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Science as a theoretical practice: a response to Gorard from a sceptical cleric

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Abstract

Gorard argues that 'theory' is a principal barrier to the adoption of mixed-methods in social and educational research. His critique of 'theory', however, raises questions that may require some further attention. In this paper I respond to Gorard's argument, informed by scientific realism and broadly committed to a 'numbers and narratives' approach to educational research. At the same time, however, I am critical of what appear to be non-realist elements embedded in Gorard's own theoretical position. Here, I attempt to shift the focus from theory to explanation in the belief that this may clarify what is at stake in the symbolic struggle over method.

Introduction

It is an odd experience to agree with so much of a paper and yet to disagree so profoundly with its fundamental thesis. But such is the position I find myself in, and it is a state of mind that can only be settled, by one with a theoretical and clerical disposition, through the labour of conceptual analysis and clarification. To hope that the results of this process will prove of interest to anyone else is one of those minor professional egotisms without which all our thoughts would remain private and uncirculated. My argument here is that the incorporation of mixed methods in social and educational research is hindered, more so than for any other reason, by the insistence of qualitative researchers that their methods cannot be utilised within the context of what they regard as 'positivist' research.

This thesis merits a response. As Gorard (2004) has enlisted my work on his side, and quite generously so in the context, I may have a plausible entitlement – even a right – to offer such a response. Gorard presents his case as a defence of the Enlightenment against a new form of clerical reaction. This is an interesting conceit, and I will enter the spirit of the game as a *sceptical cleric* (if such I must be). In truth, however, the scientific realism I uphold is one of the definitive contributions of the Enlightenment.

Gorard and I have a lot in common. I have advocated an interdisciplinary, multi-method 'numbers and narratives' approach to social research, and specifically to educational research, within the strong framework of scientific realism. Gorard's position does not differ that much from mine: his 'largely realist' ontology, 'somewhat' relativist epistemology and basically pragmatist methodology seems a little eclectic, but has its feet on the ground. From Gorard's own account of his commitments in this respect, it is clear that *theory* cannot be eliminated from the practice of research. Our views on the training of researchers are likely to be similar: I too would discourage the formation of definitive methodological identities; promote interdisciplinary research (which should ensure plurality of techniques); and reward informed and accurate report writing. There are divergences in our positions, and they will become clearer, but there should be enough agreement between us to sustain a dialogue.

We can probably fundamentally agree that the defining activity of science is the investigation of events, processes and states of affairs in order to explain how they happen, operate, or come to be. But I believe that the second part of this statement, which is often neglected, requires as much attention as the first. Science is about getting to the bottom of things so that we have an explanation of how they work. For that reason, I prefer to talk about explanation rather than theory, and about how one can decide whether explanations are correct (or more or less correct) rather than whether theories are true or false. Above all, I think that explanation *is* a theoretical activity, science is a theoretical activity, and that the separation of science and theory can only lead to confusion.

The consequences of this difference in our positions are evident regarding the nature of research. The production of information about the character of society is effected by a system whose work contributes to that end. While this system includes public and private institutions, universities, research centres and so on, it is certain that most large-scale social research is supported by public funds. Gorard's argument seems to be premised on the assumption that those who allocate funds, generally through the award of research contracts, are entitled to decide what is done with them and to evaluate the results in their own terms. At this level, the whole debate seems to be about who decides what is worth knowing and how much it is worth paying to obtain it.

The examples of research Gorard provides indicate the kind of applied research activity these institutional conditions are likely to generate. There can be no objection to the suggestion that studies of 'cost/benefits of single-sex teaching, decreased class sizes, homework, group work, rote learning' (Gorard 2004, p 2), and so on, are practical matters of some importance, but they do not exhaust the category of worthwhile knowledge that might be obtained about the educational system. A recent newspaper article reports that, '[r]ather than hoodies and track pants, schools were seeking a more formal look to build pride in the school and pupils', and quotes a primary school principal saying: 'We wanted something different. We wanted to be not run of the mill' (Uniform delight 2004). There are some fascinating assumptions behind all this, and the questions that arise might be recognised as worth serious attention. The question of whether children who have to wear a school uniform do better than those who do not belongs in Gorard's grouping. However, there are questions about school uniform, perhaps just as interesting to the general public, that do not fit as well into his conception of practical research. Let us consider some of these questions.

How many schools, by location and type, have a uniform policy?

When were school uniforms introduced?

Why were school uniforms introduced?

Why has school x decided to introduce a school uniform?

Is the number of schools with a uniform policy increasing?

These are, of course, different questions. They may all be of interest, even practical interest, to sections of the public whose taxes support social research, although they may not seem of equal urgency and immediate relevance to the special interests of those responsible for distributing research funds. Some of the questions listed above require quantified methods and some of them do not. To discover whether the number of schools with or without a uniform now and at some point in the past has changed will obviously require some counting. The question of why school uniforms became common, however, does not call for any quantification, and might even be answered in terms of the archaeology of knowledge.

