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Editors

EPSA15 Selected Papers

The 5th conference of the European
Philosophy of Science Association
in Düsseldorf

Chapter 2

Case Studies, Selective Realism, and Historical Evidence

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Abstract Case studies of science concerning the interpretation of specific theories and the nature of theory change over time are often presented as evidence for or against forms of selective realism: versions of scientific realism that advocate belief in connection with certain components of theories as opposed to their content as a whole. I consider the question of how probative case studies can be in this sphere, focusing on two prominent examples of selectivity: explanationist realism, which identifies realist commitment with components of theories that are putatively required to explain their empirical success; and entity realism, which identifies realist commitment with certain putatively causally efficacious entities. I argue that while case studies are essential to debates about these positions, they are not compelling in the way that their intended use suggests. Regarding explanationism, concerns about the “neutrality” of historical evidence are ultimately infeasible. Regarding entity realism, arguments for and against naturally dissolve into disputes about the reference of theoretical terms which are insulated from the details of cases. I conclude by suggesting that the morals of this discussion extend to other forms of selective realism, namely structural realism and semirealism.

Keywords Selective realism • Historical evidence • Explanationism • Entity Realism • Theory change

2.1 The Role of Cases in Debates About Selective Realism

One common formulation of the idea of scientific realism is that it is the view that our best scientific theories correctly describe both observable and unobservable aspects of a mind-independent world. This view is, of course, controversial, and subject to a number of precisifications and qualifications that are themselves controversial – criteria for identifying theories that merit a realist interpretation, for admitting theories that yield descriptions that are sufficiently close to the truth

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even if not completely true, and so on. This essay focuses on one precisification and the relevance of case studies of science to assessing it. What is the proper unit of analysis for a realist epistemology of science? The most popular answer to this question in recent times is that scientific theories are, in fact, the wrong unit of analysis. Instead, most contemporary forms of scientific realism (simply 'realism' henceforth) are *selective*: they identify only certain aspects of theories and models as suitable for belief. Leaving aside other precisifications and qualifications here, let us focus on the notion of selective realism, and on the role of case studies in arguing for and against it.

The fact that cases of past and present science are brought to bear in assessing proposals for realism is, of course, unsurprising. After all, realism is a proposed epistemology of science. Sharpening the obvious relevance of the subject matter, however, is the fact that for more than fifty years, the history of science has served as a crucible for realism in debates with different kinds of antirealists. On the one hand, Thomas Kuhn's (1970/1962) picture of the development of science over time as a sequence of effectively discontinuous frameworks for conceptualizing and interacting with the world set realists to arguing against the implication that scientific knowledge in any given domain is itself discontinuous over time, effectively resulting in the disposal of past "knowledge" with the advent of new paradigms and the theories that in part constitute them. On the other hand, Larry Laudan's (1981) rendering of the so-called pessimistic induction – a number of related arguments to the effect that the history of science, which contains theories that were empirically successful and yet far from the truth (especially concerning unobservable objects, properties, mechanisms, events, and processes) – set realists to arguing that the possible skepticism this might entail regarding a realist attitude toward contemporary theories could be answered.

Enter selective realism. The strategy of being selective with respect to the content of theories whose epistemic credentials a realist might like to defend has proven influential in attempts to defuse the antirealist implications of the apparent historical evidence for discontinuity and success from falsity. Turnabout being fair play, many realists have repurposed case studies in support of selective realism. The kernel of the strategy is to acknowledge that many past theories that realists would like to defend are false, while nevertheless asserting that they contain parts that are largely on the right track, close to the truth, or perhaps even true. On this approach the burden of realism is then to demonstrate that these elements are in fact preserved across theory change and, ideally, in a way that suggests that they are likely to be retained in future developments of the relevant science. Thus we see a ramping up of the role of cases: they are presented as *prima facie* challenges to realist interpretations of science and in reply and counter-reply, the ensuing debate sees them adduced on both sides. I will argue in what follows, however, that contrary to what all of this sound and fury conveys, historical narratives of scientific theorizing and modeling cannot settle the question of whether forms of selective realism are tenable.

Before diving into specific illustrations of this contention, let me offer two preliminary remarks. The first is to clarify the matter of what, for present purposes,

a historical case study is, exactly. In keeping with *de facto* practice in much of science studies, I will use the terms 'case study' and 'historical evidence' in an ahistorical way, to refer to descriptions of scientific practice (theorizing, modeling, experimentation, beliefs and various non-doxastic commitments, etc.) in both the past and the present. While the techniques of and the resources available for case study scholarship clearly differ across temporal parameters, many are shared, and the outputs – descriptions of scientific practice – are of a type. When thinking about the prospects of realism in ways that take into account the development of scientific knowledge over time up to the present and into the future, the past is but one source of important "historical" evidence.

