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**The Perils of ‘Impact’ for Academic Social Science**

‘Impact’ has become a standard way of conceptualising the contributions that research makes to policymaking and practice, this reflecting the shift from a state-patronage to an investment model of research funding. However, little attention has been given to the metaphor that underpins the term ‘impact’. Yet this is important because there are ways in which it can distort our understanding of the contribution of academic research, and thereby create false expectations about it. Some problematic assumptions built into the metaphor are examined, along with several ways in which it may obscure our understanding of the relationship between research and policymaking or practice.

Keywords: impact; academic research and policymaking/practice; the public function of social science.

This savage novel [*The Jungle*] of the bestial conditions among the stockyards and slaughterhouses of Chicago in the early years of the twentieth century is perhaps the most influential and harrowing of all Upton Sinclair’s writings. [...] So great was the furore caused by the publication of this novel that the food laws of the United States were changed within six months (Back cover blurb, Lewis 1965)

There is an important sense in which all academic researchers want their work to have an impact. Most of us hope that what we do will be found of value by others, rather than remaining unknown, ignored or neglected. Usually, of course, what researchers crave goes beyond the sheer fact of their work being read and judged worthwhile: there is frequently a desire for social research to have beneficial effects on policy or practice, to serve a progressive cause, or even to transform society. Indeed, some see having such effects as the goal of social research, and therefore as the main criterion in terms of which it should be judged.¹

¹ This is sometimes presented as demanding ‘engagement’: that researchers must engage with policymakers or practitioners in order to try to ensure that their research has impact: see Gewirtz and Cribb 2008; also Hammersley 2008. Advocacy of some kinds of action research or intervention research represent even more radical versions of the idea, in which the research process itself must be transformed by the participation of those whose practice is being studied; indeed, it may be required that they carry out the research themselves.
Needless to say, the pressure for research to have ‘impact’ has not come only from researchers themselves. There have been recurrent demands from governments and other funders that research make a substantial contribution to policy and practice. And, in recent times, these demands have intensified, especially in many Western countries, for example in calls for it to contribute to evidence-based policymaking and practice (Reynolds and Trinder 2000). In large part, this reflects the shift from a state patronage to an investment model. Previously, academic research was generally treated as of value in itself; and, even though it was funded by the state, its pursuit and evaluation were seen as properly left under the control of researchers. By contrast, the investment model views research as only of value if it makes a direct and demonstrable contribution to policy or practice: in other words, there must be sufficient return on the ‘investment’. On this basis, what research produces is to be assessed in external terms by ‘users’, such as various sorts of policymaker or practitioner. Indeed, it has sometimes been suggested that decisions about what is to be investigated by academic researchers should be democratically-accountable and/or market-led, or at least judged technically in terms of what will maximise impact. Davies et al (2005a) comment that ‘knowledge of when and how funded research makes a difference should enable research funders to make better decisions about how and where they allocate research funds’ (p2). Effectively, this implies the incorporation, of academic research into the policymaking process. While it is common for there to be a qualifier in statements about the importance of research impact, such as ‘At the same time it is important that non-instrumental social and economic research is also valued and supported’ (Davies et al 2005b:2), these usually leave it unclear why research having no intended impact should be supported, and increasingly it will not be.

It is against the background of this shift in form of governance that

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2 For a history of these demands in the field of education, see Nisbet and Broadfoot 1980. A similar story could be told in most other areas. For a discussion of models of the relationship between research and practice, see Nutley et al 2007.

3 This is part of a broader shift in policy towards universities, see Collini 2012 and 2013, and some have claimed that it represents a secular trend in the character of research in post-industrial societies (Gibbons et al 1994, Gibbons 2000). My focus here is on the significance of impact for academic rather than for more practical forms of research (for this distinction, see Hammersley 2002:ch6). In fact, the investment model tends to reduce academic to practical research, or at least to require that it be dressed in its clothes. A small industry has developed concerned either with demonstrating the impact of social science (see the Campaign for Social Science website: http://campaignforsocialscience.org.uk/; Bastow et al 2014) or with advising researchers or policymakers about how they can increase the ‘impact’ of research (for examples, see the LSE Impact of Social Science Handbook, available at (accessed 19.11.13): http://blogs.lse.ac.uk/impactofsocialsciences/the-handbook/, Brown 2013). What is taken for granted in almost all of this literature is that academic research can be strategically managed so as to produce ‘impact’, and that it is desirable to attempt this.
the requirement that academic social research must have impact needs to be examined. And the starting point in this should be to look closely at exactly what the term ‘impact’ means. A metaphor is involved, and metaphors can mislead as well as illuminate. The analogy is, of course, a physical one: in the simplest terms it can be formulated on the model of one snooker ball hitting another – this has been taken as a paradigm case of causal relations generally, at least since Hume (1777:Sect.IV,Part I, paras 24–5). On this model, research is conceived as coming into contact with policymaking or practice and as a result changing its direction, in much the way that a cue ball can do this to an object ball.