The concept of theory

Gorard of course understands that 'theory' now has such a wide reference, it can denote a 'loose collection of concepts' or a narrative that explains how some event, process or state of affairs occurs, operates or exists and; moreover, perhaps in such a way that it can be tested by empirical evidence. In Gorard's view (and in mine), the over-extended use of 'theory' is an unfortunate development. Gorard's response to this situation, however, is ambivalent. On one hand, he asserts the normative position that, '[t]heories lead via logical deduction to hypotheses, via operationalism to observation, via scaling and measurement to empirical generalisation, and so to further theories' (p 14). But on the other, he must grant the status of theory to paradigms, perspectives, frameworks, stances, tools etc in order to reach the conclusion that 'theory' of this kind is useless in the conduct of empirical research. The problem here is that to engage in a polemic directed at *theory* (as a barrier) rather than at *untestable* theories or *pseudo* theories, which are actually concepts with no explanatory content, Gorard runs the risk of shutting down legitimate and essential forms of enquiry.

The options are clear: we can either, (i) insist that 'theory' refers to an account of the origins and operations by which some event, process or state of affairs has happened or come into being, and regard as scientific theories only theories of this kind that can be subjected to empirical test; or (ii) we can accept that such a normative concept of theory cannot be imposed – that the attempt to displace the use of 'theory' in the wide sense is unlikely to succeed – and content ourselves with specifying the reference it has in our own work. However, without risk of confusion, we cannot hold the normative view and at the same time argue that *theory,* as if there were a common referent, is of limited practical use.

Any attempt to impose normative concepts of science and theory would meet with failure. The reality is that we have these broad concepts, within which different positions, perhaps of scientific and non-scientific theories, might be delineated, but the boundaries between them will always be contested. Gorard's own normative concept of scientific theory, for example, will bear examination. We are assured that, '[a] theory is a tentative explanation, used for as long as it explains or predicts real-world events, not as an end in itself' (p 14), and this is presumably an illustration of his professed 'somewhat relativist' epistemology; but is this notion really satisfactory? Is Darwin's theory of evolution, for example, a *tentative* explanation of the evolution of species? Is Hartley's theory of blood circulation a useful explanation so long as it predicts – what? That the blood *does* circulate? There is something unsatisfactory about this (it stems from a residual positivism which rejects *demonstration* as evidence of actuality), and it is not entirely clear that Gorard does, in fact, have a coherent concept of theory at all.

As science is a theoretical practice, it is simply inevitable that we are stuck with theory: good and bad. It is clear that Gorard understands this, but when he finally accepts the all-embracing definition he protests against, '[a]t its most abstract level, theory is about how and what we can know about the social world of our research' (p 15), he makes 'theory' a synonym for epistemology and ontology – probably the most demanding and technical areas of philosophical enquiry. If Gorard wants this, then so be it, but he can hardly then be in a position to complain about the 'looseness' of concepts in the works of black-listed 'great thinkers'.

Shifting the focus to explanation

The purpose of a scientific theory, in the more or less precise sense, is to provide explanations of the processes and events it is intended to cover (Kim 1983). In short, an explanation is an account of how something works involving a mechanism of operation. The most powerful explanations have a hypothetico-deductive form, in which a covering law carries the explanatory weight. In Bunge's (1998, p 4) account, 'a rational explanation of a fact involves the *subsumption* of the *explanandum* under one or more *generalizations* via information concerning the *circumstances* accompanying the fact that is to be explained'. A broad view of theoretical explanation is accepted by scientific realism. There are, however, difficulties with the positivist account that must be described.

The positivist scheme of explanation supposes that every successful explanation is derived from an empirically supported general statement of the kind, following Popper (1983): ‘you can’t carry water in a sieve’, ‘you can’t have inflation without political protest’, and so on. Thus, the question, ‘why does this utensil leak?’ can be given an explanation based on a suitable covering law, such as ‘water flows through holes’ (a reliable fact established by observation but, in Popper’s view, entails no reference to the essential properties of water or holes); proceeding to ‘this utensil is full of holes’; and so to the conclusion: ‘therefore it leaks’. In the same way, the answer to the question ‘why is there political protest in Coronia?’ should take the form: ‘inflation [tends to] generate political protest; the inflation rate in Coronia is 50% per annum; therefore, people have taken to the streets’. To give an explanation, in this view, is to provide an account of how something has come to be the case. To understand an explanation is to possess that information in such a way that it can be put to some purpose.