Secondly, in hopes of preventing misunderstanding before it can occur, it seems wise to stress at the outset what I am *not* attempting to show in what follows. The inefficacy of cases in settling the tenability of forms of selective realism does not by itself entail anything concerning *other* roles and functions of historical evidence in discussions of scientific knowledge. Indeed, I take the truism that case studies are indispensable to any such discussion to be uncontroversial. In the absence of this kind of work it is often at best unclear what the object of analysis is meant to be in the first place, since scientific theories are not typically offered on a platter but must be discerned and extracted from a complex mix of the apparent commitments of scientists, mathematical and other descriptions, collections of models, presentations and publications in a variety of media, and so on (cf. Burian 1977, p. 29). This kind of discernment is part of the historian's craft and is imitated by philosophers of necessity with varying degrees of success. The goal in view is thus not to dispute the fact that cases are important, since their importance is implied by their indispensability, but to dispute the idea that they are capable of settling debates about selective realism.

2.2 Debates About Explanationism: The Potency of Historical Narrative

Let us begin with the form of selective realism most transparently associated with dueling historical case studies, *viz.* explanationist realism, or *explanationism*. This view, commonly associated with Philip Kitcher (1993) and Stathis Psillos (1999), is explicitly presented as a response to historical challenges to realism by means of counter-case studies exemplifying the theme that realism is best conceived as attaching to those parts of theories and models that are crucial to explaining how these representations were and can be used to make successful (in the best case, novel) predictions. Whatever theoretical posits play this role in the context of the relevant science is what realists should believe, and because this leaves much aside as inappropriate for belief, the more general falsity of the representations is no threat to realism. So argues the explanationist. Furthermore, the fact that belief-worthy posits play central roles in explanation suggests that they are likelier than posits that

play no such role to be retained across theory change. It is with this sort of view in mind that Kitcher (pp. 140–149) distinguishes between the ‘presuppositional posits’ of theories, which do no essential explanatory work in connection with empirical success, and ‘working posits’, which do.

In order that these distinctions carry their intended force, however, it is incumbent on realists who analyze cases to construct historical narratives in such a way as to be unsusceptible to counter-counter-narratives – that is, to narratives that indicate that what one philosopher takes to be a merely presuppositional posit regarding something that realists need not believe (and which, as it happens, disappeared as theories in the relevant domain changed over time) was in fact essential to the empirical success of the theory in the minds of the relevant historical actors, or at least some of them. This focus on what historical actors (scientist and philosopher alike) were in a position to believe, could have believed, and did in fact believe is key to avoiding the charge of ineffective rationalization *post hoc*. That is to say, it will not serve realists simply to claim that certain components of past theories were worthy of belief and thus retained across theory change if the only reason for thinking so is that later and current theories contain them, since this would forever undermine realism concerning theories of the past and present – whatever time slice one considers – until one is in a position to judge from the perspective of the final science, which no one is expecting any time soon, if ever.

Are explanationist case studies irreproachable? As is well known to readers of the history of science, generally they are not. Even so, this is compatible with the fact that some historical narratives may be judged better than others, and the concomitant possibility that cases favoring realism (or antirealism) should prevail. There are certainly historiographical standards according to which this kind of judgment is rendered in practice. All things being equal, case studies that are informed by more impressive varieties of trustworthy sources of evidence are better than those that are not, and no doubt some studies exhibit better execution in judging the relative worth of different sources and in constructing compelling narratives from them. At this level of abstraction, however, shared standards are insufficient to render unequivocal narratives. At the level of concrete execution, shared standards are differently interpreted and applied and different narratives result (cf. Kinzel 2016). When this occurs, it is no consolation that judgments about the superiority of one historical narrative over another can be given, because the very nature of these judgments is compatible with conflicting verdicts. This inevitably leads to discrepancies between realist and antirealist readings of the historical evidence concerning selective realism.

