Three abuses of 'theory': an engagement with Roy Nash

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Abstract

This paper summarises three abuses of 'theory' described in my previous paper in this journal. These are the theory of incommensurate qualitative and quantitative paradigms, the needless deification of past theorists, and the insistence by peer-reviewers on an explicit theoretical framework for all empirical work. These three abuses are widespread in UK education research. They form part of the difficulty in enhancing the capacity of professional researchers to treat a variety of research methods with respect, and to consider the use of mixed methods in their own work. The paper follows the response by Nash to my previous paper. It recognises our areas of agreement, suggests that some areas of disagreement are based on misunderstanding, and considers the impact on Nash's response of his well-known enthusiasm for using the theoretical concepts of Bourdieu.

Introduction: the three abuses

I shall start this engagement with Roy Nash by expressing my delight that he had read my original paper (Gorard 2004a), and taken the trouble to respond in full (Nash 2005). I generally enjoy reading such ongoing discussions in what are otherwise the sometimes rather dry pages of journals, especially when the discussions are of a matter of fundamental importance to research – as I believe this is. I was, therefore, happy to accept an invitation from the Editor to keep the discussion going.

Nash started his paper by outlining some of the many ways in which we agree about the ways of doing research, the initial training of new researchers, and probably many other things. I have always enjoyed reading his work, and am disappointed that our mutual connection to Wales has not led us to a meeting yet. Clearly, with so much agreement it is more efficient to focus in this new paper on the areas where there is less agreement. However, I should like to stress that I fully

endorse the idea of preferring the term 'explanation' instead of the much wider term 'theory' in the instances suggested by Nash, and as used in my original paper.

The three abuses of theory, or of the term 'theory' if Nash prefers, in my title are: the invented research 'paradigms' used as silos to protect their inhabitants from having to work with both text and numbers; the formulaic parading of the conceptual terms from a big theorist in papers by avowedly 'qualitative' researchers; and the insistence by many peers in social science on the necessity for a lavish theoretical framework for *all* empirical work. All of these abuses have in common a celebration of theory for its own sake, and a tendency to get in the way of researchers attempting to make the research combination of text and numbers a routine phenomenon. It is important for Nash to realise that I do not suggest that these abuses of theory are the only or even the greatest barrier to combining methods (see Gorard with Taylor 2004). So, for him to argue that there are other important barriers as well is not to argue with my thesis at all (see below).

I shall briefly rehearse my concerns about these three abuses before commencing a more detailed response.

The paradigm abuse

From 2000, I was charged by the UK Economic and Social Research Council to direct a project aimed at enhancing the capability of the education research community to undertake rigorous studies relevant to policy or practice. One of the four main objectives specified was to enhance our capability to undertake work that routinely mixes the approaches traditionally called 'qualitative' and 'quantitative'. There were several barriers to such a project, not least of which is the very low proportion of researchers with skills in 'quantitative' work (but see Gorard et al. 2004). However, beyond the technical, training, resource, and confidence issues lay a further barrier – expressed as outright hostility to the mixing of qualitative and quantitative approaches, almost invariably from senior mono-method qualitative researchers, and justified by reference to Kuhn's theory of scientific paradigms.

Kuhn's theory of paradigms (Kuhn 1970) suggests that groups of investigators tend to settle within a norm-referenced framework to try and solve closely defined 'puzzles', and that these frameworks are periodically disrupted to such an extent that there is a paradigm shift which eventually settles down to a new puzzle(s), and so on. The shift may have a variety of determinants, but a common one would be new evidence based on a new way of looking at the puzzle. The shifts from Newtonian physics to relativity to quantum physics are often cited as examples. However, the permanently hostile division between those education researchers who are prepared to use numbers as evidence and those who are not is so far from this idea of paradigms that it is not worth developing further here. Nash agrees that there is no epistemological reason why researchers should not use both numbers and narrative together. So the paradigm abuse is not discussed further here (but see Gorard with Taylor 2004).

