoscience”. The holists apparently do not worry about the relationship between their own psyches and the ways in which they report their observations. They do insist on a need for empathy with the culture being studied. The semiotic school does not require empathy. “Rather, the ethnographer has to search out and analyse symbolic forms – words, images, institutions, behaviours – with respect to one another and to the whole that they comprise” (Myers, 1997, p. 280). Add to this an overlay of *critical hermeneutics* where “the object must be a

text, or a text-analogue, which in some way is ... in one way or another unclear.” (Taylor, 1976, p. 153; quoted by Myers, p. 281) Graft onto this notion the *hermeneutic* circle by which is meant that the whole of a text gives meaning to the parts which in turn determine the meaning of the whole. Myers goes on to assert that the “idea of the hermeneutic circle can be applied to the organisation as a ‘text-analogue’”.

So on this account we treat the organisation as analytically equivalent to a text about which we can say nothing objective or definitive. There is no reality and, so, no reality to capture.

Interestingly, Myers goes on in the same chapter to give an account of a critical ethnographic study of the introduction of an information system for a New Zealand mental hospital. In that account, it is clear that different stakeholders have different concerns, views and even fears about the introduction of particular modules of the information system. The introduction of the system was itself a consequence of political decisions taken by the New Zealand government.

Myers’ account of all this is clear, the relationships among the different stakeholder groups are stated explicitly as are the differences in views within stakeholders groups and the reasons for them. After giving this account, Myers then relates various aspects of his account to the critical ethnographic approach. In whatever way the critical ethnographic perspective influenced either Myers’ approach to the elicitation of the relevant stakeholder views and knowledge or the way in which his report was cast, this reader at least was unable to identify any way in which the account itself would have been less interesting or comprehensible without any mention of critical (or any) ethnography elsewhere in the chapter.

The conclusion to be drawn from this reading of that chapter and other articles and textbooks using the approaches of ethnography, grounded theory, case studies and the like is that they are all capable of providing clear, well constructed accounts of social processes that do not depend in any crucial way on the particular approach taken in the research from which those reports emanated. Moreover, these reports do not seem to be of a different *genus* than the descriptions of events used to specify empirically oriented social simulation models.

4 A role for the modeller?

The argument so far is that:

- Economic modelling is bad science, rightly rejected by the qualitative management research community
- Qualitative management research can and frequently does generate reports of historical episodes and corresponding social processes that stand independently of the methodological precepts of the investigator and reporter.

The issue to be addressed now is whether it is possible to do modelling as good social science and, if so, whether such modelling will be useful in qualitative social research. In this section, the issue of utility (*i.e.*, usefulness) is considered. Good model-based social science is demonstrated in the section following.

4.1 Five criteria of good science(including social science)

The following are offered as criteria of good science that apply to social science in particular:

4.1.1 The language of analysis should be expressive enough to describe the observable phenomena to be analysed

Reports of qualitative management research are typically highly expressive and are frequently extremely detailed. Proponents of the application of critical theory from literary criticism to management research argue that the use of language should convey emotional as well as the social content of the subject. At the other extreme from critical theory are logical formalisms that rely on syntax to represent relationships at a purely abstract level. Issues of concern in such formal approaches are the meaning of such relationships as belief, trust, reliability, responsibility, duty, and so on. There are contributors to the social simulation literature to take this approach. There is also in the social simulation community a number of modellers – I count myself among them – who seek to represent empirical phenomena more directly. The models produced by this strand of social simulation have much more semantic content and are less reliant on syntax. Nonetheless, because the models are written in computer programming languages, they have a formal syntax. They are also less richly expressive than the analysis of critical theorists and also less detailed in their descriptions of agent behaviour and motivation. Their purpose is to conduct the analysis of social processes, how they result from and condition the behaviour and cognitive processes of individuals, at a coarser grain of analysis than is the norm in qualitative management research. In practice, the expressiveness of the discourse

must be richer and more evocative as the grain of analysis is finer. Since there is no obvious limit to the fineness of the grain of analysis, there is no obvious limit to the expressiveness that *might* be appropriate in some circumstances. It therefore seems unreasonable to argue that some particular degree of expressiveness is preferable to any other unless it is reasonable to argue that some grain of analysis is preferable to any other.

