How to Declutter Sociology:
A Manifesto

Edmund Chattoe-Brown University of Leicester

Abstract

This article argues that if being a professional sociologist entails too many unproductive intellectual commitments (which appear to have accumulated quietly over time as commitments do) then the result may be fatigue and loss of enthusiasm. By analogy with the recent informal movement for decluttering (reducing the commitments entailed by material possessions for greater psychological wellbeing) I list some candidates for unproductive commitments in sociology and explore the nature of their potentially demotivating effects. The candidates discussed (though there are probably others) are subjective defense of arbitrary disciplinary boundaries, unjustified anti-scientism, valorizing social theory, giving unsolicited advice, resistance to novelty, failing to resist enforced publication, and engaging in political apologia. I also consider the ways that such unproductive activities can feed off each other and be exacerbated by institutional pressures in a neoliberal academia. The aim of this article is to help professional sociologists to reinvigorate themselves while recognizing the considerable restrictions and pressure under which they now operate.

Keywords: Decluttering, anti-scientism, methodology, social theory, wellbeing.

It seems to me that sociology is suffering from a sort of fatigue. Perhaps I am really talking about myself and not sociology at all, but I shall write this article anyway and see what happens, in case I am not. In a way, this situation is odd because, far from wondering why people become sociologists, the question is really why they don't. If you don't care about religion, inequality, social change, politics, gender, sexuality, bureaucracy, ethnicity, employment or family (just to pick a few topics), surely you must be pretty much dead from the neck up? (Doubtless if I were being peer reviewed, I would have to be more academic in tone, to the point where no emotion risked discomfiting the reader. Perhaps that is part of the problem too. Enthusiasm and measured tones are uneasy bedfellows.)

Nor is it easy to see how finding out things that nobody knew before could fatigue us (assuming that this is what we aspire to do). This is particularly true if these discoveries contradict what politicians, journalists and other frequently unprofessional sociologists use, in their privileged positions, to impose on the general public. The UK is not a classless society (Breen & Jonsson, 2005) and it is recently becoming, if anything, more not less unequal (Alderson & Nielson, 2002). These facts are doubtlessly true of many other countries but I don't know what the politicians in those countries claim about them. The tools needed to understand these issues are still within the reach of first year undergraduates. Who could resist access to such dangerous truths? What kind of world do we live in when intellectual honesty (even about relatively uncontroversial descriptive matters) remains one of the most subversive activities of all?

So if our raw materials and methods are broadly as relevant as they ever were, what else could be going on? (The world also blesses us with a steady supply of important new things to understand like the Internet, Living Apart Together relationships, failed states and so on.) Could we, more or less directly, be doing this to ourselves? Certainly, we might well feel less fatigued in an academic paradise where we didn't do ever more teaching, where administrative change was modestly geared to tangibly improved outcomes rather than seeming to be a managerial end in itself, and where we did not have to target our research at the constantly moving goalposts of funders and governments. However, it is not clear that sufficient political will exists to fix these problems even among academics, let alone the general public. (It is an interesting question if we might be doing *that* to ourselves as well. Might governments—and the general public—take sociology more seriously if sociologists took social theory less seriously, for example?) So, then the question is what can we do as sociologists, within the real constraints that bear on us, to avoid this fatigue? How can we do sociology so that it boosts our energy (as the subject matter and methods suggest it should) rather than sapping it?

I chose the metaphor of 'decluttering' deliberately. This is a relatively new movement as a self-conscious approach (Cherrier & Belk, 2015) in which our attachment to possessions can be seen as directly or indirectly standing in the way of our higher happiness. We may simply find ourselves tripping over things or unable to find what we need or, less directly, the responsibilities of what we own may somehow make us feel weary and oppressed. (There is a sociological element to this logic. Everything we own entails commitments, from the need to store fragile possessions safely to the stream of expenses and tasks that come from owning a car. It is apparently not so difficult in the modern world to reach the point where one's life feels entirely controlled by these commitments.) By reflecting on what we *really* need and why, we may not just obviously make our houses tidier but surprisingly lighten our hearts. There are other accounts we could use with similar resonances. It may be tempting to eat lots of junk food but after a while one sickens, if not with the food itself, then with being overweight and unhealthy in consequence. (And we should not presume that such a long-term general malaise could be easily traced back to its source.) Restraint and exercise may be less immediately appealing but lead perhaps, in the longer term, to more uplifting satisfactions and a more robust sense of wellbeing. (But I cannot help being an empiricist. This may not work for everyone. It may not work at all. But until it has been clearly expressed, it cannot even be tried. That, I suppose, is the baseline of social science.)

