

PART D: SWANS AND DUCKLINGS OR LEOPARDS AND SPOTS?

11.1 *Crisis as a way of life*

Like many similar efforts which have preceded it, AS is convinced that despite all the contentiousness and disarray, it is possible to facilitate a unification of sociology. Ugly duckling it might be but beneath the feathers all stubby and brown, there is a swan waiting to emerge. We are far less sanguine. It seems to us that if sociology looks like a duck, quacks like a duck and walks like a duck, then it probably is a duck. Or, to change the metaphor, the problem with transforming sociology is in getting the leopard to change its spots. We don't know if sociology can or cannot become a science. What we do know is that it doesn't really seem to want to. Embedded in the culture of the discipline are traits which systematically subvert any such proposal for transformation into being a science.

A quarter of a century ago Richard Schweder and Donald Fiske (1986) thought that sociology was in an 'uneasy' state which might best be described as a crisis. The main reason was its failure to deliver the universal generalisations framed in the deductive nomological structures which were then thought to be the hallmark of scientific explanations. This failure was taken either as proof of the persistent immaturity of the discipline or a good reason to give up entirely on the idea that sociology might be a science; a conclusion many have concurred with and acted on. Today, once again the question of whether sociology is in crisis is being posed, though this time it is motivated less by epistemic worries than by the fashionable conviction that sociology is a critical discipline and, as such, it deserves a significant social and political role. Ironically, this view prevails at a time when sociology actually seems to be increasingly marginalised because of supposed threats from (a) its own dispersal into enclaves within adjacent and not so adjacent disciplines and (b) non-sociological organisations and institutions which are gathering of 'big data' on social life.

We mention Schweder and Fiske not because they used the image of crisis but to underscore a more important point. The questions which preoccupy AS are perennial ones and what AS represents is not the uncovering of a novel general problem in sociology's methodology but just the latest version of a well established *strand* in sociological thought, namely that the discipline should adopt the form of a mature (usually natural) science. For our part, like many others we think that while sociology has several deep problems, that it is not sufficiently like a science is *not* one of them. It would be absurd to deny we are often quite critical of contemporary sociology, but this is not because it falls short of being a proper science but because it lacks good arguments for the claims it makes about its findings and their implications.

We recognise though that AS and others have a different view and no doubt would want to debate with us. Indeed, one of the reasons we took an interest in what AS was proposing was because we recognise we might be wrong about the prospects for a scientific sociology and wanted to see if we were being proved wrong. As is apparent from our discussion, thus far, at least, we don't think so. AS was of interest for another reason. We are sympathetic to any form of sociological thinking that recommends a dose of simplification as a way of disentangling the Gordian knots that the discipline counts as its problems. However, we would expect the pay-off from this simplification to be quite modest and often deflationary. We believe it will be a great deal harder to make sociology into a science than some sociologists seem to think. In that respect, it is our conviction that AS is mostly engaged in misplaced effort. It is only setting out a plan for a scientific sociology, not producing it.

The one lesson from Thomas Kuhn that hardly anyone ever mentions is that mature sciences aren't developed from blueprints. The exemplars that become the focal point for an integrated complex of normal science just appear within a field and then displace the preoccupations which previously dominated. Further, simply because sociology resembles the early history of some now well established normal sciences doesn't mean it will automatically develop its own transformative exemplar. For that to happen, someone has got to do the work of delivering the exemplar. For all we know, this might well be taking place. However, past experience tells us it is far more usual for sociologists to tell us what needs to happen and then refrain doing it themselves. Their manifestos typically and misleadingly offer 'science: the easy way' while at the same time proving just how difficult it is to improve much, if at all, on the current situation.

When Schweder and Fiske were writing, by and large the generalisations sociology was trying to force into the deductive nomological format complied with Dibble's first law: namely, some do; some don't.³¹ Many manuals were produced offering templates for how to form deductive nomological structures but rather than setting sociology off on a new footing, all they tended to do was induce scepticism as to whether the theories so constructed did anything more than superficially mimic the required models, and even if they did do anything more, whether the inferences they supplied were either interesting or useful.

11.2 Never mind the quality, look at the volume!

The one insight we think can be taken from all the talk of crisis in sociology is the observation that the discipline's empirical research does not seem to be cumulative. There are multitudes of studies and projects but their results do not consolidate into a unified corpus of knowledge. The point of the remedies for theoretical reformulation such deductive nomological models or middle range theory, is to find ways of connecting research findings together through the medium of an integrating theory or model. Unfortunately,

³¹ in 1964 Berelson and Steiner produced an 'inventory of findings' from the behavioural sciences, extracting some 1045 'findings about human behaviour we thought had some decent claim to substantiation', presumably only a small number relative to an already voluminous collection of studies. Berelson thought that the 1045 could be reduced to "a 3-proposition grand summary: (1) some do, some don't; (2) the differences aren't very great; and (3) it's more complicated than that".

deductive nomological modeling yielded neither generalizations that could be put to use nor the unification of research. And it looks like AS will share the same fate.