Consider an example. Psillos (1994) provides a study of the sort of case from the history of science that is commonly taken to present a *prima facie* challenge to realism: the theory of caloric as a fluidic substance of fine particles, in terms of which phenomena including changes in temperature and state were putatively explained on either side of the turn of the nineteenth century. It was posited that hotter bodies are richer in caloric than cooler bodies, and that the substance itself could flow from one to the other. The obvious challenge to realism here stems from the fact that according to the subsequent kinetic theory of heat, which explains these

same phenomena in terms of molecular motion, there is no such thing as caloric. Psillos contends that those who were prominent in theorizing about caloric, such as Joseph Black, Pierre-Simon Laplace, and Antoine Lavoisier, were not in fact especially committed to the *existence* of the substance. Indeed, the most important empirical predictions of the theory can be derived without supposing its existence, which suggests the justifiability of this non-doxastic state in context. According to the narrative presented, these derivations depended solely on a commitment to other aspects of the theory which were ultimately retained as part of thermodynamics. Thus, a consideration of both what the actors themselves believed as well as what they were in a position to believe promotes explanationism.

For every counter-narrative, however, there is a counter-counter-narrative. In response to the explanationist view of the caloric theory, Hasok Chang (2003) and Kyle Stanford (2003) suggest that what may appear superficially as noncommittal attitudes on the parts of some scientists towards caloric are in fact better interpreted as merely rhetorical statements addressed to particular audiences, masking genuine commitments to the existence of a caloric substance which were hardly divorceable from their understandings of what is required in order to derive the empirical predictions of the theory. Here as before, narratives are constructed from what scientists and their contemporaries wrote and said in articulating their thoughts about the nature of heat. In this construction, substantive interpretation is unavoidable. Granted, one may argue for one interpretation over another, and granted further, the very notion of interpretation may seem trivial in connection with certain *kinds* of alleged historical facts – ones for which the evidence is so strong that to doubt them would be to adopt an implausible form of skepticism. Contentions regarding what the relevant historical actors here believed deep down, however, let alone the *degrees* of belief they entertained, and speculations regarding what one might have justifiably believed in the context, are not of this kind. In this regard Psillos (1994, p. 162) is self-aware: ‘I do not deny that my use of historical evidence is not neutral – what is? – but rather seen in a realist perspective.’

In pointing out that attempts to support disputed philosophical theses with historical cases are often theory laden, I am not claiming any astonishing novelty.¹ Somehow, though, the thought that the construction involved in substantiating philosophical contentions with historical evidence leaves room for interpretation, which is itself amenable to shaping by the contentions at issue, has not been registered in debates about explanationism. It is perhaps too easy to slip from the sensible views that cases are essential to these discussions, and that these discussions may help to advance our understandings of the disputed claims in productive ways (for example, by helping us to refine the positions at issue; cf. Saatsi 2012, Vickers 2013), to the view that they are capable of resolving the conflict between opposing sides. The former views do not entail the latter and as I have suggested, however

¹Laudan (1977, p. 157) discusses the idea of “self-authentication” in historical tests of accounts of rational theory choice. In related veins, see Hull 1993, Burian 2001, Pitt 2002, Schickore 2011, and Kinzel 2015.

sympathetic one may be to the former (as am I), there is ample reason to doubt the latter. Questions about the beliefs of historical actors and what one might justifiably believe in specific contexts of historical analysis are pregnant with interpretive leanings. The fact that scientists involved in one and the same research program often appear to disagree among themselves about the precise details of what should be believed and to what degree only confirms and exacerbates the problem.

2.3 Debates About Entity Realism: Bait and Switch

Let us turn now to another example of selective realism for which the limitations of cases have yet to come into focus. *Entity realism* rose to the fore in work by Nancy Cartwright (1983) and Ian Hacking (1983), in keeping with a general suspicion toward assessing the prospects of realism in terms of a knowledge of theories. With the rise of the philosophy of scientific practice, an emphasis on what scientists actually do in laboratory and experimental settings furnished a new way of thinking about the purchase of realism. In case studies of experiments, entity realists suggest that there are conditions under which realism about the existence of certain entities is an obvious and perhaps *the* natural doxastic attitude to hold regarding laboratory work. The conditions are variously specified – the seeming ability to manipulate the relevant entities in highly systematic ways; the apparent use of such entities to intervene in phenomena involving other, less well understood things; the exploitation of entities as causes of phenomena described in causal explanations – and all admit of degrees of impressiveness. Given a sufficiently impressive case, and once one has genuinely immersed oneself in its details, not merely considering them in the abstract but internalizing them in the way that practicing scientists do, it is difficult to be anything other than a realist about the relevant entities.