This is all very well, but what should we get rid of? What is fatiguing us and why? Here is an incomplete list. Doubtless, if a wider movement to sociological decluttering starts, it will be possible to add more items or to refine the discussions presented here.

Sociology Itself

I am not so naïve as to suppose that disciplinary boundaries are unimportant. Jobs, students and grant funding go to departments with names like Sociology and Social Policy, and this is unlikely to change significantly. However, whether this practical importance should influence our thinking about things like research (where academics still retain some limited control) is another matter. I would like to outlaw the phrase "That's not sociological" with immediate effect. Quite apart from being entirely subjective, such statements are faintly ridiculous on reflection. Ethnography isn't sociological. We borrowed (or stole) it from anthropology. We borrowed rational choice (much as it is reviled) from economics. We have also lent to (or been robbed by) subjects that have subsequently partially or wholly separated from sociology: How much criminology is still based on work by people who called themselves sociologists and worked in sociology departments? The same is true of Gender Studies and Media Studies (and similar activities with variant names) however much people working in those fields may now dislike and underplay that fact.

To be slightly more analytical, it really isn't clear that such disciplinary boundaries are sustainable in practice. All we really have are subject matter and research methods, and neither alone (or even in combination) effectively marks out one discipline robustly from another. Sociology studies gender but so do psychology and criminology. Anthropology uses ethnography but so do sociology and management studies. (In a way, the clear exceptions are the most interesting. Why does economics have absolutely nothing to do with any qualitative method? I have never managed to extract a sensible empirically based justification of this from an economist.) The things on which we depend to keep us apart (for advantages that are not at all clear) actually turn out to be the tacit beliefs and non-evidence based claims we could best do without, even if these are dressed up in vague but affirmative sounding terms like the sociological perspective or point of view. Rarely such overlaps become interestingly visible. Economists, psychologists and sociologists all now do laboratory experiments to varying extents. Each group has deeply held (though largely undocumented) beliefs about such matters as payment and deception of research subjects, and these beliefs are significantly different. It seems that it cannot both be the case that paying subjects is the only way to get them to take experiments seriously and that, if you pay them, you will distort their behavior and yet these differences of opinion (plainly not fact since they contradict each other) become what effectively divide different experimental cultures. (This suggests a different approach to academic organization in terms of methods based communities. Perhaps experimentalists actually have more in common that their disciplinary differences would suggest?) These "religious" divisions seem to have no scientific advantages and stand in the way of us actually knowing something useful: The most effective way to arrange laboratory experiments accessing the data we want based on proper evidence that it *is* the best way. (Another implicit belief is that topics well handled within disciplines compensate for topics poorly handled by falling between them. Any evidence for this belief is, by its nature, biased. Who speaks effectively for topics of the latter kind? It is far too easy to ignore a psychologist who claims that sociologists are making a bad job of something.)

Clearly this is not an argument for "anything goes" in sociology or for the abolition of disciplines (which is not a realistic goal in any event) but I do wonder if good not harm would come from discontinuing attempts to enforce arbitrary boundaries on entirely subjective grounds. (If nothing else such policing does not seem recently to have sustained, let alone grown, sociology as a discipline – at least in the UK.) On a very practical level, next time someone presents an unusual piece of research in your department or at a conference, don't ask if it is sociological. Instead ask if it is any good! (This approach also links to other aspects of decluttering like seeking novelties that don't necessarily confirm one's own prejudices. How many sociologists when conducting a literature review make even a cursory effort to find out what other disciplines have to say on the topic? If this isn't happening, what are the reasons? It is hard to think of commendable ones.)