However, these failures didn't extinguish the wish for generalisation and unification. AS (inter alia) offers its own way of revitalising the aspiration by dispensing with what it sees as the misguided idea that scientific laws are universal generalisations. Instead, it insists respected science uses mechanisms which, in their 'sometimes true' character more closely resemble the kinds of generalisations that sociology commonly comes up with.³² A not-unsympathetic Amazon customer complained in his review of the *Handbook of Analytical Sociology* that the statement of AS' core principles is a statement of methodological principles only. Explanatory principles are notably absent. It seems the problem that AS faces is the same one that confronted the enthusiasts for deductive nomological models – providing a rationale for the adoption of the form is the easy part. The hard questions bear on what is going to be put into the format. In the end, sociology's reformers often turn out to be precisely that, re-formers; their main effort being the recasting of existing sociological materials – ideas and studies - within the form they prefer while at the same time leaving it hard to determine what added value is actually gained. The logic is one that says since existing sociological materials *can* be recast, they should be rather than demonstrating that there are sociological findings and other materials which are crying out to be analysed within that form, ideas which *just can't* be set out in any other way. A number of times, we have mentioned the way in which AS recurrently appeals to a small number of precedents such as Schelling's models. Such analytical lash-ups serve more as proof of concept demonstration of the enhanced explanatory power of the AS approach. Just as is the case with many of sociology's other supposed 'paradigms', these precedents are less Kuhnian exemplars than interesting one-offs.

Like everyone else, AS wants to improve sociology's explanatory power, but, again like everyone else, it treats explanation as more a matter of applying an explanatory form rather than making intelligible something that has previously defied understanding. Sociology doesn't really have problems that defy understanding – it is as prolific as it is because it doesn't run up against phenomena which are just so hard to figure out such that they require the adoption of some new kind of explanatory machinery. AS, after all, makes its fundamental explanatory principle the BDO scheme which is pretty close to a default mode for everyday explanations of people's actions, namely that people respond to situations on the basis of their reasons. To take another example, Arthur Stinchcombe is a prominent point of reference for AS and has been a strong advocate of more stringent forms of theory and yet when he tries to explain why there is always resistance to organisational innovation, his argument is that innovations unsettle existing organisations by threatening some of the things that people in them value such as their status symbols (one obvious example being reduction in the amount and kind of office and work space that is theirs).³³ Stinchcombe's analysis is reasonable and readable and makes some interesting points as it ranges over different aspects of the effect of and problems in changing established relations, but the only unifying explanatory principle given is the banal

³² Dribble's law is still the model, though!

³³ See Stinchcombe (1986)

idea that "resistance to administrative innovations is mostly due to (more or less) rational anticipation that concrete interests (career prospects, risks of failure, chance for exceptional success, etc.) are endangered " (Op Cit p. 221). The unifying theme of the discussion is the setting out of possible variations on this central motif with the main explanatory work consisting of how instances from organisational life can be recognised as instances of self-protecting resistance. Interesting connections are made and interesting examples given that certainly bring to mind aspects of organisational change one wouldn't have thought of, but these are ones which fit readily with, and to an extent derive from, what we already understand about how organisations work, and certainly fit comfortably within a wide range of existing forms of sociological explanation as well as being heavily dependent on common sense familiarity with organisational life.

Jon Elster is someone else that AS treats as a fellow traveller. Elster, however, tends to arrive at what, for him at any rate, are disappointing conclusions about the prospects of developing effective general theory in sociology. He explicitly admits that his own attempt to understand social solidarity and to generate a general theory of social movements falls short of his ambitions and consequently suggests that his own unsuccessful efforts are as good as can be expected. In this, Elster rather like those who, treating their own theorising efforts with which they have now become disillusioned as if they were somewhat more than the sketchy and simplistic accounts they really are, conclude that society is too complex and motile ever to be captured in a general theory (or 'Grand narrative'). Such self-confessed failure serves less to determine the limits of what sociology might achieve than as an illustration of the widespread tendency of sociologists to bite off vastly more than they can chew (to repeat James Davis' complaint about the general preference for problems that sociologists are in no position to solve that we cited in Part A). Just to be clear, though, we are not suggesting a counsel of hope for a grand narrative, only reservations about any presumption that the production of general theories is both quick and easy.