As in the case of explanationism, I will not pause here to consider whether entity realism is ultimately compelling as a proposal for selective realism. My interest here is rather in thinking about the role that cases play in arguing for it. The question of what precise form of argument these cases yield is itself open to considerable pondering (see Miller 2016), but with present purposes in mind, let me focus on a specific feature of them that is telling, when viewed in a historical perspective, for my own argument. Imagine that a given case study of experimental practice is sufficiently impressive to convince an entity realist of the reality of some entity – say the electron. In any domain of science in which theory change has occurred, even if one holds that the sequence of theories describes one or more of the same entities, one must accept that it is generally the case that theoretical characterizations of these entities have changed significantly over time. This is clearly true of the electron, from its conception in 1897 in terms of corpuscles composing cathode rays in experiments by J. J. Thomson, to the relativistic field theory conception of contemporary quantum electrodynamics. On a Kuhnian picture of discontinuities in scientific knowledge over time, it is simply a mistake to identify the putative referents of these radically different conceptions with one another. And anyone

unmoved by this picture but nonetheless intrigued by the pessimistic induction may well balk at the scale of this suggested preservation of reference.

Thus, much like the explanationist, the entity realist faces a *prima facie* challenge from the history of science. This challenge might be evaded if some or all of today's experimental practices met the conditions that entity realists offer as favorable to realism *exclusively* – that is, if only contemporary science were apt for realist interpretation. This is not, however, what entity realists typically believe. Their view is compatible with the idea that past science has, on at least some occasions, met these conditions also. Furthermore, there is presumably at least some expectation that future science will too, despite the fact that future conceptions of entities currently worthy of realist commitment may also change significantly. Quite independently of the intentions of the first advocates of entity realism, one reason the position has proven attractive to others is its potential for making sense of the history of science in a way amenable to realism, by making certain kinds of entities the bedrock of belief in a sea of changing theoretical descriptions. The challenge of extending successful reference across theory change thus cannot be evaded insofar as entity realism is to serve as a viable, candidate form of selective realism.

How, then, is reference preserved? What is needed is a theory of reference that is compatible with changes, sometimes fairly drastic changes, in theoretical description. And we have such a theory of reference to hand, which allows for the possibility that even very different and conflicting descriptions can be associated with one and the same thing. The causal theory of reference as developed by Hilary Putnam (1985/1975, chapter 12) and Saul Kripke (1980) and applied by many to scientific entities in particular offers to secure reference through initial “baptismal” events in which a term is “attached” to an entity (or kind of entity) with which an agent is in causal contact through observation or some other form of detection, after which a causal chain of referentially successful use of the term is rooted in this initial event of naming. Now, there are challenges facing all theories of reference and the causal theory is no exception. Some worry that a purely descriptive theory (according to which reference is secured through true description) is too rigid a straightjacket for linguistic practice, placing too much emphasis on the intensions of terms to reflect the ways in which we actually use them. But others worry that the causal theory's emphasis on the extensions of terms likewise fails the intuitive test of practice, making it virtually impossible to fail to refer once a term has been associated with some vaguely ostended phenomenon, thus trivializing reference altogether.

In between the extremes of purely causal and purely descriptive theories of reference are variations on the theme of splitting the difference with some form of causal-descriptive theory, which emphasizes descriptions but only within some limited range of causally relevant features. These views also face challenges, and it is not my intention here to decide which if any of these theories is correct. The present point is simply that, having appreciated the nature of the issues in play, it should be clear that their resolution does not depend on the details of scientific case studies. Historical evidence *underdetermines* theories of reference. Recourse to cases is essential in making the case for entity realism, but in assessing whether the

position is ultimately tenable one inevitably confronts a bait and switch. Whatever plausibility one associates with the position turns out to depend on a rather specific understanding of the nature of reference given the history of science, and the history itself is powerless to decide the matter. Given a series of cases of experimental practice putatively involving electrons, one might interpret them in such a way that reference is preserved through different conceptions of them, or one might view the very idea that we and J. J. Thomson are talking about the same thing, given the radically different conceptions involved, as a kind of *reductio*. History does not adjudicate this.

Perhaps one could hold out hope that through a more careful study of cases we will find that scientific practice does in fact yield verdicts about when it is appropriate to take terms to refer across theory change, but such hope is, I submit, groundless. There does not appear to be any “policy” exemplified throughout the history of the sciences that dictates when the continued use of a term signifies reference to one and the same thing ostensibly picked out by one’s predecessors, or when terminological innovation signifies a change in referent. These are philosophical issues which are not often addressed in any very direct way by scientists, though admittedly the historical record testifies to such thinking. If the hope of interpreting these hints regarding what scientists themselves believed, or could have believed, or were in a position to believe, is the basis of an aspiration to decide questions of reference through case studies, however, we have now returned to the problematic of the previous section, and that did not end well.