Anti-Scientific Assertions

This seems to me to be the real sociological equivalent of junk food: Superficially appealing but bad for you in the long run. There has been a significant industry in sociology attempting (whether deliberately or not) to undermine and devalue notions of science and evidence. It seems very bizarre that terms like "positivist" and "empiricist" and in extreme cases even "scientific" have become disparaging. (Sociology should not have shared rhetorical terms of disparagement any more than cricket should have.) As C. S. Lewis put it incisively (but in a somewhat different context) in *The Screwtape Letters*: "But flippancy is the best of all. In the first place it is very economical. Only a clever human can make a real Joke about virtue, or indeed about anything else; any of them can be trained to talk as if virtue were funny. Among flippant people the Joke is always assumed to have been made. No one actually makes it; but every serious subject is discussed in a manner which implies that they have already found a ridiculous side to it. If prolonged, the habit of Flippancy builds up around a man the finest armour-plating against the Enemy [God] that I know, and it is quite free from the dangers inherent in the other sources of laughter. It is a thousand miles away from joy; it deadens, instead of sharpening, the intellect; and it excites no affection between those who practice it… ". It may be easier (and perhaps more modishly controversial) to label the quest for evidence and pursuit of scientific method somehow naïve, but it doesn't seem to do the profession any good. Clearly, as with any approach, things can be done badly. Statisticians may be naïve about causality and the reification of variables. But it is simply faulty logic to argue that because things are capable of being done badly, that they cannot actually be done at all. (Another obvious example is the observation, apparently meant to serve as a critique, that social science is not like the natural sciences. This much is obvious, and has been for decades, but it is not an argument for avoiding the scientific method, but for working out how it can be properly applied to a recognizably different domain. If it turns out, at the last, that this really cannot be done, at least we will be absolutely sure why.) There is a whole range of approaches (from social construction—see Del Rosso, 2011—to postmodernism—"Simplifying to the extreme, I define postmodern as incredulity toward metanarratives [presumably including science]."—Jean-François Lyotard), which appear to suggest that the quest for scientific objectivity is some kind of delusion, and they can certainly offer anecdotal instances of naïve (or even simply false) beliefs in objectivity or science, but that is not the same thing at all.

One of the difficulties here (and another thing that may need to be decluttered) is the confusion of claims made at different levels. We can all recognize ground level sociological claims: "The odds ratio for a child getting into the salariat with salariat (relative to working class) parents is 36." In order to make sense of such claims we need to understand definitions and concepts, collect relevant data and conduct certain kinds of analysis. The rejection of such claims typically operates at the same level: Your data is biased, you misunderstood the concept or you used the wrong statistical test. The problem arises with the evidential status of higher-level claims: "Quantitative statements about social mobility are not objective but reflect a process of social construction". Of course, one can cash out such claims in specific instances (showing, for example, how the analysis of class was formerly constructed to mean the class of *male* workers – Acker, 1973) but doing that is much harder intellectual work and (in any event) doesn't prove the more glamorous claim that *all* statistical analysis is socially constructed. Indeed it isn't clear how such a sweeping claim *could* be proven. (As a practical example, I challenge the reader to identify a single empirically surprising finding in the article by Del Rosso, 2011.) Thus we find ourselves in the odd position that arguments against approaches based on evidence are themselves merely assertions. This displays encouraging consistency in the proponents of those views but puts them in a weak position. It is possible to support the claim that empirical relationships can be discovered empirically (simply by doing so). But it is not possible to support general claims about the absence of objectivity empirically. To do so would be contradictory. Given this, unless such higher-level claims make a real contribution to ground level sociology (the evidence in favor of social construction as an approach—rather than an empirical claim—for example is how much it finds out empirically that could not plausibly have been discovered otherwise), it seems that sociologists would be perfectly entitled to disregard them.