11.3 *Silk purses and sow's ears.*

It has been suggested that one reason for the relative weakness of sociology's findings and conclusions compared to those of the sciences is the technologically impoverished nature of the instruments that it uses (Turner and Kim 1999) though, of course, sociology doesn't really use 'instruments' in that sense at all. When sociologists talk of 'research instruments', they usually have in mind some type of social survey which, after all, is just a way of asking people questions. When they do use instruments based upon genuine technologies, these tend to be domesticated versions of the familiar audio and video recorder (even so, in our experience lots of sociologists can be unduly impressed with the results so gleaned). This all leads us to think that even if they were able to deploy the most sophisticated of instruments and technologies, the quality of the results generated would be pretty much the same as now.

It is for these reasons that we suggest the use of computing tools in sociological analysis will not make significant, indeed any, difference. Of course, the computer is still recent enough to count as an impressive technology, and the capacity of computer systems to capture and process 'big data' is currently stimulating great expectations (as well as acute anxieties that sociology might lose out relative to business and public

bureaucracies that have the resources to acquire vast quantities of social data and powerful means to analyse them. The computer is also seen as opening up new ways of doing scientific research through the use of simulations rather than experiments and studies. Clearly, ABM, for example, provides a (potentially) powerful way of calculating the cascading and compounding consequences of elementary recursive operations, which fits well with AS' idea of what sociology needs to do, namely to track the accumulating consequences of the causal effects of individual actions. However, the usual problem remains. Powerful scientific tools come with their own requirements for proper and effective use, and it is here sociological repurposing of these technologies runs into trouble. The rationale offered is that these sophisticated devices will force sociologists to rethink and raise their game so as to satisfy the instruments' requirements. However, the usual response is precisely the reverse. The requirements of the technology are relaxed in order to accommodate the sociology being processed. The two examples of ABM modelling which we examined indicate just how this happens. Both aim to connect with sociology through implementation of its theories, and both choose what are complex and, like most sociological theories, vague, if not obscure, about how they might yield specifications of the determinate causal chains connecting the phenomena they address. In both cases what is implemented is a version of the theory which is determined more by the ease with which it can be represented in the simulation than it is by the necessities of capturing the theory it is purporting to explore. Salgado et al's 'chatty game', for example, serves to do little more than remind us of the insightfulness of Wittgenstein's remark about how problem and methods often pass one another by.

11.4 *The virtue of aspirational realism*

The maxim 'cut your coat to fit your cloth' is unlikely ever to be adopted as sociology's motto.³⁴ Failures of unrealistically ambitious efforts don't have to be projected on any very great scale to show how difficult it is to shape sociology's conceptual and theoretical questions sufficiently sharply to allow telling and definitive answers. But rather than causing us to reflect on why we have fallen so short, the failures are taken as demonstrating the complexity of both problems and phenomena and as a consequence act as a stimulus to pose yet more questions we can only answer in the loosest possible way.³⁵

The difficulty of providing definitive answers involves more than simply making the questions more complicated. There are also deep problems concerning our ability to ensure that the data we can collect captures (or maps onto) the phenomena we are interested in exploring. As we have just suggested, sociology has a limited repertoire of investigative methods and tools. Very often, these cannot capture the phenomena we are interested in which would, in any case, require very complex evidence to be documented. The fact is that the tools – which, as Turner and Kim noted, are mainly variants on talking to people – are used despite their known inadequacies. The persistent slippage between the intended and actual objects of study (just one of the many well known instances of slippage throughout sociology!) is usually taken as something to be lived with unquestioningly. Earlier we recounted how James Davis complained that it is rare for any two

³⁴ We think Conversation Analysis is a rare exception to this.

³⁵ We'd be apt to say, adding new questions to the huge number that are already being begged.

sociologists study the same thing, which is doubtless one reason why replication is rare, but the form of much sociological research also serves to make replication impractical. Sociological research involves collecting raw materials, processing that raw material for publication as data, and then making claims made about what the data evidences and its relation to the problem originally addressed by the inquiry. Though 'data collection' occupies a disproportionately large place in sociology's methods manuals and reflections, as well as of the distribution of effort within research projects themselves, there is no reason to suppose that, in practice, data collection is a highly disciplined procedure. As Harold Garfinkel (1967) demonstrated, it is usually impossible to recover either the precise procedures used to assemble the raw materials or the procedures used to transform them into publishable data. The procedures themselves are not normally captured, and are likely to be almost as little known to the data gatherers as they are to their audience. Garfinkel also noted that the data presented in sociology is often such as to accommodate different, even opposed, conclusions regarding what has been done, demonstrated and achieved (especially when more than one of the varied perspectives in sociology are applied to the same data).