4.1.2 Clarity in the expression of relationships

Whereas the previous criterion concerned semantics, this criterion concerns syntax. Some forms of syntax provide a greater clarity concerning, for example, cause and effect relationships than do other forms. Generally speaking, what we mean by a greater degree of formalisation is a greater reliance on syntax and a lesser reliance on semantics. The purpose of formalisation in such cases is to describe relationships and how relationships differ from one another with more precision. This is not quite the obverse of the previous criterion in that a greater reliance on syntax does not imply a coarser grain of analysis.

4.1.3 Integration within a single conceptual framework of evidence of different types and from a range of sources

A conceptual framework is an analytical device to relate analyses at different levels of abstraction and different grains of analysis. For some purposes, it might be appropriate to analyse social phenomena at a high level of aggregation such as the industrial sectors of a region or country. To understand those phenomena, it might be helpful to analyse the behaviour of (say) industrial organisations within the various sectors in order to develop a deeper understanding of the reasons for observed behaviour at a sectoral level. Again, there will frequently be some advantage to a consideration of behaviour within component elements of the organisation, and so on until we are considering the determinants of individual behaviour, the social consequences of that behaviour and the effects of those consequences on the individual – a process known as *structuration* (Giddens, 1984 being the classic reference). If the object of the overall analysis is policy prescription, then there are obvious advantages to being able to understand the essential mechanisms or behavioural norms that will make that policy effect at whatever grain of analysis is necessary. In such circumstances, it will clearly be an advantage if we could rely on consistency among models or qualitative reports at different grains of analysis.

4.1.4 Clear conditions of application of the conceptual framework

It is clearly important to know *when* a conceptual framework yields results that are useful and when results obtained within that framework are unreliable or misleading. In practice, the conditions in which a set of formal models or approaches to qualitative analysis will be appropriate are unlikely to be known with precision though that precision might increase with experience.

4.1.5 Specification of novel, plausible phenomena (that are then observed)

Prediction in the social sciences has a bad name due in no small measure to the claim by economists since Milton Friedman (1953) that it is a satisfactory alternative to making models in some sense realistic. Our experience is that social systems are too complex to support systematically correct prediction. Indeed, many social processes and institutions have been devised to enable decisions to be taken in the belief that prediction is not possible and is certainly not reliable. Such processes and institutions include the commodity markets and the purchase and sale of futures contracts; options on the financial markets and so on. In industrial production, goods that are time consuming to build and require large fixed capacities that are expensive to maintain are always produced to order and the producers maintain order books to keep them employed for years ahead. Indeed, without such order books they cannot obtain finance. Such practices are the norm in the heavy engineering industries where the effects of economic upturns and downturns are large and unpredictable.

This criterion includes, but is not the same as, prediction. It is possible for models to be developed in collaboration with domain experts and, in particular, with stakeholders. The models are used to explore with the stakeholders their own understanding of phenomena (at, perhaps, a coarser grain or higher degree of abstraction than ethnographic analysis would be used for). If some possibility is identified that seems plausible to the stakeholders and that they had not previously considered, then the analysis has changed their understanding. If in some circumstances the novel phenomenon should be observed, then confidence in the conceptual framework used to anticipate that phenomenon will naturally be enhanced.

In short, though successful prediction is not a necessary consequence of the use of conceptual frameworks in the social sciences, it is a desirable phenomenon that helps to distinguish the circumstances in which such a framework is relied on.

4.2 *Implications for the process of social research*

The five criteria of good science suggested above are best viewed as indications of the objectives that we, as scientists, should adopt in developing our techniques of analysis. The disquisition on economic modelling in section 2 indicated clearly that the adoption of a specific set of analytical techniques, in particular a narrow range of acceptable forms of model, prevent economists from addressing actual problems in a defensible manner. The defence of those techniques prevents economics from being both rigorous and relevant.