There is nothing wrong with using physical analogies to understand social processes; or even with employing relatively simple models of this kind. They can be useful tools for thinking. Thus, the snooker ball analogy underlines the fact that research must have contact of some kind with policymaking or practice if it is to affect it; an obvious but nevertheless important point. Yet, with any metaphor the correspondence between the source model and the target situation to which it is being applied is necessarily only partial. It will not capture everything that is important, from all points of view, and could even distort our understanding of those aspects of the target situation that it does address.

Careful attention to these matters is required, then. And if we think of how the snooker ball model maps on to the idea of research having impact on policy or practice, it becomes clear that a number of assumptions built into it need to be highlighted, and questions asked about whether they apply to the case of research and its effects:

1. The model involves the cue ball being struck so as to hit the object ball. The first ball thereby has directionality and momentum: in other words, it has ‘force’. Is this true of research?
2. The object ball is stationary, and only moves as a result of direct and immediate contact by the cue ball. Is this true of policymaking or practice?
3. Contact between snooker balls operates under laws that predict repeatable patterns of action, given certain background conditions. Is this true of the relationship between research and policymaking?

Little attention has been given to this. For example, despite claiming to focus on ‘conceptual issues’, Davies et al 2005a do not examine the concept of impact itself. It is usually formulated in terms of the older game of billiards, but the same types of ball and table are used in both games. Of course, the fact that the reference is to a game indicates that the analogy is not an entirely physical one. For examination of a different metaphor – translation – that is frequently applied to the relationship between research and policymaking/practice, see Hammersley 2014.
or practice?

I will discuss each of these assumptions in turn.

**Pointing and pushing**

First of all, then, there is the question of whether the publication of research findings points, and also pushes, policy or practice in specific directions. It is certainly true that research reports often contain stated implications or recommendations for policy or practice. In this sense they give pointers. They may carry ‘rhetorical force’: in some circumstances the very publication of a particular set of research findings may itself stimulate change in policy or practice; or at least it may contribute to this, for example by providing a resource that can be used by some parties to press for change. All this is in line with the assumption, on the part of many researchers, that their findings indicate the need for political or practical change of some sort, and that it ought to carry weight in bringing this about. Similarly, when policymakers and practitioners look at research they will usually be concerned with what its implications are for their work, and may sometimes experience it as pushing them in one direction rather than another.

At the same time, there are important ways in which this aspect of the model can mislead. First, we must ask what the status is of the implications or recommendations found in research reports, and especially about their relationship to the factual findings that are the primary product of social science research. It is true that implications and recommendations are often presented by researchers as if these followed automatically from their factual findings or empirical discoveries. But, in fact, they never do: they only follow if a particular set of value assumptions is adopted, albeit ones that are often left implicit. Multiple, quite different, practical implications or recommendations can always be derived from the same body of factual findings, depending upon which set of values is used as a framework. So, any implications or recommendations put forward by researchers are conditional upon acceptance of the particular value assumptions they have adopted in order to formulate those recommendations, rather than being derived entirely from their research findings (Hammersley 2013:34-5). While, in a few contexts, for instance in some fields of medicine, the assumptions involved will be relatively uncontroversial, this is not true in most areas of social and economic policy. For example, evidence about poverty levels within a population, or even about the effects of a policy designed
to reduce poverty, does not, in itself, tell us what is wrong and what should be done.

This has important consequences for the relationship between researchers and policymakers/practitioners because the latter may well operate on the basis of assumptions that diverge from those employed by researchers, leading them to view the significance and/or implications of the research findings in a different manner. In other words, the direction in which policy or practice is actually ‘moved’ by the research, if any, will not necessarily be that indicated by the researcher’s recommendations. To capture this in terms of the physical model, we would need to think of the object ball having an in-built bias, this representing the particular values of policymakers or practitioners at the relevant time.

