



Evaluating Realist Evaluation: A response to Pawson's reply

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Evaluating Realist Evaluation: A response to Pawson's reply

I am very grateful for this opportunity to respond to Ray Pawson's (2016a) reply to my immanent critique of realist evaluation (Porter 2015a). To aid comparison, my response follows the headings and structure of Pawson's reply.

Let me start on a positive note of unambiguous agreement. I entirely concur with Pawson's position in the first section of his reply that research methodology involves a reciprocal relationship between principles and practice. However ...

The main thrust of his argument in this section involves an attempt to contrast the grounded methodologist rooted in the practicalities of research (Pawson) with the rootless theorist obsessed with disconnected doctrines (Porter). Unsurprisingly, I do not accept this characterisation.

As a general observation, all research, whether explicitly or implicitly, is founded on ontological, epistemological and axiological assumptions. If those assumptions are confused or contradictory, then this is likely to have deleterious consequences for the practical conduct of research. Theoretical efforts to clear confusions and contradictions are therefore a legitimate component of the research process.

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3 More specifically, while I respect Pawson's aspiration to drive realism into
4 research practice (Pawson and Tilley, 1997), in his efforts to do so he has
5 never shied away from pontificating on matters philosophical. I assume this is
6 because he accepts that Realist Evaluation (RE) requires a cogent theoretical
7 foundation. But whatever his motivation, the fact remains that he has
8 frequently presented readers with ontological, epistemological and axiological
9 arguments, so he can hardly complain when they are subjected to scrutiny.
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21 Conversely, his assertion that 'not a jot, not an iota of the empirical work is
22 discernable within Porter's fragmentary "deconstructions"' (134) is simply
23 false. Each of the three main sections of my paper includes an illustrative
24 discussion of empirical RE that I have engaged in. His accusation that my
25 critique fails to interweave principle and practice has no merit.
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34 Pawson's attempt to rule my arguments out of court because of the terrain I
35 place them on is a dangerous ploy. As he observes, 'an obstinate method
36 which fails to learn will ossify or die' (133). May I suggest that, in his
37 reluctance to countenance critical analysis of RE because it addresses
38 ontological and axiological issues, it is Pawson rather than me who is
39 displaying obstinacy? As will be seen, his disdain for my supposed pretence to
40 philosophical perfection, combined with his persistent tendency to use
41 argumentative gambits of dubious pertinence, means that he rarely engages
42 directly with the substance of my arguments. This is unfortunate because it is
43 through such engagement that we can all learn.
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Instrumentalism?

This section begins with Pawson complaining that in citing his use of the value-laden terms 'poor performance' and 'inappropriate behaviour' in his explanation of the origins of interventions, I 'do not even bother to mention that the research in question [from whence he states these terms are taken] is a review of so-called naming-and-shaming programmes' (134). The reason why I didn't mention it is not because I couldn't be bothered but because there is absolutely no indication in his text that this is the origin of his statement, not even a citation. All there is is a bald didactic pronouncement that I treated as such.

In response, he argues that the use of ostensive definition in his invisible review means that my 'stricture on conceptual exactitude misses the point' (134). I have no idea what stricture on conceptual exactitude he is talking about. I wasn't concerned with the exactness or otherwise of these concepts. What interested me was their expression of values and how researchers might choose to approach those values.

He goes on to provide a clear explanation of the contingencies that characterise the causal chains involved in policy initiatives, concluding that 'outcomes, by and large, are at variance with policy expectation. In short there are no givens here' (134). I am happy to acknowledge the important insight that the outcomes resulting from interventions should never be taken as givens, but those were not the 'givens' that my argument related to. Instead, I was pointing to the danger of researchers taking the value judgements of

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3 policy makers ('poor performance', 'inappropriate behaviour') as givens, and
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5 therefore not subjecting them to critical analysis. While internally cogent, his
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7 argument is beside the point.
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10 11 12 13 14 **Facts?**