Perusal of the qualitative management research literature indicates that there is considerable discussion about the techniques of analysis that are appropriate to the analysis of particular phenomena and problems. That is, there is certainly an element of problem-oriented analysis in qualitative social research in general. That seems a likely reason for the rapid turnover in leading-edge approaches in sociology and, generally, qualitative approaches to social science. It also seems plausible that the lack of conventional criteria or any framework within which to discuss and elaborate approaches to the development of analytical technique accounts for what appears to the casual observer to be a lack of direction in qualitative technical development.

Moreover, while the rejection of formal modelling is understandable in light of the reputation generated by *economic* modelling, the restriction of analytical approach limits the testing of our analytical approaches and therefore the confidence that the users of our analyses of social processes and institutions can place in our conclusions.

5 Agent based social simulation and the third way

5.1 *Grain and formalism*

Two characteristics of analysis identified above are the degree of formalism and the grain of the analysis. Greater reliance on syntax coupled with a lesser reliance on semantics implies a greater degree of formalism. The definition of more highly aggregated composites of individuals or, for a given aggregation, a less detailed or more partial representation of such entities constitutes a coarser grain of analysis.

An intuitive view of the relationships among the models and approaches considered so far is set out in Figure 4. The Solow model was clearly less formal and rigorous than the Arrow-Debreu and the Radner models. In addition, the target representations of the Arrow-Debreu and Radner models included individual households and firms and, so, were at much finer grain than the aggregate production

function. It is also clear that the qualitative management models are less formal than any of the economic models and deal with individuals so that their grain of analysis is at least as fine as that of the general equilibrium models. As indicated in Figure 4, there appear to be a large number of qualitative social models at similar levels of grain and (lack of) formalism while the economic models tend to be less formal as they are at coarser grain.

The purpose of more coarse grained analysis in economics is to generate “rough and ready” representations for use in statistical/econometric analysis. In social simulation modelling, coarseness of grain is used to reduce the number of entities being considered so that the analyst can keep the number of relationships in view down to a “manageable” number.

It has long been known (Miller, 1956) that people can hold in short-term memory between five and nine “chunks” of information at a time. Such “chunks” can be more or less complex depending on the experience of the person with the information and relationships being considered. For our purposes, these cognitive limits are sufficient to justify coarsening the grain of analysis.