A second issue concerns whether research is or should be designed to bring about some specific kind of change in policy or practice, in the way that the aim in snooker is to hit the balls into the pockets. Many forms of human action clearly do have this teleological character. Examples would include coercion and manipulation. Here, we can think of the agent as aiming to bring about some effect as regards another person’s behaviour. However, most researchers, and most users of research, would not regard the task of research as equivalent to coercion or manipulation. A more acceptable parallel would be with treatments, interventions, or policies designed to resolve some problem or improve some situation. Policies and practices are frequently thought of as intended to have impact, and there are often investigations of what impact they have had. Much evaluation research is precisely concerned with this. However, most academic social research does not involve a treatment or intervention, though some applied research and research-and-development perhaps certainly does.

Another form of action having a teleological character is persuasion. Clearly, in seeking to persuade we are attempting to move others in some particular direction, but this time we do it – rather than through coercion, manipulation, or treatment – by making clear to people that there are good reasons for taking the action concerned. There is an important sense in which the communication of research findings is always an effort at persuasion. However, I suggest that the persuasion that academic researchers engage in when writing their research reports ought to be concerned solely with showing readers that their findings are true; and even this should be constrained by the collective enterprise in which they are involved – which ought to be devoted to discovering the truth about
what is being investigated (Hammersley 2011). The purpose of research reports should not be to persuade readers to engage in, or to desist from, a particular line of action – in the way that reports produced by interest groups and charities, quite properly, are. Moreover, this bar on persuasion aimed at bringing about action should even apply to the implications and recommendations presented in research reports, since for reasons already stated these are conditional in character – they depend upon the adoption of a set of value assumptions. These cannot be validated through the research, and so researchers cannot legitimately seek to promote one set of value assumptions against another (Hammersley 2014).

A third point is that the idea that there is, or should be, a directional relationship between research findings and the decisions of policymakers or practitioners assumes that the latter ought simply to adopt all research findings uncritically. But why should they accept the validity of the factual findings produced by researchers at face value? They ought surely to evaluate the validity of those findings, since all knowledge claims are fallible. Moreover, they usually have access to other evidence, albeit of varying quality. Once again, in terms of the physical analogy, this could well mean that the direction in which they move will not follow straightforwardly from the research findings.

The nature of policymaking and practice

Turning to the second assumption built into the analogy, it is by no means clear that policymaking and practice are stationary, in the way that the object ball is assumed to be in my formulation of the physical analogy; or in the manner that often seems to be implied when researchers declare that their aim is to bring about change, or to disrupt the status quo. Indeed, it can be argued that both policymaking and practice, in all their forms, are streams of activity that are ongoing and that are affected by a range of factors; very often adapting and changing all the time in response both to new demands and to recurrent problems. This means that any effect that research findings will have depends to a large degree upon the extent to which they are aligned with the prevailing ‘speed’ and ‘direction’ of the relevant form of policymaking and practice at the time, and with the preoccupations of policymakers and practitioners more broadly (Shove 1998). What is obscured here, above all, is that we are examining a relationship between two or more different forms of ongoing activity, not simply a moving ball and a stationary one.6

6 It has also often been noted that the speeds of these activities are very different; so that, even when
This underscores the fact that policymakers and practitioners will be selective both in whether they give attention to particular research reports or reviews of research literature, and in how they interpret and respond to them. As noted earlier, they will do this, in part, on the basis of a framework of value assumptions: about what are and are not matters of priority, what is and is not desirable, what is and is not worth the effort or how much effort it is worth, and so on. Closely intertwined with these will be factual assumptions, for example about what is and is not feasible and likely to be effective. And, generally speaking, they will treat all of these assumptions as more or less fixed parameters, as largely resistant to questioning and change until further notice – having found them conducive in the past to dealing with the sort of task that is faced and the circumstances in which it must be carried out, including the expectations of significant others. Furthermore, these assumptions will be closely entangled with commitments and interests of various kinds, political and economic.⁷

What this means is that there will be a strong tendency for policymakers and practitioners to assimilate research results to their working assumptions, and while research may be valued where it can be seen as confirming or complementing these, it will tend to be ignored, neglected, or reinterpreted where it does not seem relevant, or where it challenges basic assumptions on which they rely. In these circumstances it will be difficult to detect any impact. Of course, there may be occasions when some of these working assumptions come to be regarded as less secure, or even as open to question. Here, research may have greater impact: it may reinforce doubts and lead to change. But, even under these rare conditions, the change may still not be in the direction recommended; and, even if it is, it may still be judged not to go ‘far enough' or to go 'too far'.

