ARTICLE IN PRESS

Studies in History and Philosophy of Science xxx (2016) 1–11

Contents lists available at ScienceDirect

Studies in History and Philosophy of Science

journal homepage: www.elsevier.com/locate/shpsa

Structural realism versus deployment realism: A comparative evaluation

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ARTICLE INFO

Article history:
Received 29 June 2016
Available online xxx

Keywords:
Scientific realism
Structural realism
Deployment realism
Pessimistic induction
Divide et impera
Novel predictions
No-miracles argument

ABSTRACT

In this paper I challenge and adjudicate between the two positions that have come to prominence in the scientific realism debate: deployment realism and structural realism. I discuss a set of cases from the history of celestial mechanics, including some of the most important successes in the history of science. To the surprise of the deployment realist, these are novel predictive successes toward which theoretical constituents that are now seen to be patently false were genuinely deployed. Exploring the implications for structural realism, I show that the need to accommodate these cases forces our notion of "structure" toward a dramatic depletion of logical content, threatening to render it explanatorily vacuous: the better structuralism fares against these historical examples, in terms of retention, the worse it fares in content and explanatory strength. I conclude by considering recent restrictions that serve to make "structure" more specific. I show however that these refinements will not suffice: the better structuralism fares in specificity and explanatory strength, the worse it fares against history. In light of these case studies, both deployment realism and structural realism are significantly threatened by the very historical challenge they were introduced to answer.

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1. Introduction: deployment realism and structuralism

This paper endeavors, ultimately, to examine structural realism as an explanation for novel predictive success. A great lesson of 20th century philosophy of science is that evaluations of empirical hypotheses are comparative and ultimately triadic: it is not merely hypothesis versus data; it is rather hypothesis versus hypothesis, with adjudication between them being focused in part on data. The relevant data in my empirically triadic evaluation are those from the history of science. And the foil against which I will compare structural realism is the more robust, and arguably the most sophisticated, variant of scientific realism, deployment realism. Before turning in the spirit of Laudan (1981) to the historical data, we can compare these positions conceptually.

According to deployment realism, we can be justified in believing the following meta-hypothesis: those theoretical constituents that were genuinely deployed in the derivation of novel predictive success are at least approximately true. The justification for believing this meta-hypothesis: it would be a miracle were our deployed theoretical posits to achieve such successes were they not at least approximately true. Deployment realism has much going for it. It is testable, as I will try to make clear below. It is applicable, offering the promise of allowing us to identify, in at least some instances, those constituents to which we can, according to deployment realism, commit ourselves. And it appears to be genuinely explanatory: credit for success is not due to those elements that were mere "idle wheels," rather it is due to those and only those that were responsible for the particular successful predictions. While the need for comparative evaluation is a central lesson from 20th century philosophy of science in general, this emphasis on the proper attribution of credit may be one of the central additional lessons arising from the realism debate, in particular. Forgetting either lesson would constitute a backward step.

Worrall looking to Poincare embraces what he calls "*syntactic* or *structural realism*" (1989, 157; see also 152), singlehandedly introducing it to the contemporary scientific realism debate. Structural realism (hereafter "structuralism") purports to be a logically weaker, less committal, position than deployment realism: we can be justified in, and need only commit ourselves to, believing the structures expressed by those constituents genuinely deployed toward success. Like deployment realism, structural realism appeals to the no-miracles argument articulated above, but suitably modified: the only explanation for novel success is that the structural elements of those constituents responsible for those successes

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http://dx.doi.org/10.1016/j.shpsa.2016.06.006
0039-3681/© 2016 Published by Elsevier Ltd.

Please cite this article in press as: Lyons, T. D., Structural realism versus deployment realism: A comparative evaluation, Studies in History and Philosophy of Science (2016), http://dx.doi.org/10.1016/j.shpsa.2016.06.006

capture the actual structure of the world. Comparative plausibility requires that structuralism meets the demands met by deployment realism. It must be testable. The structural elements of the deployed constituents must be identifiable. And, in order to stand as a genuine explanation of novel predictive successes, the structure must be genuinely deployed toward the novel predictions. Beyond embracing the same explanatory argument, structuralism shares with deployment realism another feature: both Worrall and Poincare brought it in to deal with the history of science, in particular, instances in which historical theories were successful but are nevertheless now taken to be false. Although we cannot say that Fresnel's ether theory is approximately true, it was impressively successful, predicting, for instance, the infamous white spot in the center of the shadow of an opaque disk. How are we to make sense of this success, given the rejection of Fresnel's ether theory by contemporary science? Worrall answers, "there was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content" (157). Because structuralism is less demanding, we should expect that, with respect to given instances of novel successes, it fares better than, or at least as well as, deployment realism. Offering an explanation for novel predictive success, along with an ability to handle the historical data that challenges traditional realism, structuralism purports to offer "the best of both worlds," to use Worrall's apt phrase (1989).