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16 Pawson takes me to task for suggesting an affinity between his position and
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18 Weber's (1949) notion of social scientific value freedom 'whereby the scientist
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20 is mandated to take an objective, value-neutral approach to her subjects'
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22 (Porter 2015a: 248). Pawson counters this by pointing out that he has never
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24 cited and does not accept Weber's views on objectivity. But I didn't assert that
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26 he accepted Weber's position, simply that there was an affinity between the
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28 two positions. What is odd is that Pawson then goes on to confirm that affinity:
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30 'As a matter of fact, I do believe that research should cherish the aspiration to
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32 be objective' (135).
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39 It gets odder. In support of his contention that 'objectivity does not reside in
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41 the search for facts' (135), he uses an example that assumes three facts:
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46 A job creation scheme may be regarded as a 'success' as measured
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48 by significant recruitment from the unemployed, or as a 'failure'
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50 because the jobs created are largely part-time and short-term (135).
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54 That jobs are created and that most of them are part-time and short-term are
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56 all presented as empirically established facts. Of course, the quotation also
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3 indicates that their interpretation can differ widely. That's why the remit of
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5 social science includes both measurement and interpretation, and why any
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7 claim to explanatory power has to take account of both. Given these truisms,
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9 it is perplexing that Pawson has committed himself to this self-contradiction,
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11 whereby on the one hand he discounts facts, and on the other he counts
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13 them.
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18 Taking a more sensible tack, he continues by noting that observation is
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20 always theory-laden (though not determined by theory); that data always
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22 require interpretation; and that the pursuit of objectivity is a social process
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24 (Pawson's preferred process being one that relies on the distinctly
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26 Machiavellian attributes of distrust combined with ambition). His argument is
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28 that had I been sufficiently well-read to be aware of these points, I would not
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30 have accused him of having an affinity with Weber. But this contention
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32 depends on the oxymoronic assumption that the founder of interpretive
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34 sociology didn't accept that observation is theory-laden, or that data require
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36 interpretation, or that science is a social activity. It falls on all three counts.
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42 43 **Values?**

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45 Pawson starts this section with a vigorous condemnation of emancipatory
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47 approaches which he characterises as deciding *a priori* what the world ought
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49 to be and then criticising those who depart from this view as deceitful. He
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51 uses Bhaskar's (1979) adoption of the Marxist concept of false consciousness
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53 as an alleged example of this.
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3 I happen to agree with Bhaskar's emancipatory approach, and elsewhere
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5 have explained why (Porter, 2015b). However, I didn't make any such
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7 argument in my immanent critique. Being obliged to use my limited wordage
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9 here to deal with at least vaguely pertinent issues, I do not have the space to
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11 tackle this red herring comprehensively. This is unfortunate because Pawson
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13 (2016b) made the same argument in his reply to my paper defending critical
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15 realism (2015b), to which, despite requests to the editor of *Evaluation*, I have
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17 not been afforded the opportunity to respond. I mention this so that readers
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19 can appreciate that my silence in that journal is not because I have no
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21 arguments to make, but because I have not been allowed to make them.
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28 The only comment on this distraction that I have room to make here is to
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30 scotch Pawson's implication that I regard those engaging in RE research as
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32 somehow duplicitous (that would be hypocritical to say the least, given that I
33
34 have engaged in RE myself). My concern is that if an evaluation method does
35
36 not include a critical stance towards the values of policy makers, there is a
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38 danger that, depending on those values and the outcomes of the interventions
39
40 based on them, there may be negative consequences for people that are not
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42 picked up by the evaluators. Indeed, as I pointed out in one of my (non-
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44 existent, according to Pawson) empirical examples, I have fallen into this trap
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46 in my time.
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52 The values argument in my immanent critique was based not on Bhaskar, but
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54 on Andrew Sayer's (2011) thesis that all of us have concerns about our
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56 flourishing or suffering. These concerns result from a combination of
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3 observation, reasoning and values. Sayer argues that factual statements that
4 describe objective needs or lacks inherently contain the inference that there is
5 merit in responding to alleviate them. I also included Sayer's qualification that
6 this process does not automatically mandate a particular response. Pawson
7 takes this qualification, quotes it at length, and presents it as my prime
8 argument, thus relieving himself of the obligation to confront the main issue.
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18 He moves on to provide an example of how RE deals with values – his review
19 of Megan's Law. He tells us two things about the outworking of that law. First,
20 that it resulted in unintended consequences that varied according to the
21 contexts into which it was introduced. Second, that different people viewed it
22 in different ways. We have been here before, so I will repeat my acceptance
23 that causation is rarely linear and subjective perspectives rarely homogenous.
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32 But once again, I have to question the pertinence of these insights.
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36 Because things are complicated and subject to disagreement, we have to be
37 very careful about how we respond to them, hence my inclusion of Sayer's
38 codicil. However, the fact that life is complicated does not warrant us ignoring
39 avoidable suffering, hence my inclusion of Sayer's main argument. Pawson's
40 identification of complexity and perspectival interpretation does not directly
41 address the issue of researchers' values, and does not amount to a good
42 reason for abandoning evaluators' responsibilities to take people's concerns
43 into account.
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3 Maybe being a nursing scholar adds to the degree to which I am sensitised to
4 this issue, in that a pretty much universally shared assumption in nursing
5 research and practice is that the avoidance of avoidable suffering of patients
6 is a paramount value position. In other human sciences, matters may be less
7 clear-cut, but that does not obviate the need to address the same principles.
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16 **Change?**