In order to take account of all this, my original formulation of the

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⁷ While this argument has often been formulated in terms of the concept of ideology (see, for example, Weiss 1983), there are dangers with use of that term: it carries a great deal of baggage, much of it pejorative. It tends to suggest that these assumptions are simply a matter of irrational commitment, whereas they are based on experience and are open to reasonable deliberation even though there may be no single correct answer to the questions they address. For this argument as it applies to justice, see Sen 2009:xvii-xviii. Furthermore, in my view the fact that policymakers and practitioners rely upon a largely fixed framework of assumptions is as it should be, or at least it is unavoidable. This is not, of course, to say that the particular assumptions adopted by particular policymakers or practitioners at particular times are justified, indeed they may often be seriously misconceived.
physical analogy could of course be altered, so that the object ball is now already in motion before the cue ball is hit (as in some sorts of trick shot performed by expert snooker players). However, we should note that it is now rather less likely that the cue ball will hit the other ball. Moreover, even if there is contact, the effect will depend heavily upon the prior direction and speed of the object ball. Applying this to the case of the impact of research, it now appears to be an extremely uncertain, and perhaps even random, process as regards whether there is any contact, and, if there is, what character this will have, and what the consequences will be. Of course, like snooker players, researchers can attempt to play ‘trick shots’ so as to have ‘impact’. For example, they may ‘spin’ their findings in order to attract the attention of policymakers or practitioners, and engage in policy discussions designed to nudge them in particular directions. However, it should be remembered that trick shots in snooker usually require very careful setting up for them to come off, and researchers operate in a very unstable environment in their dealings with policymakers and practitioners, and have much less control over the situation. More important still, for reasons I have already explained, such spinning of findings amounts to a breach of researchers’ obligation to be exclusively concerned with producing value-relevant knowledge – this being what distinguishes them from policy entrepreneurs, public intellectuals, advocates, and propagandists.

The nature of the relationship

It should be clear from the discussion so far that there are significant differences between the source model and the target situation when the concept of impact is applied to the relationship between research and policymaking or practice. Contact between snooker balls on a flat surface under normal circumstances operates according to fixed and relatively simple laws, allowing for considerable (though not complete) predictability. The same is unlikely ever to be true of the relationship between research and policymaking/practice. This is because the nature of the contact – in terms of whether it happens, if it does what form it takes, and what the consequences will be – is much more contingent than in the case of the original model: many factors are involved that vary over time, so that the relationships amongst them are probabilistic at best, and most of the variables are not under the control of researchers (nor should they be).

8 This will, of course, vary somewhat between researchers. But while some have public reputations and social connections that give them considerable influence with policymakers, most do not.
There are several points here, then. One is that the scope for researchers directing their work so as successfully to impact on policy or practice is much more limited than the metaphor implies. In other words, getting research to have ‘impact’ is even harder than potting snooker balls! Indeed, just predicting the future impact of particular studies, or of a whole body of work in a field, will usually be very difficult. This severely threatens any efforts on the part of researchers to meet the demands that are increasingly being placed upon them to specify the likely impact of their work in advance, to bring about that ‘impact’, and to document it afterwards. In short, it is very unlikely that they can operate in a way that matches the requirements of the investment model, even though that model increasingly serves as the basis for public accountability of university research, this now being treated (with questionable justification) as analogous to a public service.

More importantly, for researchers to attempt to do this amounts to a breach of their ethical obligations. What this indicates is that some of the mismatch between the source model of impact and its application to the case of research and policymaking/practice stems from the proper commitments of researchers and of policymakers or practitioners. So the problems are not produced simply by the fact that the target situation is more complex than the model, or that either side of the relationship has made insufficient attempt to ‘engage’. Any attempt to construct the relationship between researchers and policymakers/practitioners on the metaphor of physical impact would require a distortion of the proper commitments of those on one or both sides of that relationship. And, in practice, this is likely to impact most on academic researchers rather than on the other parties. Moreover, the pressure on them goes beyond shifting their work towards a more policy- or practice-focused approach. In effect, it amounts to requiring that they serve a pedagogic or political function: advising policymakers and practitioners on how to bring about ‘good practice’; promoting particular political or practical goals; or simply trying to satisfy the demands of policymakers or practitioners for information. This undercuts not just academic research but even independent forms of practical inquiry: both are reduced to serving some cause or some set of policymakers or practitioners. And, given the increasingly marketised character of research funding, this is likely to mean researchers working for whoever has the funds to buy their