The big thinker abuse

The second kind of abuse of theory suggested in my original paper is the routine quoting of the theoretical terms of past big thinkers in new, particularly, qualitative work. The paper argues that, in many cases, this form of citation amounts to a litany of no relevance to the substantive content of the work. In other cases it is worse than useless, producing needless obfuscation. Given Nash's regular use of ideas from the writing of Bourdieu it is, perhaps, unsurprising that he disagrees that this abuse exists.[1]

Nash objects to a purported 'list' of names of the big thinkers highlighted in my original paper (he actually refers to it as a 'black list'). He accuses me of chauvinism because, according to his description, the people on this list all have 'foreign' names.[2] This is a ridiculous assertion that does Nash no credit at all. First: there *is* no composite list in my paper - only in his. Second: the names mentioned in differing places in my paper are not produced by me, but are suggested by a number of sources, including Hollis (1994), Sullivan (2002), and Tooley with Darby (1998). Each of these argues against the big thinker abuse, but based on a *different* set of actual individuals. The names of these big thinkers appear in my paper because these are the ones most often used in educational enquiry – which is my field and also that of the journal in which the paper is published. They are the ones occurring most often in my analysis of the 9,000 pieces of research submitted to the 2001 Research Assessment Exercise panel for education in the UK (Gorard et al. 2004).[3] Does this make the individuals submitting this research to the RAE chauvinistic, or perhaps inverse-chauvinistic, because they tend to cite theorists with, what Nash calls, 'foreign' names?

Later in his paper, Nash imagines an era in which only the theories of Giddens are safe from criticism. My comments on the weaknesses of Giddens theories will have to be the subject of another paper, but the point here is that if Nash considers 'Giddens' not to be a foreign-sounding name then he should be aware that the reason Giddens is not mentioned in my paper is that none of the sources I cited mentioned him, for the perfectly proper reason that his ideas are seldom used in mainstream UK education research. He is omitted as an irrelevance, rather than due to his possession of an Anglo-Saxon name (if that is what it is). Nash is quite wrong to suggest, as he does, that my paper simply lists these theorists 'with foreign names, particularly when they happen to be French, as if this were sufficient to condemn them'. In fact, I present a generic argument against the big thinker abuse, refer to the writings of others who are also critical of the big thinker abuse,

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[1] Although it should be noted that I have yet to read anything of his that comes into the category under discussion.

[2] Would it have been better or worse for me to have included Derrida, Weber, Bernstein, or Durkheim? Interestingly, in light of what Nash goes on to say, 'Gorard' is actually a *French* Huguenot name. What about Xin Xiang (the name of my son); is that not a little 'foreign' sounding?

[3] Bernstein also appears among these 9,000 publications, but not in the intersection with the cited critics of this particular theory abuse.

and then give a longer consideration to the deficiencies of UK work that uses Bourdieu.

From this longer section in my original paper (which he must therefore be aware is not merely a list deemed 'sufficient to condemn them'), Nash tries to defend the work of Reay (1995). He sets out to explain her claim that 'habitus can be viewed as a complex internalised core from which everyday experiences emanate' (p. 14) as follows. According to Nash, this sentence 'states that there is an internalised core from which everyday experiences emanate'. Insofar as this simply repeats Reay's original sentence this explanation is unhelpful. And, because it does not refer to habitus (the subject under discussion) it is also misleading. It might be true that such a core exists, as Nash's version suggests, but that it is not the same as 'habitus', which is what Reay claims. When I asked what this claim meant I was *not* simply asking rhetorically. I genuinely do not know what habitus is, and neither of the explanations proffered helps me. What does this core consist of? Is it internalised to an individual? How? Does Nash really believe that our everyday experiences, such as watching TV, *emanate* from this core? I thought TV was broadcast from outside my house. Is Bourdieu proposing a form of solipsism here?

Given that Reay also claims that it is the very looseness of the concept of habitus that appeals to her, I strongly suspect that she means nothing of any scientific interest by it at all. As I described in the original paper, many of these theoretical terms, such as habitus, used second-hand in the ways that I consider an abuse are either nonsensical or uninteresting once converted in clearer language (see also Mills 1959, Cole 1994). Nash's call for me to be more charitable in dealing with the 'minor and theoretically unimportant confusions' of Reay is, therefore, misplaced. Reay is not merely evidencing an unintended looseness in the use of theory – that, after all, could happen to any of us – she is actually *advocating* and celebrating looseness in the use of theory, and that is why her work was selected for critical comment by me. Interestingly, Nash ignores this vital distinction, and the similarity between her celebration of ambiguity and the nonsense of postmodernism (in science as opposed to art), when he joins me in condemning the latter. Perhaps if my extended example had involved Foucault, whom Nash has 'little time for', instead of Bourdieu, the particular litany that Nash uses, our area of agreement would have been greater.

I am not alone in believing that the conceptual terms of big theorists are often used by others in a formulaic way – witness the many references in my original paper, and also the unanimity in the interview quotations in chapter nine of my related book (Gorard with Taylor 2004). I am surprised that Nash has not experienced the same. This does not mean that the work of these thinkers cannot be read with profit, as I hope could the writing of others such as Nash, and myself.