Is there anything different about understanding an explanation in the physical sciences, such as an account of geothermal activity and its distribution; and an explanation in the social sciences, such as the relation between inflation and political action? The positivist scheme maintains that explanations presented in the form of a hypothetico-deductive model can be given in the physical and social sciences alike, and can be understood in those terms. For example, in an explanatory model, the inclusion of a law that water will expand, as steam, under specified conditions of temperature and pressure, will allow anyone capable of following a logical syllogism to understand the given argument. In the same way, the inclusion of a law that economically oppressed groups will generate dispositions that lead to protest action can similarly be understood within its framework. It will be noted that this form of explanation is in its logical form, equally applicable to physical and social events.

As far as most social scientists are concerned, the problem with this form of explanation lies in the difficulty of discovering reliable social laws. It is true that regularities of social behaviour are easily observed. For instance, the sociology of education attempts to explain that children from the upper 10th of the family income distribution are 8–12 times more likely to enter higher education than those from the lowest 10th. And this is a ‘law’, incidentally, that might have a place in an explanation of certain events.

For example, one might take this regularity into account when comparing the examination performance of schools with different social class composition; but the fundamental task, of course, is to explain that regularity by discovering its causes (Humphreys 1984). In fact, an explanation gains power from the availability of reliable covering laws, not its hypothetico-deductive *form*. It is likely that most social theorists do not believe that laws about society, at least of a non-trivial kind, can be obtained. Many insist, furthermore, that human activity cannot be explained without human understanding in a special sense.

In a ‘very short introduction’, Bruce (1999, p 12) presents this argument for the difference between social and physical research in plain words:

> … what counts as explanation in the social sciences is quite unlike explanation in physics or chemistry. We explain why the kettle boils by citing the general laws of pressure, temperature, and volatility. Because the water has not decided to boil (a decision that it could change on some other occasion), we do not need to refer to the consciousness of water.

This position may rest, however, on a misplaced relationship between the distinct terms ‘explanation’ and ‘understanding’. An explanation, it is true, must be comprehensible – there is no point in trying to explain the special theory of relativity to a three-year-old – but that does not make understanding necessary to explanation in the sense proposed. The argument must be, for example, that political action, as for those who have seen their life savings disappear virtually overnight, can only be understood by observers able to empathise with the sentiments of those who find themselves in this situation. Anyone who finds such empathy difficult might be thought lacking in some basic human quality. However, understanding scientific explanations with a positivist form does not require *that* quality, but a willingness to accept well-founded empirical laws of behaviour and the ability to follow a logical syllogism.

It might be more successfully argued that understanding of a particular kind is one of the crucial *purposes* of explanations in the human sciences. The difference lies in this: as maintained by realists, the empirically based generalisations of physical science, the laws of nature, are known by acts of demonstration; and there is a strong case for the view that the demonstration of certain human dispositional states is achieved through empathy. Charlesworth’s (2000) account of working-class life in Rotherham, which I consider in more detail below, provides a convenient point for discussion. It is described on the cover as ‘moving’, and many readers will surely find it so, but if the positivist account of explanation is correct, why should Cambridge University Press think this quality worth noting? No one expects a book on geothermal activity to create any emotional response in its readers, and certainly not one that will enable them to understand the behaviour of geysers and hot springs any better. This is of course why the humanities curriculum is based on the interpretation of texts that deal with the activities of human beings in their real complexity.

One reason for teaching *Othello* is to encourage students to become more *humane*, in this sense, by reflecting on the experience and expression of jealousy. Although such teaching is often corrupted by ‘official interpretations’ to be learned from course notes, there is no suggestion that *this* understanding can be achieved through a process of logical deductions from premises about mislaid handkerchiefs. It is one thing to construct arguments in a formal manner, leaving conclusions to be deduced from premises, but that does not necessarily generate understanding, which is a state individuals may or may not attain. Teachers are well aware that the presentation of a logical form of argument does not guarantee that it will be understood. When all is said and done, we *understand* the relationship between class experience of a particular kind – the dispositions it generates, and the practices they produce – through a process of *human empathy*, and not by deduction from a set of formal propositions.

Realism explicitly rejects the doctrine that ties explanation to prediction, and the commitment to conceptual nominalism, operationalism and Humean causality that continues to inform quantitative research in the positivist tradition. There are encouraging signs that the use of quantitative methods within a non-positivist framework is gaining ground (Byrne 1998). A realist social science does not entertain any prejudice against quantification; it endorses moderate empiricism, accepts the correspondence theory of truth (in some version), and argues for methodological rigour in the search for effective social mechanisms. Scientific realism is also committed to the belief that social events and processes can be observed, and their several causes determined by, controlled observations and experiments. It also recognises that the analysis of causal relationships in social processes is far more complex than our models can usually reflect (Emmet 1984). There can be no support in a realist science for the attempt to win greater privilege for data obtained by one method of data collection rather than another.