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9 For an outline of some of the issues here, albeit encased in what is in my view an overly-optimistic assessment of the prospect of measuring and predicting ‘impact’, see Davies et al 2005a.
10 However, policymakers have sometimes sought to use research as a means of control over professional practitioners, furthering a process of de-professionalisation: Hammersley 2013:18-20. This indicates that the consequences for practitioners may also be significant.
In my discussion to this point I have taken the impact model largely at face value, but current accountability procedures operating on research involve only a weakened form of this notion. Rather than ‘impact’ being defined as bringing about actual change in policy/practice, various proxy measures have been adopted, and treated as substitutes: that the research can be shown to have implications for current policy or practice; that it has been published in journals with high citation rates; that research reports have been specifically directed at policymakers or practitioners; that testimonials have been obtained from such ‘users’; that the research findings have been mentioned in policy documents; and so on. While this weakening of the meaning of ‘impact’ eases the demands on academic researchers by comparison with the requirement that their work be judged in terms of its actual impact, it still represents a very significant pressure towards assessing academic research in terms of its practical implications and consequences, when these should be seen as incidental to the pursuit of academic research. Furthermore, as with other recent reforms in the organization of public services, the investment model also requires that all parties to the relationship act on the basis of a myth: in this case the pretence that the relationship between research and policymaking/practice has the character that the impact model assumes, when, in many respects, it does not – as we have seen.

The function of research

Up to now I have focused on three aspects of the physical analogy underlying the notion of research impact, showing that, while there are parallels, there are some important respects in which it does not match closely the relationship between academic research and policymaking/practice, and that attempting to make it fit involves distortion. In this section I want to examine a few respects in which the model omits or obscures aspects of this relationship that are crucial.

First, the notion of impact implies change: in terms of the metaphor I have used, contact with the cue ball produces movement on the part of the object ball. While this fits with a prevailing public policy rhetoric about the continual need for innovation and change, we should not accept it at face value; certainly not the idea that change is always needed and desirable. The obverse of this point is that research can be of value in ways that do not entail policy change. Findings can provide a useful service by confirming existing beliefs where these are not already well-
established, or where they have come under challenge. In such cases, no change in policy or practice may result. Research can also stimulate worthwhile reflection or further consultation without this necessarily resulting in any actual change of policy or practice – it may lead simply to increased awareness of significant costs and dangers.

It is also important to note that the impact model implies a relatively immediate effect – the motion of one ball is transferred directly to, and displayed in the movement of, the other. Yet, arguably, most causal processes in social life do not involve immediate contact and change of this kind: longer, more complex and indirect causal processes are more common (Hage and Meeker 1988; Abbott 2001). Furthermore, investigations of how research has influenced policymaking show that what is almost always involved is the second kind of causal process rather than the first: research is found generally to affect policymaking and practice in much longer-term and more indirect ways than the impact model implies. And, as a result, its effects are a great deal harder to detect than the word ‘impact’ suggests (Weiss 1980; Gabbay and Le May 2004).

A further aspect of the relationship between research and policymaking/practice that the model obscures is that we are presumably not interested just in whether research has an effect on policymaking or practice but more importantly in whether it has a desirable effect. If it were to have undesirable effects, then maximizing impact would itself be undesirable. While it is common for social scientists and others to assume that the impact of research will always be beneficial, there is no sound basis for this assumption. Crude interpretations of Enlightenment thought that treat progress in knowledge as always leading to social progress are fallacious. Indeed, the relationship between the truth of empirical findings and the desirability of any policies that these are used to formulate, justify, or implement is to a large extent contingent. As a result, increasing the impact of research may have undesirable rather than (or, usually, as well as) desirable consequences. Moreover, where this occurs it does not mean that the research was poorly conducted or its conclusions invalid.

A detour into academic impact

Even if the idea that academic research should be judged in terms of its impact on policymaking and practice is rejected, it could still be argued that studies ought to be judged according to their impact on the development of knowledge in the field to which they were designed to
contribute. Does the impact metaphor apply better here? The answer to this, I suggest, is yes and no. Many of the problems of alignment are reduced. There is, or should be, more likelihood that the researcher and her or his colleagues are ‘moving in the same direction’, or at least are concerned with the same task. Moreover, even where what is produced by a study is at odds with prevailing views, there is an in-principle obligation built into the academic world requiring that deviant views be given attention and properly considered. After all, much of the rationale for the academy rests on the idea that it is open to new and critical ideas.\textsuperscript{11} For these reasons, there should be less severe conflict between what any researcher intends and the interests, preoccupations and activities of an audience comprised of other researchers in the field.