The framework abuse

The abuse of insisting on a big explicit theoretical framework for all empirical work is discussed at length at the start of my original paper and, again, many commentators are quoted in support. The paper is very far from advocating

abstracted empiricism, but gives examples of important questions for research that could be tested without grand theoretical work. Unfortunately, Nash has not understood my purpose in these early passages. For example, my list of possibly important projects on topics such as single-sex teaching, class size and so on, are suggestions which are essentially practical, and driven by practitioner demand. Nash seems to believe that I recommend these simple projects as the *only* ones worthy of study. The point I am making is that a big theory is not always essential. Nash reads me to mean that a theory is never necessary. If this difference is due to my lack of clarity then I apologise, although, in truth, I cannot see where the ambiguity lies in my original piece. Readers aware of my work relevant to, and testing, public choice theory (Gorard 1997, Gorard et al. 2003) or human capital theory (Gorard and Rees 2002, Selwyn et al. 2005), for example, will know that I could not have meant what Nash imagines. Despite the fact that, on occasion, I work with big theories I wish to reserve the right not to on other occasions, just as I wish sometimes to combine methods and sometimes not. These pragmatic choices are what the framework abuse, as exemplified in the comments of peer-reviewers, seeks to deny me. Since Nash does not seem to recognise this third abuse of the idea of theory in research any more than the second, the paper continues with a more detailed engagement with this issue.

So what is a 'theory'?

I have no wish to 'separate science from theory'. My concern is only with theories having no explanatory/predictive power. But my idea that a 'theory is a tentative explanation, used for as long as it explains or predicts…' is mocked by Nash asking whether Darwin's theory of evolution is meant to be tentative, or whether Hartley's theory of blood circulation should only be used while it *predicts* the circulation of blood. My answer is that both are indeed tentative, and could be replaced tomorrow if a superior explanation is suggested. However, both are so well-established (Darwin's in particular) that they now contain elements of tautology, because the things they explain are themselves partly characterised by the theory. For example, when Darwin's theory is superseded it is likely to be by a theory that changes our idea of what evolution is (and so what a relevant theory addresses) rather than by a superior explanation of evolution as currently understood. Hartley's theory, anyway, does not have to continue to predict anything according to my description of theory because of the word 'or' in the phrase 'explains *or* predicts'. Nash seems to have read this 'or' as an 'and'.

Ironically, at the start of the next section, Nash provides an approving description of scientific theory from Kim (1983) that appears to be little distinguished from the one he objects to me suggesting. Nash also upbraids me for suggesting that epistemology and ontology lie, at least partly, in the realm of theory. In his world, apparently, philosophy, or perhaps only what he terms 'the most demanding and technical area of philosophical enquiry', is not theoretical. In my world, philosophy is theory *par excellence*, and quite properly and usefully so, although it is naturally open to abuse. I hope that he and other readers are clear on my intent. I realise that the word 'theory' is used widely and loosely, and cannot

hope to change that. I wish to distinguish the useful theory work from mere persiflage or worse.

Nash is confused in his discussion of isometric and non-isometric theories. Although the distinction is one of graduation rather than clear categories, the kinds of non-isometric theories that Nash advocates are not sound theories at all. When a scientist refers to the expanding universe as being somewhat like a rising ball of dough with raisins in it, they are not suggesting a theory, or an explanation as Nash intends this to mean elsewhere in the paper. Analogies and metaphors can be useful when trying to 'explain' to others, to lead them to your understanding. But the astrophysicist in this example would not seek to test the dough 'explanation', and we would be making a crucial error of understanding if we set out as a consequence, to seek the raisin-like properties of galaxies, or debated whether the meta-oven enveloping the universe was powered by gas or electricity. Nash is correct, as far as I can see, in stating that the 'theories' of Bourdieu (among others) are precisely of this metaphorical kind – more useful in narrative or lecture than in scientific explanation.

Just how complex is social science?

Nash attempts to defend social science, especially sociology, from the accusation of having a large proportion of useless research by agreeing and then pointing out that things are probably just as bad in many other fields and disciplines. This is a non-sequitur (when accused of theft, it is no defence to argue that your neighbour is a thief also). Several commentators believe that the situation for sociology is particularly bad (Steuer 2002). They condemn pretend social science conducted by those who are actually antagonistic to science, and who are predominantly concerned with big theory and buzzwords such as globalisation, risk, or the network society. However, I do not work in the other fields listed by Nash, and nor does he. I work in education within a broader social science context. So does Nash. I believe that a lot of research in education is 'bunk' (a term from Davis 1994) and Nash agrees. We would be sensible to focus mainly on improvements in education research, and leave journalists and politicians to do the same in their own fields.