The structure-disposition-practice scheme is so important here. An explanation should be constructed in terms of system properties, individual dispositions and individual action within recognised social practices in such a way that the effective linkages between these levels are open to demonstration. It is clearly necessary, therefore, to specify the relationships between structures (of positions), individual dispositions and adopted practices in appropriate concepts, and also to develop adequate techniques of quantification. The causal relationships, for example, between family class location, parental desire to stimulate children's intellectual development, and the adoption of a specific social practice, must all be demonstrated by practical observation and theoretical argument of a kind appropriate to the events and processes studied. A complete explanation will include an account of the social mechanism by which cause and effect are linked.

The basic form of sociological explanation

The paradigm sociological explanation has a structure-disposition-practice form: what are the implications of this for social research? Consider two accounts of why many working-class students leave school at the earliest possible opportunity. The first theory argues that adverse environmental circumstances (such as low income, over-crowding, inadequate public amenities), generate demoralisation which lead students to abandon their schooling (Schools Council 1970). The second theory argues that structural relations of class exploitation generate resistance, with the effect that working-class youths leave school in order to celebrate their own image of life (Willis 1978). Which of these explanations, if either one, is right? Indeed, it is logically possible that both could be right for some proportion of working-class school leavers.

Is the nature of the structural relation expressed most satisfactorily by the concept 'environmental adversity'? Or does the concept of exploitation, a form of class domination imposed through extracting surplus value from labour power, describe the generative mechanism more accurately at this level? The information necessary to answer these questions – the forms of investigation required – are inherently theoretical. In exactly the same way, we may ask whether the effective disposition is one of demoralisation or resistance. This question, too, demands a high level of theoretical attention, involving sociology and psychology.

Gorard asserts that, ‘in natural science the actual philosophy adopted by practitioners makes no difference to how they proceed’ (p 12). But this depends somewhat on who the practitioner is: Cyril Burt’s philosophical views, for example, certainly affected how he conducted himself as a scientist. Nevertheless, Gorard believes that, ‘[i]n an ideal research world the evidence uncovered by any researcher would be very similar whatever their starting point’ (pp 11–12) – and this is just what cannot happen, he suggests, in a social science infected with the disease of continental theory.

But if we go a little deeper we might find this view to be unduly pessimistic. It is possible that researchers do generate similar results more often than not, despite their adherence to very different theories. The accounts of contemporary English working-class life presented by Skeggs (1997) and Charlesworth (2000) contain good illustrations of what might be called this divergence to the real. The influence of the ‘great thinkers’ who have inspired these authors (Foucault for Skeggs; and Heidegger and Bourdieu for Charlesworth) could hardly be more apparent – as students in this respect, Skeggs and Charlesworth have certainly paid their dues – and yet, as narratives of working-class life, their books have a great deal in common. These writers certainly complement rather than contradict each other, and this is largely due to the fact that both use a structure-disposition-practice scheme when explaining specific states of affairs. This fact is apparent even from a brief account of their research.

Skeggs writes of the desire of the women she studied to be perceived as respectable, presenting this as an amalgam of signs and practices so that they are ‘positioned by their furniture and paint’ (p 90), and of how they speak from given subject positions in discursive frameworks. The word ‘positioning’ must occur, and more than once, on every page of the book. But one can plainly detect the implicit argument that structures of the labour market (educational provision, child-care, housing etc) not only give rise to characteristic dispositions of doubt, unease and cynicism, but also to dispositions that support capacities for self-determination; and that social practices emerge from this set of dispositions that make it possible for the women to individually and collectively survive .

Charlesworth, on the other hand, has a different theoretical lexicon. In his book, the key words are ‘experience’ and ‘being’. The task of the researcher, he says, is not only to describe social practices, but to show how people feel and why; what those feelings reveal about their relationship to social reality; and ‘how their being is mediated such that they are the subjects of such feelings’ (p 8). The objective conditions of working-class life are such that class becomes ‘a locating of the flesh through inhabiting a particular social realm’ (p 65) – a realm so oppressive, it generates dispositions of estrangement and alienation of such intensity that Charlesworth likens the privations of the ‘displaced men and women who rot alone in council flats and bed-sits’ to ‘the punishment of solitary confinement’ (pp 278–

279). The explanatory scheme – from structure to disposition (*habitus*) to practice – could scarcely be more explicit.

Charlesworth has produced the more penetrating work – at least in my opinion – and it is possible that he only 'got under the skin' of the people among whom he lived because he tried to do exactly that. It is also possible that the women Skeggs studied, just as intimately and actually for a longer period, would have seemed more vivid had they not been treated as the mouthpieces of 'discourse'. But it is quite feasible that these authors could have swapped theories and come up with much the same accounts of working-class life as the ones they have presented from their chosen perspective. If I think that Charlesworth has written the better book, it is not because I think his 'theory' is necessarily better, but because his insights seem more profound, and because his handling of the real explanation they share, in its account of particular events and states of affairs, is more thoroughly worked out with respect to several linkages.

