

OPINION PIECE

From Grounded Theory to the Practice of Theory-Building

Carl May

Newcastle University



One of the reasons that I have always been a little suspicious of 'Grounded Theory' studies is that on closer examination they often seem not to develop or offer theory. Sometimes they seem not to be terribly well grounded, either. But lately I've been thinking about this problem more sympathetically. I've looked again at how Barney Glaser and Anselm Strauss were able to draw from their ethnographic studies of medicine and health care a set of substantive theoretical constructs about *trajectories*. Having done this, they worked them into a formal 'theory of status passage' (Glaser and Strauss, 1971). After their unfortunate personal disagreements of the late 1980s revealed the differences in the ways that they had understood their joint methodological project, Strauss continued in this vein. He made a germinal contribution to what has become 'social worlds theory' (Strauss, 1993). These are major theoretical contributions: the theory of status passage offers a middle-range theory of interactional identity that we ought to take more notice of, and social worlds theory is a substantial contribution to social theory.

However, there are all kinds of reasons why I am still not completely certain about 'Grounded Theory'. After all, many Grounded Theory studies don't actually theorise. Instead they offer a set of empirical generalisations. These are regularities in the data that call for explanation, but they are not theoretical explanations in themselves. In this context, scale matters. Empirical generalisations may be all that it is possible to achieve, inductively, in the kinds of small scale qualitative study that are most commonly performed and published by sociologists in the UK. But Grounded Theory did not come about through such studies. To build theories, Glaser, Strauss and their colleagues drew on – and wove together – comparative analyses on a large scale. *Status Passage*, for example, draws on three large studies in which Strauss was a senior investigator - *Boys in White* (Becker et al., 1961), *Psychiatric Ideologies and Institutions* (Strauss et al., 1964), and *Awareness of Dying* (Glaser and Strauss, 1965) - to develop a theory of interpersonal trajectories. These were large studies, consisting of many field researchers conducting hundreds of interviews and observations in a very intensive process of data gathering and interpretation-in-action. Individually and together, these studies made major theoretical contributions to the development of medical sociology as a field. They did so by means of cumulative and comparative theoretically informed analysis.

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Although for some students and researchers Glaser and Strauss's *The Discovery of Grounded*

Theory (1967) may now primarily be of historical interest, the processes of theory-building that this history reveals are still important. One of the interesting features of sociological research around health and illness over the past decade has been the growing significance of its interactions and relationships with science and technology studies, or STS. A number of commentators - of whom Caspar Jensen (2008) is the most recent - have argued that an explicit shift to the theories and methodological perspectives of STS is necessary to secure the intellectual future of 'medical' sociology. I don't wish to argue about this here, although it is notable that STS writers have made considerable efforts to establish the wider relevance of their perspectives across a range of social science fields. These efforts have been at least partially successful. My research has certainly been influenced by STS although - it seems - not enough for Jensen.

In some ways, STS seems to have a good fit with the ways that sociologists of health and illness construe their own field of research, at least in the UK. It is constructionist in its theoretical approach; privileges contingent and relational aspects of social life; focuses on specific incidents or cases of general problems; and is largely conducted through the application of qualitative research techniques - often ethnographies, but increasingly interview-based research. Importantly, as John Law (2008) has argued in a recent position paper, there is a strong argument within the communities of practice that make up STS that it ought to concern itself primarily with case studies, description and classification, and that it should neither seek causal explanations nor attempt quantitative generalisation.

Law's position, that causality and generalisation are outside of the ambit of the STS researcher, has some congruence with the conduct of theory and research in the sociology of health and illness. After all, we collectively acknowledge that causal mechanisms are hard to disentangle, and that confounding variables are everywhere. But it seems to me that this is also a counsel of despair, and a recipe for empirical fragmentation and loss of analytic power. Small scale, qualitative case studies of contingencies and relational processes proliferate and, as they do, empirical generalisations - peculiar objects that are neither facts nor theories but something in between - also multiply in number.

If empirical fragmentation of the kind I have described above is a problem, is there a solution? It may be that it is not seen to be a problem, for such studies continue to proliferate rapidly in both medical sociology and STS. Although they are often case studies on a small scale they generally seem to aim to produce new knowledge about observed or hypothesised phenomena. There's no doubt that they often succeed, too. The question is how to bring together the results of large numbers of small scale case studies and their resulting empirical generalisations.