I suspect that, at heart, Nash's problem with my position is clearest in the section he entitles 'Shifting the focus to explanation' (a peculiar title since the previous section contains his critique of my description of theory as a tentative 'explanation', and so it is difficult to see what the focus is shifting *from* here). Nash presents the well-known thesis that theories in the social sciences cannot be similar to those in the natural sciences because social science is so much more difficult than natural science. Social science, for example, has to deal with the consciousness of individuals. And, as is usual in this thesis, his target is something he refers to as 'positivism'. It is a peculiarly self-defeating way of disagreeing with my paper in which I explicitly reject positivism, pointing out that the term is now little more than a lazy term of abuse for almost any scientific approach in social science.

Nash uses Bruce's (1997) example of explaining how a kettle boils without reference to consciousness of the kettle, and contrasts this with an explanation of

political action by a group of people. Let us be clear that we have no evidence at all that consciousness is a causative mechanism of political action or anything else (Gorard 2002a). Any explanation that involves conscious causation can be re-worded so that the consciousness is merely an epiphenomenon instead (via eliminative materialism, for example). Similarly, we have no evidence that a kettle, for example, does not have consciousness. If Nash means by 'consciousness' something that is only applicable to humans, then he is led to a tautology in any ensuing explanation that demands consciousness of humans but not of the other objects or actors involved. On the other hand, if Nash allows that consciousness might be applied to other objects or actors, such as other animals, then his distinction between explanations in the natural and social sciences collapses. There is no hint here that Nash wishes to classify biology as a purely *social* science, for example. He has a lot more work to do to make any headway with this rather tired defence of the separateness of social sciences.

The notion that social science is harder than natural science is, anyway, dealt with in my original paper. Does Nash believe that students rush to pursue A-levels, degrees, and careers in physics, chemistry or mathematics because these subjects are intrinsically so much easier than the more demanding fields of sociology or education? If so, he has been out of the classroom for too long. Does he imagine that most people faced with the choice of attempting an explanation for either an educational problem or one from particle physics would choose the latter for its comparative ease of understanding? Does he really believe, as he claims, that the bonds holding a family together are *less* open to inspection than the bonds holding molecules together? Most people would have a good idea what a family was (they could see one, for a start) and what the question about bonding involved. Most would be able to present several tentative explanations of the bond that could be compared with available evidence. Many people would have only a very vague idea of what a molecule was (probably only ever having seen a non-isometric model of one), and almost no coherent suggestion on how these could be bonded. And yet, as argued in my paper, natural science has seemingly made greater strides than social science. If natural science is not intrinsically easier, as I would argue, then it is feasible that at least part of its superior progress comes from its methods - including its somewhat more coherent use of theory.

In the section entitled 'The basic form of sociological explanation', Nash rehearses two general forms of theory to explain the differential education participation rates between occupational groups. And he outlines the difficulty of judging whether the structural or the dispositional explanation is the more satisfactory. Here is a clear example of the difference between us. Nash believes that we can go no further in testing these explanations. He offers no way out other than faith in one or other or both ideas. I, on the other hand, believe that we can test these theories but that if we can not then they are a scientific irrelevance. A truly scientific approach does offer a way out. But it is one that seems anathema to most social scientists who behave as though finding out was less important to them than having a big impressive theory. We could intervene in such a way that we could test directly the power of these two competing theories of participation. We could, for example, alter the 'adverse environmental conditions' of a subset of the disadvantaged group,

and monitor the extent to which individuals in this group left school at the earliest opportunity in comparison to any other groups, including the more advantaged in society and those who are disadvantaged but remain in adverse environmental conditions. Of course, if the funds were available for this then it might be preferable to move everyone out of adverse environmental conditions without concern for educational participation as such (but that is another matter).

Many social scientists espouse theories of a kind that could be tested directly, but *never* are, and this was another important point in my paper. It might be argued against my idea of testing theories of structure and agency that the two are interlinked in such a way that their impact cannot be isolated. An experiment of the kind outlined above could not help but be contaminated. Perhaps by altering adverse environmental conditions, we also alter the 'structural relations of class exploitation'. In this case we are dealing with an example of Hebb's rectangle, where it makes no sense to ask which side of the rectangle contributes more to its area. For all intents and purposes, the competing theories become untestable and so actually useless as scientific explanations. If we cannot distinguish between their impact on the real world, then to debate them any further becomes pointless, and so an unethical use of research funding (Gorard 2002b).