We should not, for a moment, entertain the notion that the 'great thinkers' who have guided these writers have led them to produce worthless reports. Is it seriously argued that the support given to these writers would have been more productively spent on quantitative social research? This is just a mistake. If there are weaknesses in these studies, quantitative information would not in any sense remedy them. It might be useful to know that the number of 'displaced men and women who rot alone in council flats' is this or that, but counting them is one thing and attempting to show that their experience is like 'the punishment of solitary confinement' is another. I also have my own opinion as to which task is more challenging.

Why are social sciences, like politics and economics, so backwards when compared with chemistry and physics? In asking this question, Gorard repeats a familiar lament. But the answer is almost self-evident. It is because the structural relations, dispositions to act and established practices that must be included in our explanation of social events, processes and states of affairs are not like the structural relations, behavioural disposition and modes of operation of physical entities. Social and physical entities have different properties. This point should require no elaboration or defence. The bonds that hold together a family are not of the kind that hold together the molecules of timber and steel of the house in which they live. What is the nature of the relations that constitute social entities? What dispositions are generated? What practices are established so that the society – or more strictly speaking, its members – can achieve what they want?

This actually requires a form of quasi-legal investigation rather than one modelled on the procedures of physical science (Eliaeson 2002). When one says, for example, that the jury is out on whether the effective structural mechanism is 'adverse circumstances' or 'exploitation', or the effective disposition is 'demoralisation' or 'resistance', then the metaphor is quite accurate. Indeed, these questions must be settled in this way; by a consideration of the evidence, for and against. The quality of that evidence, sometimes quantitative in form and sometimes not, depending on the nature of the case, may only partly be assessed by the procedures used to obtain it. The most scrupulous methods can sometimes come up with nothing: the most slovenly methods can sometimes give us all the evidence we need.

The kind of naturalism Gorard advocates – 'at core, the nature of scientific enquiry is the same in all fields' (p 12) – is perfectly acceptable, but as one is concerned with entities that are different in kind, scientific realism must recognise that relevant differences will affect the particular techniques by which their properties are described. This is the basic reason why a research programme designed to accumulate knowledge in a specific domain is not necessarily suited to answering all questions that might be put to the world. Sometimes it is the nature of social structures that 'inhibits cumulation' (p 9), a complaint Gorard echoes, rather than a non-scientific theory as such.

The model of a cumulative science is, of course, taken from the natural sciences. When teams of scientists from all parts of the world undertake to map the human genome, they collaborate on a project that accumulates knowledge in the clear sense of working towards the completion of a finite task; towards the description of a well-defined area of the world. Sociologists may carry out projects of this kind, but not all sociological investigations can be of that kind. The only sense in which studies of, for example, working-class life can be cumulative, is that they fill up the library stacks. We should just accept that this is the nature of the work: the structures that define the position of the working-class, the dispositions of classed beings and the generated practices of class life are ever-changing. It is plainly wrong-headed to suppose that social research can be cumulative in the sense that a programme with full methodological integrity might produce the definitive account, the completed map. I have suggested elsewhere that a lot of sociological research has the same character as housework: as soon as one has finished, it is time to start again (Nash 1999).

The place of models in explanation

It may be scientism rather than science that pushes Gorard to argue that science is concerned with providing simplified models of events. A simple model is useful when it is possible to construct one, but if the double helix model of DNA is 'simple', then that is because the reality is 'simple'. A one-tenth scale-model of an airscrew that can be tested in a wind tunnel is also simplified in certain respects. The introduction of ideal types to social science was directed at simplification for the purposes of explanation. Indeed, a characteristic simplification of this kind is the fundamental assumption of marginalist economics: that consumers can be treated as if they were rational agents interested in maximising the utilities intended to facilitate the construction of models for explanatory purposes. In itself, simplification only counts in an explanation because it is easier to deal with than complexity.

The way that models explain has little to do with the fact that they are, in different respects, simplifications of the situation they represent. A model is an explanatory tool in that it simplifies, actually or in a conceptual sense, an aspect of reality and its conditions of being that we need to know. A computer model of the effects of a 10-metre flood on a river plain thus explains why the water will reach a certain point on the ground in an actual flood. A *copy* model – a model of the thing itself – explains to some extent simply by making it convenient for researchers to inspect its nature. The double helix model of the DNA molecule has that form, and what was remarkable about Crick and Watson's *discovery* was the fact that they made it on the basis of the limited information available. The isometric model they constructed is actually a description of reality, and has explanatory capacity in that it shows why the empirical data, the combination of bases etc, take the pattern they do.

Some models are designed to provide estimates of the discrete contribution made by specific properties of the world that affect processes under investigation in a controlled environment. Researchers can scale down a turbine propeller, place it in a wind tunnel, and plot its energy generation with systematic tests of the effects of changes in pitch, wind speed etc. The model is explanatory in that it shows how the full-sized version will behave in the environment in which it is designed to operate.