How is this bad for us? It may be that some really outstanding thinkers have managed to give clear and coherent accounts of such issues, but more and more sociologists give the impression that they think that scientific argument based on evidence isn't fundamental to what we do and that attempting even to strive for it is (in some unspecified sense) wrongheaded. They haven't proved it for themselves (and no such proof may be possible). It simply becomes a demoralizing thing that is widely believed. Why is it demoralizing? Because it means, potentially, that sociologists cannot do anything that anyone else could not do. I have definite skills (for example computer programming) and these skills are scarce and hard to acquire. I could probably sell them outside academia and I can certainly impart genuine expertise (given the chance) to students who do not have it. But if all sociology can offer is narratives why should anyone pay us to develop these, listen when we propound them, or take us seriously when we teach them? (To say that students are qualified to decide what they need/want to learn is like saying that patients are qualified to approve medical diagnoses. It is completely ludicrous except on the presumption that sociologists have no distinctive skills at all. Unfortunately, sociologists, by their choice of activities, may place themselves in a position where that is increasingly true.) Postmodernism and social construction might be intellectually sexy but their wider consequence (banal anti-scientism?) may be to turn the rest of sociology into valueless mush (not just as research but also, less obviously, as teaching): If any fool can teach it, only a worse fool will want to learn about it. Some of these problems are worsened by neoliberal folly in running universities. Letting students choose what to learn seems rather likely to move all but the most able away from challenging right or wrong areas like statistics and towards fields like social theory, where even professionals apparently cannot always tell sense from nonsense (Scruton, 2015; Sokal & Bricmont, 2013). Ironically, this pseudo consumer choice may actually harm less able students (as it would harm patients who didn't have the sense not to try and reject medical diagnoses they disliked). Under these institutional arrangements, such students can choose to render themselves practically unemployable with the connivance of academics and the government (which enforces spurious consumer choice).

I suggest we might all feel better if we spent no time on (and more importantly gave no credence to) activities that are the intellectual equivalent of sawing off the branch that you are sitting on. There are plenty of interesting and valuable challenges still to be met in collecting data, analyzing it and developing robust ideas from it (and critiquing those concrete ideas to improve them progressively) on the presumption that the whole exercise really means what it appears to mean.

Social Theory

One of the most problematic aspects of decluttering sociology is that the things that potentially do the most damage seem to be those that are least definitively critiqued or searchingly discussed. Far from being seen as any sort of problem, social theory still seems to be regarded in some quarters as the apogee of sociological achievement. (Like many of the points here, I am far from the first person to make these claims and novelty in detail—rather than synthesis—is not my aim: "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask: Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion."—David Hume, *An Enquiry Concerning Human Understanding*, 1748. However, it is interesting how little difference such claims seem to have made even after all this time. Perhaps my point that these beliefs will make you unhappy—rather than being wrong, which may involve a category mistake—has an element of novelty.) The first odd thing about social theory is how unusual the use of theory is in its name. Rational Choice Theory (for all its weaknesses) makes claims that are clear and provably right or wrong (for example in laboratory experiments—Güth, Schmittberger, & Schwarze, 1982). By contrast, social theory often seems to mean obscure general discussion of some topic. (The common-place lack of clarity raises other obvious problems: Without empirical research based on some sort of formal representation, how can you tell if your new theory isn't just an old theory with different terminology, or if your vague theory and that of someone else really differ substantively?) In extreme cases, it is hard to understand what has been claimed and even if that is clear, it is frequently unclear how (or even whether) such claims could ever be tested. Having a separate domain of social theory without any apparent expectation that its theories will be testable creates a perverse incentive structure which produces, for example, too much social theory relative to empirical research on the same topics: For example, consider the balance of empirical and theoretical work on structuration (an admittedly very useful concept in its simple form). Sociologists (and students) are often made to feel uncomfortable for thinking that some social theory may simply be empty nonsense and waste time trying to make sense of it. Those who imagine they have succeeded have arguably only had their critical faculties compromised. This is a sure fire recipe for fatigue. (Again, there is a connection here between different aspects of the decluttering process. If it weren't for anti-scientific assertions, nobody would even countenance the possibility that social theory could avoid having its content assessed empirically. In the same way, scientifically, the burden of proof would be on the social theorist to show that the theory meant something by what could practically be done with it. I can't think of any other discipline that endorses such a significant disjunction between theory and evidence.)