Alternatively, one might think that the proper course of action here is first to decide which theory of reference is correct, and then to accept whatever consequences for interpreting case studies may follow. In this way, cases would be telling for entity realism after all, one way or the other. But this suggestion is surely confused about the philosophy of science. The philosopher of science first considers and then is sympathetic or unsympathetic to entity realism. Inclinations toward one theory of reference or another then follow, to at least some significant degree, as a matter of course.

2.4 Generalizing the Moral: The Proper Functions of Cases

I began this essay with the observation that the most popular forms of scientific realism for some time now have been selective, not least in response to challenges to realism citing the history of science. That selective realism should itself be subject to “testing” by means of case studies of science is thus hardly surprising. I have argued, however, that such tests can only be so telling. Despite the ways in which they are presented by advocates and critics of realism alike, they are not telling with respect to the ultimate question of whether forms of selective realism are tenable. Certainly, if there were no cases at all in which forms of selective realism could be squared with the historical evidence, or no principles according to which such coherence with the details of scientific cases could be recognized if it were to obtain,

this would be fatal to the very idea. There being no consensus regarding the truth of either antecedent, however, attention must turn to arguments for and against them, and what we have found is that these arguments eventually morph into disputes that cannot be resolved merely by examining the evidence of cases.

In making this argument I have focused on two forms of selectivity: explanationist realism and entity realism. Aficionados of realism and antirealism will have recognized, of course, that these positions do not exhaust contemporary approaches to selective realism, which naturally raises the question of whether the thesis for which I have argued here (assuming that it is compelling) applies more generally. In recent years, *structural realism* has been the subject of significant attention, and the position for which I have argued, *semirealism* – a hybrid of entity and structural realism – is also correctly described as a form of selective realism. While space does not permit a fuller discussion presently, let me close with the briefest of indications of why the limited efficacy of cases in testing selective realism pertains to these positions as well.

Structural realism is the view that realism should attach only to certain structures described by our best theories (typically in physics, but arguably elsewhere), and while cases are essential to furnishing examples of structures and how they are preserved (in some form – an admittedly contentious point) in theory change, they are powerless to answer the question of whether the specific conceptions of structure at issue are tenable objects of realist commitment. On the epistemic conception of the view (Worrall 1989; cf. Papineau 2010), realism is committed only to the Ramsey-sentence structures of theories, but whether this amounts to a genuinely realist interpretation of science as opposed to a form of instrumentalism is an open question, and one whose answer does not turn on historical evidence as such. The ontological conception of the view (Ladyman and Ross 2007; French 2014), which posits a self-subsistent ontology of relations at the expense of ontologically subsistent relata, is fixated on a number of discussions concerning the metaphysical coherence of different versions, all of which are (again) immune to historical considerations. Insofar as semirealism (Chakravartty 2007) represents a selective extraction of aspects (*ex hypothesi*, the most compelling aspects) of entity and structural realism, the same moral applies once again. These brief remarks are at least suggestive, I hope, of a general claim regarding the limitations of historical cases in resolving debates about selective realism.

Lest we succumb to too much negative talk of limitations and what cannot be done, let me end on a positive note. The burden of this essay has been to argue that case studies cannot be marshaled in quite the way that some interlocutors in debates about selective realism intend, but this leaves rather a lot for them to do. I have indicated some ways in which they are essential, but beyond the necessities, the proper use of case studies may yet facilitate the most interesting and important work to be done in this domain. Though I cannot argue for this contention here, there is something to be said for the idea that different forms of realism and antirealism are, in fact, coherent epistemological positions, and compatible with the evidence of some if not all scientific cases. What remains for us to understand more precisely is the boundaries of types of cases for which such compatibility is in evidence.

What historical, theoretical, experimental, cognitive, and other conditions must be satisfied in order that any given version of selective realism (or its denial) be a defensible epistemological diagnosis of science in any given case? That, only time and cases will tell.

Acknowledgments This essay arose from the symposium 'Quo Vadis Selective Realism?' at the biennial meeting of the European Philosophy of Science Association in Düsseldorf. I am grateful to Dean Peters and Peter Vickers for envisioning it and to David Harker and Tim Lyons for helping to shape it. Thanks are also due to those subjected to earlier versions of these thoughts at the &HPS meeting in Vienna and the Universities of Durham, Johannesburg, Indiana Bloomington, Peking, Renmin, and Shanxi.

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