Non-isometric models, which are not copies of the entity to be explained, provide explanations of quite a different kind, through analogy and metaphor. If an astrophysicist likens the behaviour of the galaxies in an expanding universe to that of raisins in rising dough, the model is not the thing itself, and it has explanatory capacity by virtue of the fact that it provides an analogy for assisting thought. When Bourdieu says that it is *as if* some practice is determined by the reproductive need of the system, the sort of functionalist explanations he often provides is also a model of this type (Bourdieu 1998). One example of this is the interminable duration of study required to complete an academic thesis. Harré distinguishes between descriptive and analytical models, but it is arguable that this two-fold categorisation is too narrow. Wartofsky (1979) presents a more extensive, but also more flexible, typology of models, and regards all models as descriptive and analytical in some sense.

Explanations made in terms of ideal types have a curiously problematic structure. There is another theory about why working-class students are less likely than those from middle-class backgrounds, even with similar qualifications, to enter higher education. Boudon (1982) suggests that the opportunity cost structure varies with class position: working-class students have relatively further to travel, relatively higher actual costs, and relatively higher opportunity cost (monetary and social). In these circumstances, even when students attach the same value to occupations, they may express a different preference. This is a plausible theory with some influential adherents (Goldthorpe 1996). This explanation, deriving from the ideal type model, is not that students actually *do* make decisions in this way, but that it is convenient to *suppose* that they do in order to explain the observed pattern. If the pattern is consistent with the model, and if working-class students enter lower destinations than middle-class students even when qualifications are controlled, the pattern must be consistent. Thus, the ideal type is said to have explanatory power. However, the entire procedure is obviously circular and impervious to test.

It is hard to understand why this form of explanation should be supported by social scientists who position themselves as strict adherents of the principle of falsification. Moreover, unless working-class students actually make decisions on this basis, the assumption that they do, if it is false, might be worse than useless. If, after all, the decision more often flows from ‘demoralisation’ or ‘resistance’, and if knowledge of that is important to providing intervention policies, which is likely to be so, then we are just misled. Wartofsky’s (1979, pp 38–39) comment on this is worth repeating:

> If the argument runs: ‘I don’t really take the entities in the model to exist, but it is useful to think of them in that way in order to pursue a point, or to conceive tests for a theory with cognitive claims which the model somehow images’, then I would raise the question as to what makes it useful to think of it that way at all, if there were not some sense in which the model mirrored some aspect of what it is taken to be a model of. In short, the existence of claim of a model may be limited in scope or applicability – i.e. in systematicity or generality, therefore – but to deny such a claim makes a mystery of its significance altogether.

This penetrating observation applies with just as much force to ‘statistical’ explanation. A statistical model explains insofar as it reflects the reality it is designed to represent. Therefore, the accuracy of that match can only be made given an adequate knowledge of what is actually the case. This is important: if a model explains in as much as it reflects the reality it simulates, it follows that knowledge of reality, independent of the model, is necessary to its interpretation. Statistical models in education are almost never tested in this way, and the question of what kind of explanation they give, if any at all, can be a little embarrassing.

An input-output model that can predict, for example, the gain in student achievement resulting from a given change in teacher salaries, when other variables are controlled, might appear to offer policymakers a valuable instrument of management. Models of this type, of course, should never be taken as if they were non-problematic accounts of reality. Such models are explanatory in that if the system does behave in the way predicted (if a gain in reading scores follows an increase in teacher salaries), it can be argued that the assumptions on which they are based are correct. If the social world is a closed system, as the model is by definition, then the explanation will be correct. However, if the social world is an open system, which is actually so, then the information provided by the model is not determinate and may even be irrelevant.

Explanations of social events, processes and states of affairs, which have a structure-practice-disposition form, require what might be described as a kind of elaborated common sense. The process of elaboration is necessarily one that takes place as the result of an engagement with theory, in the widest sense of a systematic and disciplined enquiry into the conceptual foundations of social science. Even if we insist on using the term theory in a strict sense, which is my own preference, we still have to find a term for this essential philosophical labour. It is not by any means clear, for example, that Gorard’s eclectic blend of moderate relativism (largely realist ontology) and pragmatic theory of truth is strictly coherent, but it would require a somewhat less friendly critique to develop that point. I am content to hint that such an enquiry would be ‘theoretical’ in character.

What are attacks on 'great thinkers' worth?