Social theory seems to exist in two broad forms and both, for slightly different reasons, could do with decluttering. Both forms are linked (as many other elements of this discussion are) to another aspect of decluttering, in this case anti-scientific assertions. There is a simple (but not disproven—merely disparaged in the style suggested by C. S. Lewis) scientific view that the boundary between science and non-science is the difference between hypothesis generation and hypothesis testing. It doesn't matter where ideas come from (sniffing glue, reading novels, walking in the park), they only become the province of science once they are suitable for testing. (It is interesting to reflect that someone who claimed to have good sociological ideas when drunk would understandably be treated with skepticism on the grounds that alcohol might well impair their judgment. But far too few sociologists seem at all skeptical about the ability of social theorists to say useful things about the real world while apparently keeping as far away from it as possible.) On this basis, social theory may be no more use than sniffing glue or reading novels as a source for ideas. It may actually be less use because of its apparent licensed disengagement from empirical testing. (Further problems may also arise. For example, social theory—rather than the disparaged reality—can all too easily just become food for more social theory since it is unlikely to become food for empirical research.) The other sort of social theory (which one might call framework building) is also problematic but in a slightly different way. It is probably useful to be aware of the basic insight of structuration theory (that agency can create structure that can constrain agency). However, without fairly rapid translation into empirical research, it is not clear how much value is added by further abstract discussion of the basic point.

One reason for decluttering social theory may be to increase the visibility of sociological objectives that are more lastingly desirable. For example, based on my expertise in Agent-Based Modeling (Chattoe-Brown, 2013) I suspect one of the reasons that structuration has remained largely theoretical (or at best a kind of loose organizing principle for analysis) is that existing research methods don't properly support it. Statistical analysis can't really represent structures and ethnographic analysis struggles to say how individual practices and interactions aggregate. Rather than debate the abstractions of structuration endlessly when existing empirical approaches permit no resolution, it may be more productive to spend time identifying and developing research methods (for example, laboratory experiments or Agent-Based Modeling) that can render these debates potentially empirical. (A similar argument can be applied to problems of causation and reification in statistics. What insight can other approaches, like qualitative research or Agent-Based Modeling, provide into why these problems arise and what—if anything—can be done about them?)

As with anti-scientific assertions, it would be inconsistent and therefore silly of me to say that social theory is wrong. (Part of the problem is precisely that as Wolfgang Pauli, the physicist, famously remarked: "It is not even wrong".) However, it *does* make sense to say it may be unproductive and thus not worthwhile for sociology to invest scarce communal resources in. (Sociologists seem to be surprisingly bad at thinking about certain kinds of wider social implications of what they do. Whatever we may think of social theory intellectually, we need to be aware of the possible effect it will have on potential students—particularly more technically able and critically alert ones, thus creating a potentially vicious circle—and on people in the real world who we may wish to take us seriously. We shouldn't assume that condoning sociology without content is just a communal lifestyle choice. It is also very dangerous, even if consoling, to see such external objections as merely politically motivated or as attacks on intellectual freedom.)

Sociological Advice

In a way, social theory can be seen as part of a wider class of sociological advice that involves telling other people how to do things (or what to do) rather than actually doing them. This is particularly visible in certain areas. For example there seems to be far more published *about* ethnography than there is ethnography. (Ethnography is difficult. Ethnographic advice is less so.) Obviously some teaching materials are needed but the proliferation of advice seems to exceed what could possibly be justified on these grounds. One hopes that pressure to publish is not so great as to make the proliferation of the easiest kinds of writing inevitable. In the same vein, there are always manifestoes urging that more research should be done on some topic. It seems rather likely that the best way to promote research in an area is to do some yourself and discover something interesting. It may also turn out that the absence of certain kinds of research does not just represent idleness or complacency but some genuine challenge in the topic that can only be discovered hands on. The temptation to tell other people what to do seems to bloom perpetually. One recent variant is the extensive discussion about the possible role of public sociology (Burawoy, 2004). For perspective, we only need to consider the role of public plumber, someone who is perhaps too busy discoursing on the wider significance of plumbing to actually do any. Again, there is a faint sense that sociology may be inadvertently revealing its own disarray. Why would we *need* to articulate a role for a field that studies what we study using the methods that we use? If it doesn't just *have* a role, something would already seem to have gone badly wrong. (Could it be precisely because we have stopped doing what is obviously useful and developed an overburden of precisely the kind of clutter outlined in the present article, that we have to cast around for a new role for sociology? Perhaps we should just get back to the role we had, sadder but wiser.)