Even here, though, we need to be cautious about the metaphor. It is not the case that value is only added in academic terms when a contribution leads to a change in the prevailing views, nor that the bigger the contribution to change the better. What is and is not desirable change in academic knowledge, as with policymaking or practice, is a matter for evaluation. Moreover, here too any effects will often occur over the longer term rather than immediately. The value of some of the most important studies was not recognised immediately, and sometimes not until after a very considerable period of time.

There is a further problem with treating researchers as accountable in terms of the academic impact of their work. This is to assume that academic social science research communities currently operate in ways that closely approximate to how they \textit{ought to} operate. However, there are good reasons to think that this is not true (Hammersley 2005 and 2011:ch7).\textsuperscript{12} To the extent that this is so, the academic impact of any particular study will tell us little about its value.

More fundamentally, there are disagreements among researchers about what contribution studies can or should make to the collective enterprise. The traditional scientific model treats the aim as the cumulation of knowledge, with productive studies ‘adding bricks to the wall’. However, there has long been recognition that the growth of knowledge, even in the natural sciences, does not match this image very closely. Kuhn’s (1970) famous account of the discontinuous development of natural scientific knowledge offers a rather different picture; though

\textsuperscript{11} This is, of course, a matter of degree: see Polanyi 1958.

\textsuperscript{12} Needless to say, there is a similar divergence from the ideal as regards much policymaking and practice today. This is a further reason why the impact of research in those fields is not a good measure of its value.
we should note that during periods of ‘normal science’ what occurs still approximates to the traditional model. However, some social researchers, especially those committed to qualitative methods, reject not only the traditional model but also this Kuhnian alternative. For example, there are those who believe that a more creative process is involved, in which particular studies have a similar status to works of art: standing alone, to a large extent, as exemplars. Here, rather than the task being to create a growing body of reliable knowledge, the aim instead is to provide illuminating views that will enable readers to refine their own perceptions and reflections on human social life (Eisenhart 1998; Hammersley 2013: ch10).

Finally, it is important to note that in most fields of social research there is, in practice, little sign of the long-term development of knowledge. Instead there are recurrent changes that seem closer to the vagaries of fashion. Indeed, they amount to a kind of caricature of Kuhn’s account: all ‘scientific revolution’ and no ‘normal science’. Moreover, this is true even where there is at least nominal commitment to the traditional model (see Rule 1997).

For these various reasons, then, the value of research cannot be judged properly even by its academic ‘impact’. Here, too, the notion of impact represents a misleading short-cut in the process of evaluating the value of particular studies or of whole bodies of work.

**Conclusion**

In this paper I have raised some questions about the currently influential idea that academic social research ought to be judged in terms of its impact, and indeed that researchers should take active steps to maximise that impact. This requirement has, of course, increasingly been built into funding criteria and into the strategic management of research within universities.

I identified several ways in which the physical model on which the impact metaphor relies does not correspond well to the relationship between research and policymaking/practice, either misrepresenting or omitting key features of it. Moreover, pressure on researchers to try to ensure that their work has an impact involves a distortion of the process of academic research. Indeed, I argued that this is true even when the notion of impact is diluted, or where it is restricted to ‘academic impact’.
In the course of writing about ‘the impact of impact’, Fielding (2003:292) draws a parallel with another term that has come to be embedded in the contemporary discourse of policy and practice: ‘delivery’. He writes:

Despite the occasional twitch of unease that survives the anaesthetizing of our educational sensibilities, we still talk about ‘delivering’—the curriculum, subjects, learning—and much else besides. And yet we know when we reflect for a moment and step back from the busyness of our work and the bruising of our imagination that to talk about ‘delivering’ these things is utter nonsense; we know with our hearts that it is a deep betrayal of education in even the most minimal sense.

Much the same might be said about ‘impact’ and its relationship to academic research. Recognising the dangers associated with this metaphor is very important, even though the metaphor itself is, of course, not the driving force behind the shift to an investment model: powerful socio-economic forces are in play (see Crouch 2011).

Needless to say, to question the conceptual basis for the idea that academic research has impact – to highlight the fact that how social research findings are likely to operate in relation to policymaking and practice cannot conform to what the metaphor requires – may be seen by many social scientists as impolitic at the present time. It risks simultaneously undermining both lay expectations about the contribution that research can make and the efforts of researchers to sustain a case for continued funding in the present financially austere climate. There is the saving grace, however, that this piece of writing is no more likely to have a direct and major impact than is any piece of social science research. Nevertheless, I hope that it will prompt further thought about the costs of the political game in which we are all currently embroiled as researchers.

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Draft: 3.2.14