How are we to interpret the criticisms of social research marred by the baleful influence of 'great thinkers' (Gorard's list comprises Marx, Freud, Vygotsky, Foucault, Bourdieu and Lyotard)? Is it just that some – no doubt, too many – social researchers misunderstand the theories developed by such authors, or that such lists of 'great thinkers' include only those whose 'grand theories' are so non-scientific in character that even their use in fully competent hands is, and must necessarily be, without merit? Gorard is unclear on this point, and it is thus difficult to refute his criticism. The academic community is full of people who think that they are among the few who understand what Foucault or Bourdieu 'really meant', and suggesting that such writers are often subject to misrepresentation is not a criticism of their complex positions.

The more fundamental argument is that the 'great thinkers' who appear on such lists have developed non-scientific theories, which cannot but generate nonsense (and potentially dangerous nonsense at that). This argument has a different status. Perhaps the best known of all such criticism is Popper's (1962) attack on Plato, Hegel and Marx, who seemed to present their theories in a way that made it impossible to prove them false. Bunge (1986), whose scientific realism is at least systematic, takes the same robust position.

The dangers of an anti-scientific orthodoxy asserting a dominant influence over intellectual activity are obvious. Insofar as Gorard opposes these tendencies, I have no quarrel with him. However, there may be just as much danger in a form of chauvinist anti-intellectualism (even when presented as anti-clericalism) that bundles together thinkers with foreign names – particularly when they happen to be French, as if this were sufficient to condemn them. I have learned a great deal from Marx, Bourdieu and Vygotsky. If I have but little time for Foucault and Lyotard, I am at least prepared to read those who have, and even with a modicum of tolerance and respect. Gorard seems to have read Rorty to his advantage, and whatever one makes of Rorty's philosophy, it can safely be said that he does not have a Popperian view of science, and thus we may both be open-minded to that degree. But to find Rorty quoted in support of a polemic against postmodern forms of thought does have its ironies (Bhaskar 1991).

Some forms of critique should not be accepted as entirely serious: amusing in their way, and easy to do, but not at all serious. Bourdieu is on Gorard's short list of those 'great thinkers' whose non-scientific approach to sociology is copied slavishly by misguided researchers to the irremediable detriment of their studies. It is not unexpected, therefore, to find that Gorard takes the concept of *habitus* to illustrate the apparent nonsense that surrounds any text in which it can be encountered. I have published articles with *habitus* in the title myself, and Gorard might easily have picked a sentence from my work, but there is a target already to hand: Gorard follows Tooley and Darby (1998) in aiming his shot at Reay.

In a study of primary school classrooms, Reay (1995, p 357) allowed herself to write in an introductory context, '[h]abitus can be viewed as a complex internalised core from which everyday experiences emanate'; and Gorard asks, rhetorically, ‘what does that mean?’ But is this sentence really so obscure? It states that there is an internalised core from which everyday experiences emanate. Her sentence certainly raises a number of difficult questions about the nature of the ‘internalised core’, including its origin and power to generate experiences, for example, but those are somewhat different matters. We should be able to extract the meaning from this sentence. It may even be a little strained to speak of *experiences* emanating from the *habitus*, but if we accept the *habitus* as a cluster of dispositions to act, then actions clearly do emanate from the *habitus*. And, in as much as actions are *experienced*, then the obscurity disappears.

We should not resort to tit-for-tat arguments, but if Reay’s concept of experience is somewhat ambiguous, the same might be said of Gorard’s. He writes, for example, ‘given that no one is suggesting that we have direct experience of an objective reality, we should be more concerned with finding better ways of describing what we *do* experience’ (p 6). But as to that, I’m more than willing to suggest that we have direct experience of objective reality. I live in a country where earth tremors can be felt several times a year, and I think that when my house creaks and rattles as the shock wave smacks through, that my experience of the earthquake is as direct as it can be. It is hard to understand, indeed, what would count as an *indirect* experience of any kind. The lesson here, to labour it no further, is that theoretical (philosophical and conceptual) writing is not as easy as it looks, and that a charitable response towards minor and theoretically unimportant confusions, if that is what these are, may be the most productive.

Even to speak of the operationalisation of concepts, for example, is to declare a theoretical position. This formulation, moreover, is arguably not the best for realist science to adopt. The term ‘indicator’ is a useful one. Indicators point to something, and what they point to is a matter to be settled by a form of enquiry that is basically theoretical or, to be more precise, conceptual. If the form of explanation we want refers to structures, dispositions and practices, then our indicators should point to a direct property that can be integrated into the explanatory narrative. But ‘operationalisation’ is more problematic. The idea is that we have a *concept*, such as demoralisation or resistance, and devise an indicator that then enables the concept to be ‘applied’ in the operations of research. However, the term is redundant and misleading, for a concept and an indicator are not combined as a single entity by virtue of the claim that an indicator points to a property expressed by a concept. The basic difficulty here stems from the nominalist epistemology of the standard approach to quantitative social science.