If there is much room for – indeed, relatively free - interpretation in the analysis of data, then there are reasons for thinking that there is also ample space for equally free interpretation in both the collection of raw materials and in their conversion into publishable data. On those relatively rare occasions when there are opportunities to compare raw materials and data, the relationships between the two can seem highly problematic and available at least to differences of interpretation³⁶. It is not a matter of saying that generally the presented data in sociological studies is seriously at odds with its raw materials. It is simply to say that for the vast majority of sociological studies there is no way of telling from what is published whether it is or is not. Again, this is well known but seen as just another condition to be lived with and, for most disciplinary purposes, disregarded. Problems in sociological research methods are never solved generically and once for all so that subsequent researchers will not have to contend with them. They are repeatedly addressed and *resolved* in the context of the demands and difficulties of an individual project and fixed in ways which meet the needs of that project.

We recognise we are treading a fine line here. While we seem to be agreeing with the complaints of those who want to the discipline more scientific and have enumerated some of the very conditions they wish to reform, we do not repeat them in order to criticise but to indicate how unrealistic the reformers are about the situation they confront and the extent to which what are, for them, regrettable states of affairs are, for a great many of their professional colleagues, unremarkably normal and entirely acceptable features of their practice. There is no point in proclaiming that because they don't prioritise making close and systematic connection with other research studies, sociologists are derelict in their scientific duty if they don't see such demonstration as needed for their purposes or required to evidence their achievements. Indeed, they mostly don't recognise, at least in practice, the demands that a 'more scientific' orientation would impose. Rather than regretting the complexity, obscurity and verbosity of many central sociological contributions, many

³⁶ In *Reinventing Evidence in Social Inquiry*, Richard Biernacki (2012) aims to make a general point about the untrustworthiness of coding practices applied to cultural phenomena, and in doing so provides a couple of striking and very detailed examples of these problems

sociologists welcome these characteristics, often being able to discern elements of profundity within (one or some of) them, and happy to be challenged to explicate what their favoured sociologies are saying. As Alfred Louch pointed out long ago³⁷, if, in the mature natural sciences, theory is used to throw light on researches, then in sociology an important function of researches is to make intelligible what the theory is saying.

It is a simple fact that because of the uncertain and often indeterminate relationship between what sociologists say and what they purport to talk about, sociological conceptions are not transitive. The fact that one understands and accepts what (selected) sociologists say does not entail that one knows what the social world is actually like. Peter de Grace and Leslie Stahl (1990) in writing about software development, argued that programming skills are narrow but deep. In contrast, you might say that sociological skills are prevalently wide but shallow.³⁸ In terms of Schutz's (1976) typology of the distribution of knowledge, whilst many sociologists like to think of themselves as 'experts'³⁹ it seems to us that they are much closer to the 'well-informed citizen'⁴⁰, though in Schutz's typification, the well-informed citizen does not aspire to expert knowledge whereas, of course, at least some sociologists do. The resemblance we find is in respect of what comprises commonly acceptable standards of knowledge and evidence. This is intensified when we consider how far sociological interests parallel the same order of socio-political concerns as engage what we normally think of as a well-informed citizen.

³⁷ 1962, to be exact. See *Explanation and Human Action*.

³⁸ For example, Jonathan H Turner lists the following 'areas of specialization':

History of Sociological Theory
 Philosophy of Science
 Contemporary Sociological Theory
 Social Change
 Theory Construction
 Roles and Interaction
 Comparative Institutions
 Social Psychology
 Social Stratification and Inequality
 Historical Sociology
 Race and Ethnic Relations
 Human Social Ecology
 Human and Societal Evolution
 Sociology of Emotions
 Bio-sociology
 Modeling of Social Processes

³⁹ Defined by Schutz (op cit) as: 'the expert's knowledge is restricted to a limited field but therein it is clear and distinct. His opinions are based upon warranted assertions; his judgments are not mere guesswork or loose suppositions.'

⁴⁰ Concisely identified by Schutz (op cit) thus: "On the one hand, he neither is, nor aims at being, possessed of expert knowledge; on the other, he does not acquiesce in the fundamental vagueness of a mere recipe knowledge or in the irrationality of his unclarified passions and sentiments. To be well informed means to him to arrive at reasonably founded opinions in fields which as he knows are at least mediately of concern to him although not bearing upon his purpose at hand."

In the end, the conclusion we come to is this. Given the apparently irredeemable tension that exists between, on the one hand, the demands that the rigorous implementation of any serious model of scientific practice would make and, on the other, the desire on the part of sociologists to continue to talk about the topics they want to study and discuss in the ways they always have, it seems to us highly likely that any programme which is designed to impose unity through conformity to a strict model of science will be condemned to failure. This is a situation which will not be remedied by waiting for the sociological ugly duckling to emerge as a fully fledged swan but only when and if our professional colleagues decide the disciplinary leopard should change its cultural and organisational spots.