One way forward is methodological, and involves secondary research that seeks to synthesise the results of prior work in a field with a view to synthesising the results of studies of processes and outcomes. The problem here is not the formulation and systematisation of 'evidence' through systematic reviews and meta-analyses or meta-syntheses. The requirement is rather more serious than that. It relates to how these studies (and their authors) learn from each other, and how this interdependence of research and researchers accumulates knowledge and understanding. The problem with systematic reviews and meta-analyses is - as Tiago Moreira (2007) has observed - that the explanatory intentions of the authors of individual studies are hidden as syntheses focus on the results of studies rather than the theoretical assumptions and interpretative work that formed them.

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If secondary analyses will not do the job of joining up multiple fragmented studies and making sense of their many empirical generalisations, what will? I want to return now to Glaser and Strauss's contributions to theory and method and to argue the case for theory-building work. Theoretical development in sociology tends to be seen, John Goldthorpe (2006) reminds us, as a highly specialised activity that is bracketed off from empirical research. It ought not to be. The

lesson of Glaser and Strauss is that theoretical development and empirical research are intimately linked.

I am interested in the links between research that identifies empirical generalisations (regularities that warrant explanation) and research that builds explanations, because it is both practically useful and intellectually rewarding. Some of my recent work, has been about building a theory of the implementation, embedding, and integration of material practices - Normalisation Process Theory. This is described in two recent papers (May and Finch, 2009; May, et al., 2009). This isn't the place to discuss theory-building methods in detail. Instead, I want to look briefly at the constituent parts of theories and the work that they can do for us. We can begin by thinking of theory not as a large scale and abstract body of ideas, but rather as a toolkit that enables us to do three kinds of conceptual work - describing, explaining, and forecasting some social phenomenon. In one of the papers that laid out the basis of Normalisation Process Theory, its co-authors had first to agree what a theory was supposed to do (May et al., 2007). We saw theory as *explanation*, formed through a set of tools that met four tasks:

1. *Accurate description*. A theory must provide a set of definitions that enable the identification, differentiation, and codification of the qualities and properties of cases and classes of phenomena.
2. *Systematic explanation*. A theory must provide an explanation of the form and significance of the mechanisms and processes at work in cases or classes of the phenomena, and should propose their relation to other phenomena.
3. *Knowledge claims*. A theory must lead to knowledge claims. These may take the form of analytic propositions, or experimental hypotheses. They may also map relations with other phenomena that are believed to possess similar qualities and properties.
4. *Investigation*. A theory must be testable. Such tests may be abstract (i.e. formal logical representations, simulations, or thought experiments); or concrete (empirical investigations).

This is a generic model of a theory that also alludes to the work that is necessary to develop it. This is exacting work, but is also very interesting and sometimes quite exciting. It begins with exactly what Law (2008) and other proponents of STS ask for when they claim that the job of sociology is to describe and classify social phenomena. This is a foundation, not an end in itself. The foundation can be laid in a number of ways: by meta-synthetic studies that concentrate on collecting the empirical generalisations and low-level theoretical interpretations from large numbers of existing studies; or by collaborative data-clinics in which researchers talk through the results of their work with their peers and identify core objects of their collective interests. It thus decides the scope of a theory. What follows from this must be the production of an explanatory account of *why* those objects take the form that they do and how they work. This is more than description. Lieberson and Lynn (2002) are amongst a number of recent writers about the nature of sociological theory who have argued that we should be interested in the generative principles or mechanisms through which social processes are formed and which interact with other factors to lead to different outcomes. From this stems the possibility of drawing together many different studies, identifying their core components and explaining these in terms of mechanisms. It also raises the possibility of comparative modelling the generic features of mechanisms and the processes and other effects that are derived from them. This approach to theory is about developing explanations for *action*, rather than exploring networks or interrogating discourse.

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I started this brief article by pointing to my uncertainties about Grounded Theory, and ended it by noting that when I came to theory-building in practice, I ended up doing something rather similar

to it. I have also observed that the fields of sociology of health and illness and science and technology studies have some important overlaps in terms of the ways that many practitioners seem to conceive of the topics of their research, and that these overlapping topics are marked by similar problems in the organisation and outcomes of empirical research. I have pressed the case first for secondary analysis and then for theory-building as ways of working through the implications of these problems. The way is open for groups of researchers to find ways to bring the data and results of their collections of studies into interesting collaborations that explain social mechanisms and processes in new ways.

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Carl May is Professor of Medical Sociology, and leads the Health Technologies and Human Relations Research Group, in the Institute of Health and Society, Newcastle University, Newcastle upon Tyne, UK

c.r.may@ncl.ac.uk

<http://newcastle.academia.edu/CarlMay>