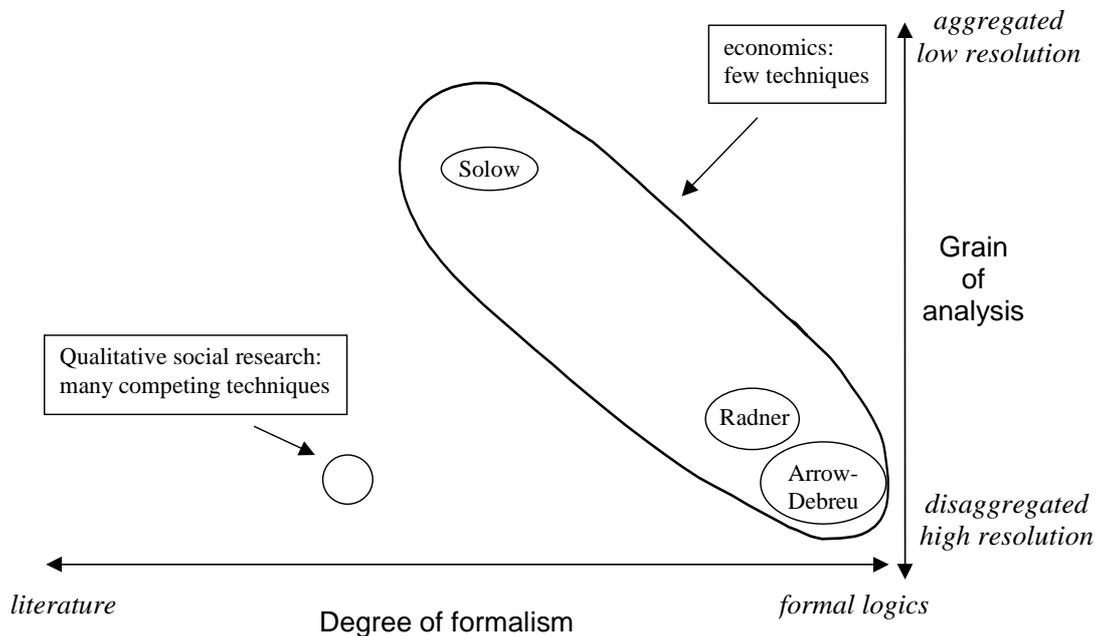


Figure 4

More formal analyses reduce the richness and expressiveness of the descriptions and specifications under consideration. An example of the difference was developed for two models by Moss (in press). One was the VDT (virtual design team) model reported by Jin and Levitt (1996) concerning research and development processes in

which cooperative action (the requirements for cooperation being captured by a critical path model) must take place within an organisational framework. The objective of the design process and steps required to achieve the design were largely specified in natural language and the sort of codes used to label nodes in a critical path model. The second model was Moss' (1998) model of critical incident management at North West Water PLC. In this model, too, actions and events were specified in effectively natural language so that events included *pumpFailure*, *intruder*, *chlorineLeak*, and so on. In order to relate these two models to one another and to a third model (Carley and Svoboda, 1996) already cast in formal terms, Moss (in press) restated the VDT and North West Water models so that the environment and the actions available to agents was represented by lists of digits and the effects of actions and interactions among different aspects of the environment were represented by matrices. Using this framework, Moss showed that the North West Water model is a special case of the VDT model and the Carley-Svoboda model is a special case of the North West Water model. Moreover, applied to the same issues of organisational structure, the North West Water and VDT models yielded the same results but the Carley-Svoboda model yielded different results from either of the other models.

The point of formalising the two semantically richer models was to facilitate comparisons between them at the same grain of analysis as the original models. The success of this exercise suggests that formalising representations of social processes can facilitate the development of suites of models with clear relationships among the models in the suite. The models within any suite of models would be cast at different grains of analysis.

The penguins in Figure 5 are icons for the sort of informal representations of real actors normally developed in the course of qualitative management research. The computers are icons for software representations of the actors. Such software representations are called agents in the computer science and social simulation literatures. By “agent” is meant an autonomous computer program that perceives aspects of its environment, is able to use those perceptions to determine appropriate courses of action and is able to act by affecting its environment.