Needless to say, it has crossed my mind that by writing this article, I might be accused of ignoring my own advice. In fact, this is not the case. I have formulated these ideas precisely by shaping my sociological practice in these ways and examining the effects. Only now am I trying to impart them to others.

Resistance to Novelty

Having had the good fortune to apply and develop a rather unusual research method (Agent-Based Modeling) and to have interests in other unpopular aspects of sociology (like functional explanation—see, for example, Chattoe-Brown 2006) I am surprised at how vigorously sociologists imply that they are seeking the new while reacting so badly when actually exposed to it. Apart from the standard subjective not sociology gambit already discussed, the new is reinterpreted to be just another spin on the old (Agent-Based Modeling is really just a kind of clunky statistics), criticized *ad hominem* (even being interested in functionalism is tantamount to being a Social Darwinist—boo hiss!) and so on. As someone who is scientifically passionate about these ideas, I long for really solid logical or empirical criticisms that actually oblige me to reconsider my position instead of great waves of "don't really want to know" or "can't really be bothered to understand". I have almost reached the point where when people start agreeing with me, I worry that I must have said something banal! It wouldn't be appropriate in an article of this kind to argue the pros and cons of Agent-Based Modeling or functionalism but in keeping with the decluttering approach, it is worth pointing out that if you too stridently and consistently reject the new, sooner or later you are going to get bored with the old. Although there has been much technical advance in areas like statistics over the last forty years, deep problems have not really changed: What is the causal status of a statistical association in complex social systems? The same can be said of ethnography: What is legitimate generalization and how can it be achieved? (Simply rejecting generalization would appear to be another anti-scientific assertion. What is the point of sociology that doesn't generalize? Isn't that just journalism? See Chattoe-Brown, 2013 for an example based discussion of these points.) I'm not encouraging you to get into my new thing (though I *would* advise you to understand it before rejecting it, as that is more energizing than just shrugging it off) but I would encourage you to get into *some* new thing that is difficult, challenging and empirical on a regular basis.

This counter-productive attitude also applies to the maintenance of sociology as a vibrant discipline. Given that we cannot really prevent new disciplines from forming and taking away core ideas from sociology, the only effective solution is to keep acquiring and developing new ideas or the result will inevitably be collapse. I am not sure that the production of empirically usable new concepts in sociology is as vigorous now as it was in the fifties and sixties. In fact, an additional reason for decluttering is that some of these elements (as well as external aspects) reinforce each other in unhealthy ways. The combination of pressure to publish and increasing teaching and administrative loads seems likely to push academics towards easier kinds of academic output (giving of advice, non empirical social theory, and so on). These kinds of work do not seem most likely to produce ideas substantive enough to create future research programs. The combined effect could easily be a shortage of the sort of ideas that will keep sociology vibrant even if the problem is worsened by external factors that we cannot control. In straitened times, we *really* need to concentrate on what will keep the discipline vigorous, not just for now but for tomorrow.

Ironically, analyses of this kind could be seen as typically sociological in reflecting on the complicated and sometimes counter-intuitive interplay of institutional and individual phenomena. What leads to individual short-term gains could simultaneously give rise to long-term collective disaster.