Scientific policy making and the relationship with theory

Gorard suggests that policymaking and philosophical commitments are relatively independent. He declares that ‘a policymaker who believes that human rights are an inalienable part of the soul and one who thinks that human rights are simply an admirable invention may both suggest the same policies’ (p 16), to which the obvious response is that a politician who believes that some human beings have no rights is unlikely to advocate the same policies as one who thinks that all human beings have the same rights. In as much as what people believe and know affects what they do, then 'theory', in this wide but perhaps inescapable sense, makes that much difference to what they do. It is really not necessary to start all discussions from scratch. There are matters to do with the relationship between policymaking and the support of social science of much greater importance, especially in education.

The complex nature of the relationship between official, academic and popular discourse has, in fact, become a matter for serious investigation. What has been called the 'dumbing down' of educational research has become the subject of periodic complaint (Wilson 2003). An institutional context seems to have arisen in which it is possible for a well-placed observer to note that, 'if you want to win contracts, there's a strong incentive to find what the funder wants you to find, and to design your study in line with their needs' (Brighouse 2003, p 132). At a more general level of analysis, Beck (1992, p 167), no more impressed with postmodern excesses than Gorard, has noted that 'science, having lost reality, faces the threat that others will dictate what that truth is supposed to be', and has examined the consequences of this for the relationships forged between governments and those contracted to provide them with knowledge. The overt politicisation of scientific research in the contemporary state, particularly in social science and education, has hardly gone unnoticed.

Gorard reproduces Davis's (1994, p 188) complaint that, 'we have to put up with an appalling amount of bunk … simply because we cannot draw a firm line between what is legitimate academic sociology and what is not' (p 13), and there is no need to disagree. But I imagine that there is an appalling amount of bunk to put up with in many areas of life – journalism, television and politics come to mind at once – and sociology is not exempt. Indeed, we might even have a different view about what is bunk in this field and what is not.

If there is an implicit suggestion here that something called 'legitimate academic sociology' cannot be defended other than by the existing processes of institutional control such as through refereed journals and qualification bodies, then we really will have to put up with bunk, for a politically regulated imprimatur is the only mechanism of control. In any case, no one is actually compelled to read what they find to be bunk, and one is perfectly free to lead a blameless sociological career influenced only by Lord Giddens, whose name I have yet to see on a list of 'great thinkers' guilty of corrupting the young (although *structuration* is likely to be as resistant to 'operationalisation' as *habitus*).

I do not underestimate the difficulties of working with the mixed-method approaches Gorard advocates. Within the last decade, I have published small-sample, interview-based studies and large-sample studies based on questionnaires. I work within a family resource framework, which I am generally careful not to refer to as a theory, developed from an engagement with Bourdieu. At the same time, I make increasing use of statistical evidence. The absence of a secure disciplinary identity, however, has probably done little to enhance the reception of my work.

Articles that actually combine methods, rather than make use of one or the other, are particularly difficult to place. Journal editors obviously have some trouble in finding readers competent to deal with ‘numbers and narratives’ in the same text, and their usual compromise of sending a ‘mixed-mode’ paper to a quantitative and qualitative reader almost inevitably provokes a negative response from both. There is no excuse for sociologists to be blind to the networks of social relations that create and conform the identities on which our professional careers are made.

So we must make our choices – or have them made for us. There *is* some common ground here. We obviously dislike the particular nonsense of postmodern thought, as did Bourdieu for that matter, but it seems to me that Gorard inclines more towards *scientism* than is proper for a real scientist – or perhaps, I should say, for a scientist of the real. It difficult to understand how qualitative researchers *could* prevent the integration of interviewing, participant-observation, textual analysis etc that constitute the techniques of qualitative investigation within programmes directed by researchers aligned with Gorard. No special consideration for theory is necessary in a particular study to achieve the combination of different data-collection techniques (questionnaires, case studies, interviews); and of data-analysis techniques (logistic regression, context description etc). But we are all aware that debates about method are often coded disputes about what research questions are asked and what should count as an acceptable answer to them.

The controversy over method is really about the effects of positivism on the conduct of research. That controversy will be resolved, if it ever is, only when greater theoretical clarity is attained on what the effects of positivism are, and how they can be eliminated. Gorard’s realism, moderate empiricism and willingness to engage in this conversation may be recognised as a move towards the construction of an integrated non-positivist theory, within which the integration of techniques should become a matter of routine. The procedure of science, the formulation of testable hypotheses, the construction of indicators, the deduction of conclusions and so on is no more than the formal shell of science. It is not the *method* of science in the most important sense. The debate about quantitative and qualitative methods as technique is unproductive, finally, because it is not method or technique that generates explanations, but informed thinking in the widest sense. Explanations, furthermore, are not necessarily cheap (for all that Gorard declares that theory is cheap); they are likely to be expensive in as much that the construction and testing of explanation is an extremely demanding form of critical intellectual labour – may we say of *theoretical* labour?

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