Nash is also confused about, what he terms, the problematic structure of explanations made in terms of ideal types. He cites Boudon and also Goldthorpe as suggesting that the opportunity cost structure for entry to post-compulsory education tends to differ for individuals in different classes, but argues that this theory is circular and impervious to test. I disagree. It is quite easy to gather evidence relevant to individuals' subjective opportunity structure (Gorard and Rees 2002). Non-participants in education generally report being aware of fewer opportunities and relevant resources than participants. For example, everyone in the UK has objective access to the internet because public access sites are spread widely around the country, and there are even mobile resources in remoter areas. However, those who end up still not using the internet are far less likely to report being aware of access to the internet (Selwyn et al. 2005). Similarly, with other forms of education. I have been in the house of an interviewee, who was bemoaning the lack of adult learning opportunities in his coalfield valley village, when a leaflet offering learning opportunities in the same street was dropped through the door. He threw this away, and when I pulled the leaflet out of the bin and challenged him with it he explained that it was 'not for the likes of me'. I believe, on the contrary, that this leaflet was intended almost precisely for people like him, but that it takes more than a leaflet to unpick the lifelong influences that had produced the relatively stable subjective awareness of opportunities that I term a 'learning identity'.

Nash is again confused by my phrase 'given that no one is suggesting that we have direct experience of an objective reality…'. He gives an example of experiencing an earthquake to show that this phrase must be incorrect. In fact, of course, there is nothing about an earthquake other than its power and danger that make it different to anything else that Nash experiences. All of it is subjective in the rather dull sense that this is what 'subjective' means. If several people report a very similar subjective experience of an earthquake at a similar time and place then we

can posit an objective reality of an earthquake. This experience of a shared objective earthquake is indirect, while the subjective experience is more direct. Therefore, we do *not* have direct experience of an objective reality. It is rather patronising of Nash to point out to me that theoretical writing is not as easy as it looks, and he is, it seems, hoist by his own petard here.

Conclusion

I briefly reiterate in this paper my concerns about some of the abuses of theoretical ideas in education as a social science. I believe that theory has a role to play in much research, but I also believe that good research is possible without a big theoretical framework. In too many cases, theoretical expositions are unnecessary throat-clearing, or an excuse not to conduct research at all. They should, rather, be a stimulus to critical testing in the field, and therefore to theory development. If not amenable to critical testing then a theory is practically useless, except perhaps as a metaphor to explain an already understood situation to others. Even as a metaphor I am not convinced by theory's pedagogic power. Perhaps their utility in explanation should be the subject of critical testing by education researchers. I feel sure that the prior theoretical position of researchers should and, in practice, does make very little difference to how genuine research is conducted and what conclusions are drawn. The examples from Skeggs and Charlesworth that Nash describes in detail, apparently to gainsay this point, are actually excellent confirmations of my point.

My critique of the widespread use of useless theory talk was intended to be independent of the methods used by researchers. However, I noted in the original paper that the barrier to mixed methods based on incommensurable paradigms is one proposed routinely by mono-method qualitative researchers. Nash, therefore, seems to believe that I privilege statistical explanations, which are often a problem because the models so generated are both based on and purportedly 'tested' by the same dataset. On the contrary, I have made clear that no method is privileged. I nearly always use mixed methods, and have considerable difficulty understanding why others appear so complacent in using one method over and over again for all investigations. And I have (I feel sure) been even more critical of the standard form of statistical explanation than Nash (Gorard 2003, 2004b).

I end, as Nash did, with a consideration of cost. Although Nash wishes his readers to appreciate the time and toil that theory work takes, his emphasis is rather disingenuous here. Research design, field-work, analysis, and producing warranted conclusions, if done properly, take at least as much intellectual intensity as theory-building. But these real-world elements of research take a lot more in addition, including finance. Theories can be a kind of thought experiment, and that is why they are cheaper than their alternatives. It is easier to divide a length measurement by two than to saw a table in half (and so on). Nash's response to my paper seems to have been admirably brisk – suggesting that he did not find it unduly taxing. The theory of relativity, often cited as the clearest example of a paradigm-shifting breakthrough, was developed as an unfunded hobby by a clerk in a patents office.

I have no desire for those in charge of allocating research funds to decide what is done with the money, or to evaluate the results. That is absurdly dangerous, and no incentive to quality research, as I have argued many times before (see Gorard 2002c). My comments in the original paper expressed concern for the views of people who actually *pay* for the research – the tax payers and charity-givers – rather than the bureaucrats who then control it. Every publicly-funded research project has an opportunity cost – the money could have been spent on schools or hospitals or simply given to the most disadvantaged in society. This means that we, as education researchers, have an ethical responsibility to use it wisely. Intellectual chatter about untestable propositions is not a good use of this money. Theories *are* cheap, and actually rather easier to deal with than real-life field-work. This is a strength that makes them useful, but one that also makes them liable to abuse by the indolent and the complacent.

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