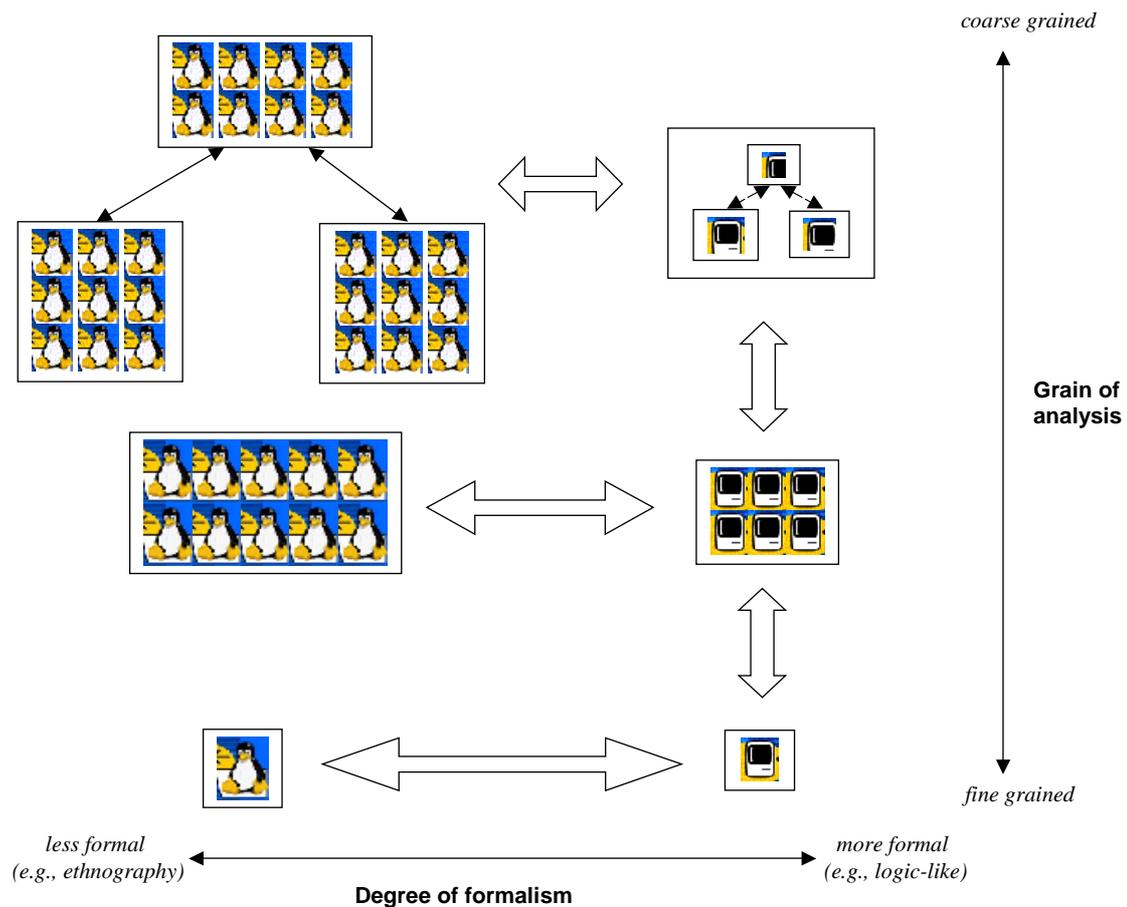


Figure 5: Grain of analysis and degree of formalism

The schematic of Figures 4 and 5 has no analytical content. In particular, the degree of formalism is not well defined though we know unambiguously that, for example, first order predicate logic represents a higher degree of formalism than does a critical ethnography. Much the same is true of the grain of analysis though a model representing firms as the atomic agent – the agent with no identifiable components – is clearly at a coarser grain of analysis than is a model of one of those firms in which departments or divisions of that firm are the atomic agents.

The finest grain of model indicated in the figure represents a single actor by a single agent.

The grain of the model becomes coarser when the number of agents is smaller than the number of actors they collectively represent. A yet coarser representation is where collections of actors are represented by a single agent.

The horizontal block arrows obviously represent the relationships between actors or collections of actors (iconified as the Linux penguins) on the one hand and, on the other hand, the more formal representations of those actors (iconified as the

Linux computer). Similarly, the vertical block arrows obviously indicate relationships among the representations of the actors at different grains of analysis. These relationships are important in the process of using models to capture the qualitative aspects of social systems. Relative to the computer science and social simulation literatures, the horizontal relationships correspond to model validation and the vertical arrows to model verification. These are considered in turn.

5.2 Validation: agents and actors

A model is validated by ensuring that it is a good representation of the real phenomena it is intended to capture. An appropriate definition of “good” in this context is that the agents in the model do not distort or misrepresent any aspect of the behaviour of corresponding real actors engaged in the activities being modelled.

At the finest grain, as represented in Figure 5, validation will entail comparing the simulated behaviour of an agent with the actually observed behaviour of individuals. A considerable amount of work in this area has been conducted by computational cognitive scientists. In several cases, whole programming architectures – specialised computer programming languages – have been developed to simulate the behaviour of the subjects of psychological experiments. Classic works in this field are the Soar programming architecture based on Newell (1990) and Anderson’s (1993) ACT-R. Validation at this grain of analysis has taken the form of comparisons of experimental and empirical results with outputs from models implemented in these architectures.