Publication

Although the institutional incentive structures are now almost exactly wrong, I think we would all be happier if we published less and spent more time carefully addressing and adding to what has already been written. The result would be more lasting novelty with progressive research being another scientific idea that seems to have suffered from banal anti-scientism in sociology. I think sociology as a whole would be more vigorous for more substantive critique, more careful peer reviewing, and less to read that cannot clearly state its contribution. (As already suggested, this is another area where different forms of cluttering gang up on us. Without the harm caused by anti-scientific assertions and the license that social theory apparently has to mean little, nobody would think twice about insisting that new publications have to rigorously justify their existence. Matters are further complicated by the economics of academic publishing. Predatory journals are obviously dishonest in claiming that they peer review when they don't, but there are so many journals and publishers now that even with peer review, it is not clear how standards can possibly be maintained. It would be interesting to know if there is any reason to think that the profit motive is compatible with the maintenance of academic quality control. If so how this market is supposed to regulate itself. I don't think I have ever heard a sociologist reflexively discuss the economics of academic publishing.) A little sociological reflection makes it clear why this is bad for us. It is a classic Tragedy of the Commons (Hardin, 1968). Everyone benefits individually by putting a few more cows (articles) on the common but we all bear the costs and, to stretch the metaphor perhaps a little far, we will end up (if we aren't careful) with an intellectual dustbowl. I had a fascinating experience recently when one of my colleagues pointed me in the direction of a truly awful journal article that had been published in a supposedly peer reviewed journal. The article made what was potentially a libelous claim about another journal on the basis of evidence that collapsed under any sort of scrutiny. In order to be clear exactly what was wrong with the article. I had to do a more careful analysis than I had perhaps ever done before, which was hugely satisfying intellectually. After many adventures, the rebuttal was finally published (Chattoe-Brown 2015) but not before a colleague had assured me (I think fortunately they may have been mistaken) that my article did not fit the definition of research by which UK academics are evaluated. The irony was delightful. You can publish empty nonsense in a peer-reviewed journal and it is called research. You can point out that someone has published empty nonsense and explain exactly what is wrong with it (to try and avoid future nonsense) and it isn't. I came very close to not being able to publish this article at all, but it is one of the things I am actually most proud of even if it isn't the most important thing I have ever written. I think this level of analysis and quality control would do everyone in the profession a power of good (both delivering it and worrying about being on the receiving end if they let their standards slip). We are all far too polite in public about tangibly bad research without being able to articulate clearly why this is appropriate. (Perhaps it is another harmful side effect of banal anti-scientism? To say someone else is definitely wrong, you have to have very robust reasons for thinking you are right. If all you have is a narrative, you better not challenge someone else unless they turn back on you.)

Political Apologia

On the face of it, this problem may appear to have solved itself but I'm actually not sure whether it has. Very few people now still wade through the kind of politically motivated (often Marxist) apologia that was produced as sociology in the sixties and seventies (see, for example, a good number of the thinkers discussed in Law and Lybeck, 2015). It is an interesting question how much harm this kind of sociology has done to the subsequent credibility of the subject (being potentially doubly objectionable as empty social theory with a clear political bias). Looking back, it seems extraordinary that sociologists thought that it was feasible to contribute to politics in this way. (It is far from pointless to think about institutional design and knowledge about the reactions of individuals and groups to different kinds of structuring conditions, but these aspects of sociology seem to be almost the exact opposite of those bought to bear on issues of political change. By its nature, Utopia is not a suitable subject for empirical research and even bringing such empirical research into the service of practical change is far from methodologically trivial.) The distinctions between means and ends and between what is empirically justified and what is politically expedient seem far too important to ignore. The problem we have now is not that this style of sociological theorizing has gone away but that the futility of its aims is (because it is so much nearer to us) so much less obvious. In thirty years, will all the general sociological railing about neoliberalism look just as futile? Economic theory has plenty of recognized technical weaknesses and it is clear that, following from these, markets have many harmful effects where they don't belong. However, if it turned out (as the ghost of Marx might whisper) that neoliberalism is mainly in the business of serving powerful interests, then simply analyzing its technical flaws (or even articulating how it serves those interests) is unlikely to do much good. If such a situation obtained, it would either be necessary to educate enough people on these matters to affect how they voted (a major project) and/or to figure out how to convince people with a lot of power to give up their justifications for it (also a major challenge).