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18 Pawson charges me with misrepresenting him by asserting that RE elides
19 social structure and agency in a manner that is similar to, and therefore
20 shares similar problems with, Anthony Giddens' (1984) structuration theory. I
21 do not accept the charge of misrepresentation. As evidence, I will cite seven
22 lines of Pawson and Tilley's *Realistic Evaluation* (1997) which contain no less
23 than three elisions of structure and agency under the rubric of social
24 mechanisms, each one italicised by the authors to emphasise their
25 importance. Had space allowed, I could have included a further three
26 examples from the same page, as well as direct approbation of the
27 structurationist model elsewhere in their book (1997:56).
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43 Social mechanisms are thus about people's *choices* and the *capacities*
44 they derive from group membership. We find the same combination of
45 *agency* and *structure* employed generally across sociological
46 explanation and we thus suppose that the evaluation of social
47 programs will deploy identical explanatory forms, reaching 'down' to the
48 layers of individual *reasoning* ... and 'up' to the collective *resources* on
49 offer (1997:66).
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5 Moving on, I am gratified that Pawson commends my modification of the CMO
6 causal configuration as potentially valuable, and hope that it lives up to that
7 potential. However, he attaches three reservations to his commendation. The
8 first is that some of my proposals are already contained in his model. In
9 particular, he states that in the RE model 'it is abundantly clear that contexts
10 (C) not only pre-exist interventions but they also have causal powers' (137). In
11 places it is indeed clear, and I pointed this out in my paper. My problem with
12 RE is that in other places a very different conception of contexts is posited,
13 which sees them as passive sets of circumstances rather than combinations
14 of causal mechanisms. It is this contradiction that led me to propose the less
15 ambiguous category of contextual mechanisms (CM).
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32 I concur with Pawson's second reservation that using formulae runs the risk of
33 encouraging overly mechanical interpretations. My excuse for doing so is that
34 I was responding to the pre-existing categorical construct of context +
35 mechanism = outcome which, for good or ill, has become the signature motif
36 of RE.
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45 His third reservation is based on the observation that the evaluator's task is to
46 address the particular dynamics of the specific social situation being
47 researched, a task that requires flexibility and imagination. I agree. But he
48 then states that 'no amount of conceptual musing can help in this, the vital
49 task' (137). The smokescreen of sarcasm that follows this statement fails to
50 hide its weakness. If he had said that no amount of conceptual musing can
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3 *replace* this vital task, I would wholeheartedly agree with him. But I simply
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5 cannot accept that researchers, in their efforts to get to grips with concrete
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7 instances of the social world, can gain nothing from an appreciation of the
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9 kinds of processes they are addressing, the kinds of knowledge that can be
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11 gained about them, the best approaches to gain that knowledge, or indeed the
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13 purpose and consequences of their investigations. The upshot of taking
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15 Pawson's position seriously would be, at best, a radically abstracted
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17 empiricism (cf. Mills, 1959). Nor can I understand why a man who has spent a
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19 goodly part of his professional life musing on concepts such as contexts,
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21 mechanisms and outcomes with a view to helping researchers should
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23 undermine his own *raison d'être* in such a cavalier fashion.
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30 But, according to Pawson, my sins are greater still. Not only do I muse on
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32 these unhelpful concepts, I also believe in their sanctity. At this point, I have to
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34 say that I am tiring of Pawson's *ad hominem* obfuscation, so let me make my
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36 position clear. What we have in RE is a methodological strategy for evaluation
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38 research that is backed up by ontological, epistemological and axiological
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40 positions. I believe that there are problems with some of those positions, and
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42 that some of those problems have posed difficulties for researchers. In my
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44 paper, I pointed them out, gave reasons why I thought they were problematic,
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46 and provided alternatives that I hoped would help resolve them. That's it and
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48 that's all. It isn't about 'metaphysical ordinances' (133); it isn't about
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50 Pecksniffian philosophy (134); it isn't about 'conceptual sanctity' (137); and, to
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52 anticipate his accusation in the next section, it isn't about 'slaying all
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54 philosophical opposition to perfect an ontology and epistemology' (138). It is
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3 simply a debate about the relative merits of two realist approaches to
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5 evaluation research; a debate that I had hoped would be conducted according
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7 to pertinent logic and evidence rather than circumvention and hyperbole.
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10 11 **Realism?**