Validation at more coarse grained models has involved either the assessment of model outputs by domain experts or comparisons of model outputs with observation. The validation of the VDT model is a particularly spectacular example of the latter.² The pilot VDT model was developed to represent the R&D process leading to the production of a space launch vehicle. The model identified two systems that were most likely to fail as a result of organisational relationships and failures of communication. The prototype vehicle went off course minutes after launch and was destroyed by the range safety officer. The subsequent investigation identified failure of both systems identified by the VDT model as being at risk.

² This story was told at the 1998 CMOT workshop by Ray Levitt who headed up the VDT project at Stanford.

A natural approach to model validation would be to use data from ethnographic studies first in the design of the models and agents and then to compare the model outputs with actual episodes. Agents are always specified in CPM models to yield explanations for their simulated actions. These explanations can be compared with the protocols obtained by grounded theorists and ethnographers.

5.3 Verification: consistency among models at different grain

The purpose of constructing suites of models at different grain is to keep the number of agents small enough to enable the analyst to understand the relationships among them even when the number of actors and organisational units is very large. There is no advantage in developing models with tens or hundreds of thousands of agents because the modeller is then substituting a model that is too complex to understand for a reality that is too complex to understand. Indeed to construct such models in practice requires the specification of extremely simple agents without any indication of the distortions in behaviour resulting from the extreme simplification.

A better approach is compositional verification in which the behaviour of coarse grained models is required to be consistent with the behaviour of models at finer grains. This is achieved by ensuring that, for example, the behaviour of a whole firm as simulated in a model of competition among firms can be replicated by a finer grained model of the firm when the environmental characteristics generated at coarser grain are imposed on the firm model. Similarly, the results obtained in simulation experiments with the firm model can be imposed on a model of a component of the firm to ensure that the model yields the results observed at coarser grain. This decompositional procedure can be taken down to the finest grain model so that the kind of individual behaviour that contributes to the most highly aggregated outcomes can be identified and validated.

Any failure of either verification or validation should focus the attention of the analyst on elements of the social system that have not been understood or appropriately represented.

5.4 Validation, verification and structuration

Structuration (the classic reference is Giddens, 1984) is a process whereby individuals develop behavioural norms, beliefs, notions of responsibility, rights and duty to enable them to live and work together. There is no shortage of historical evidence about the generation of these structures to support and enable decisions to be

made in the face of the ineluctable fact that there is no equilibrium, our cognitive capacities are limited, we as individuals differ from one another and, as a result, we have very little idea what the future holds in store for us. These structures not only guide our actions but important elements of these structures are frequently imposed on us as socially necessary constraints. Most of us do not commit murder and have no wish to do so but, also, we are not permitted to commit murder.

Though structuration processes were identified as such some 15 years ago, the only dynamic analyses taking them into account have been social simulation models such as those of Carley and Svoboda (1996), Edmonds (1999, in press) and Moss (1998, Moss and Sent (1999), 1999). These models capture (at various degrees of abstraction) processes in which norms and beliefs, *etc.* develop. Well validated models, specified using qualitative social research techniques as developed by students of management, capture the micro level processes. The more macro effects of such structures are captured by the more coarse grained models. The verification process ensures that the representations of these structures at more macro levels are consistent with the lower level processes that create them. Moreover, the effects of these structures in both enabling and constraining action by individuals are likely to require analysis with both the more coarse grained and the more fine grained models.

6 Conclusion

The lessons to be drawn from the foregoing argument is that a model based social science, validated by qualitative social science will provide a sound foundation for a science of social policy. This foundation is naturally and closely related to the third way associated with structuration theory. The purpose of these models is to simulate social processes both to understand the processes themselves and to help to identify both opportunities and dangers associated with specific social policies. It should none the less be recognised that one property of the social structures that emerges from the process of structuration is that these structures enable individuals and institutions to act in the face of the fact that prediction is a wholly unreliable activity in a social context. We cannot, therefore, expect a model based social science to predict future events, only to help us understand events as they unfold and to inform our assessments of the likely consequences of our actions.

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