The trade off between integrity and expedience is endemic in politics and it seems to rub off on academics, mainly to their detriment. It seems (regrettably) that nobody really expects politicians to be intellectually honest, but without such intellectual honesty, it is not clear what other contribution sociologists can really make. What price now The Third Way, The Big Society or the intellectual respectability of monetarism? (And these are just some of the gravestones in a cemetery that we can still read.) Again, whether or not anyone chooses to listen, a sociologist is on solid ground with evidence and argument, but almost invariably becomes as naïve as the person in the street (perhaps more naïve through leading a sheltered life) when it comes to big pronouncements on society. We should not fool ourselves that we can do what we cannot, because sooner or later we will be found out. We have more than enough to do (and more than enough value to add) with what we can *actually* achieve. (If this were not so, politicians and journalists would never get away with making claims that are obviously empirically false.)

Conclusions

Because this is an untypical piece of academic writing (deliberately polemical, not subject to peer review and not claiming originality for most of its assertions), I am understandably concerned that it may not amount to anything beyond a shopping list of grumbles.

In fact, to my slight surprise, the conclusion seems quite an interesting one. At first sight, I may seem to be putting forward a "back to basics" or turn "back the clock" view that is quite reactionary. Let's get rid of a highly institutionalized sociology with lots of social theory and go back to (critics might claim) the bad old days of naïve empiricism. Let's wish to get rid of the neoliberal world of the modern university and take forever to follow our noses wherever we will without interruption from students or those who pay our salaries.

But that clearly won't do. We already got bored with it and perhaps that is what led to an excessive anti-scientism in reaction. What we need instead is what one might call a "Back to the Future" agenda, rescuing what was good from the past that may have been obscured by unproductive later enthusiasms but, at the same time, thinking hard about what is needed to avoid those good things becoming "same old same old" as time goes on. Society will always need good statistical analysis but merely adding to technical sophistication is not enough. We must also (and this potentially requires much soul searching) find ways to own up to and tackle the deep problems (like endemic non-linearity and the relationship between association and causation). If social theory is typically not delivering what we need to understand regularities in society, how do we do theorizing differently so that it does? (I have suggested that social theory is largely unproductive but development of theory from both quantitative and qualitative research is also currently far from perfect. Why?) By naming approaches (Analytical Sociology, laboratory experimentation, Randomized Control Trials, Agent-Based Modeling, Social Network Analysis, big data), there is a danger that it will sound like I am just allying myself with a particular flavor of sociology. But my point is that there are plenty of research methods, theoretical perspectives and data sources out there to challenge us with the kind of real issues that make doing social theory, giving advice and policing artificial disciplinary boundaries seem rather insipid. I realize that, in my contributions to Agent-Based Modeling, I have developed the perspective that I have tried to articulate here. A different approach, with a different methodology, gives a new perspective (and perhaps even new solutions) to existing problems (both grand and practical). However, they *are* existing problems. We know a lot about crime (or education or class or religion) and it would be ludicrous to throw all that away simply to give the spurious impression of novelty. (The Internet has recently arrived but inequality resolutely remains.) Provably new methods and approaches, robust old topics: Back to the Future!

Finally, disagree with me, get cross, prove me wrong with evidence, do it in print where it leaves a mark. As long as you aren't personal, don't feel you have to be polite. We'll all feel better for it!

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Appendix 1

How to declutter sociology in seven bullet points:

- Sociology does not exist in any useful sense (but neither does psychology or economics).
- Social theory: Just say no!
- General arguments against the scientific method negate themselves. General arguments in favor of the scientific method do not.
- Don't advise: Do!
- If you spend too much time avoiding or rejecting the new, you will get bored with the old. If you don't, your audience may.
- With publication (as with many other things), less is often more.

- Sociology that panders to politicians of whatever flavor nearly always ends up tainted.

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At various points, Edmund Chattoe-Brown has studied chemistry, Artificial Intelligence, Politics, Philosophy, Economics and Sociology. That has cured him of the belief that any one discipline or research method has the monopoly on wisdom. His main interests are decision-making, Agent-Based Modelling and research methods but these have led him to other techniques (social network analysis, experiments, game theory) and research areas (farming, religion, drug use, theories of social change inspired by evolution, attitude change, budgeting and secondhand markets).

Correspondence concerning this article should be addressed to Edmund Chattoe-Brown, Department of Sociology, University of Leicester, University Road, Leicester, LE2 3BB, UK. E-mail: ecb18@le.ac.uk