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13 A large proportion of this section involves Pawson listing off a considerable
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15 number of the various realisms that are out there, and then criticising me for
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17 not including them in my discussion. In response, I wish to repeat that my
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19 interpretation of this dialogue is that it is about the relative merits of two
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21 approaches to realist research – Pawson’s and mine. If this is so, nothing
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23 would be gained from pouring the likes of Platonic or Scottish Common Sense
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25 Realism into the mix. I am happy to accept that Pawson came to his approach
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27 through a process of erudite eclecticism that fused various strands of realism
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29 into the RE version. But it is the product of that fusion that I am addressing,
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31 not its antecedence. I therefore make no apology for not mentioning a
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33 ‘panoply of realisms’ (138) in my account. Nor do I apologise for uncovering
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35 the specific confusions and contradictions embedded in the RE approach.
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43 In Pawson’s final remarks, we are back once again to the parodic contrast
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45 between his pragmatism and my absolutism. I concur with his observation
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47 that:
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52 Wise researchers begin with a broad attachment to a paradigm, select a
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54 subset of protocols most pertinent to the scope of their enquiry and then
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3 translate them into a research design to fit the problem under investigation
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5 (Pawson, 2016a: 138-9).
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10 But once again, I have to object to his caricature of my supposedly contrary
11 position. This time his accusation is that, in contrast to the sensible and
12 modest approach quoted above, my quest is for 'realist purity' (139). Let me
13 repeat, I have no interest in the patently futile task of trying to create some
14 sort of metatheoretical apotheosis from which no deviation is permitted and
15 beyond which no improvement is required. Like Pawson, I accept that
16 researchers have to cut their methodological coat according to their
17 investigative cloth; like Pawson, I take epistemological labour to be
18 evolutionary. I locate this dialogue in that evolutionary process.
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32 The important issue is not my irritation with the accusation that I hold a
33 ridiculous belief in the incorrigibility of a social theory, but that acceptance of
34 such an accusation closes down the debate. In contrast, I want to keep the
35 debate open, and recognise the importance of others interrogating my claims
36 to test the degree to which they are sustainable. I can think of at least four
37 important questions that might be asked: To what extent are my claims about
38 the inconsistencies and contradictions of the RE model justified? If there are
39 inconsistencies, is there a risk that they will have a detrimental effect on the
40 prosecution of practical research? Are the alternatives I pose likely to improve
41 matters? Are there better ways to deal with the issues I raise? On the
42 answers to these questions my arguments stand or fall.
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3 My disappointment with Pawson's reply is that, for the most part, he chose not
4 to take this sort of approach. Rather than concentrating directly on the merits
5 or demerits of my arguments, he decided to take the route of diversion,
6 conjecture and disparaging imputation. In turn, rather than getting to the nub
7 of the matter, I have had to use my response to point out the diversions,
8 refute the conjectures and rebuff the disparagements. Pawson's avoidance of
9 issues of substance means that the dialogue between us hasn't really moved
10 the debate on very much. Unless, that is, we make the reasonable inference
11 that his substantive silence gives grudging consent.
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