The great question left by Worrall's hint is, How precisely are we to understand structure? It turns out that structuralism has a long history. And, drawing on various early 20th century philosophers, it has come to take on many forms. One might well claim that there are more variants of structuralism than there are structuralists, since a number of individual structuralists, Worrall among them, have changed their positions. I anticipate that my historical considerations below will not only pose threats to deployment realism but will at least require clarification of some of forms of structuralism, if not significant revision. The latter result, of course, would invite even more variants.

Much discussion of structuralism has focused on a few sets of historical theories: the successes of 19th century ether theories, e.g. Fresnel's, and quantum mechanics, e.g. on the question of whether, in light of that theory, the notions of individuality, objecthood, intrinsic properties, etc, can be retained. Building on my (Lyons, 2006), the focus in this paper will be on the history of other cases from the well-known arena of celestial mechanics, in which both deployment realists and structuralists have tended to think their position fares unquestionably well: I will start with a few comments on Kepler, Newton, Leverrier and Adams, then move to predictions made by general relativity. Examining arenas of theory change that appear, initially, non-threatening to both the deployment realist and the structuralist, and then showing that those arenas do pose a threat, reveals that threat to be especially pressing.

Since deployment realism and structuralism share those core components captured above, my approach will be as follows: I will first discuss historical threats to deployment realism and then, in due course, ask whether structuralism promises to do as well or better—or whether, by contrast, the structuralist is in just as bad or (surprisingly, perhaps) worse shape given the particular historical cases brought forward. We will see that, leveraged by those cases, the structuralist's need for the retention of structure compels the structuralist toward increasingly vacuous conceptions of "structure." The better structuralism fares against the historical examples, the worse it fares in content and explanatory strength. In light of that problem of explanatory vacuity, the structuralist will be compelled to embrace some explicitly restrictive notions of structure, against which, in Section 4, I will direct the cumulative force of the discussion. The better structuralism fares in specificity and explanatory strength, the worse it fares against history. The upshot: structuralists are trapped by their need for historical retention of structure and their own explanatory demand.

2. Some challenges from the history of celestial mechanics

2.1. Preliminary cases

In his (1596) *Mysterium Cosmographicum*, Kepler articulated his theory of the *anima motrix*, a theory that causally explained some primary features of planetary motion. Because that text was written years before Kepler met Brahe, it is clear that it was not put forward to accommodate Brahe's detailed data. And what we find is that, directly deploying its foundational posits, now taken to be patently false, Kepler made a series of temporally novel predictions. Toward the latter, seeking to explain why the planets are moved in paths around the sun, he posited the following: planets move only when forced to move; they could not move themselves, because they would tire; the sun is unique and in fact supreme, not by its size, but its divinity; it is positioned at the center of the universe; the sun is that which pushes the planets in their orbits; it emits rays that do the pushing, *the anima motrix*, etc. Later, in a 1605 work, *Astronomia Nova* (1609), Kepler derived from that theory the prediction that the sun spins: "since the [emanation] of the source, or the power moving the planets, rotates about the center of the world, I conclude with good reason… that that of which it is the species, the sun, also rotates" (1609, p. 387). He also predicted that the sun spins in the direction of planetary motion and that it spins along the plane of the ecliptic. Returning now to his (1596), beyond providing a physical explanation of why planets moved, he wanted to explain, why those with a greater mean distance from the sun traverse their orbits at a slower pace than those closer to the sun. To do this, he conjoined to the above posits the hypothesis that, as the *anima motrix* pushes the planets, its strength decreases in proportion to their mean distances from the sun: "there is a single moving soul in the center of all the spheres, that is, in the Sun, and it impels each body more strongly in proportion to how near it is. In the more distant ones on account of their remoteness and the weakening of its power, it becomes faint, so to speak" (1596, 199). In answering how that force diminishes, he invoked his hypothesis that the intensity of light is inversely related to distance from the sun. Moreover, "motion is dispensed by the Sun in the same proportion as light" (1596, 201). From these, in conjunction with the posits above, he arrived at his prediction that the sun spins faster than any of the planets revolve around it (1609, 387–8). Further, the *anima motrix* gave him "the reason and the means" to "defend" the "irregularity in" the planetary paths: a "planet will be slower" when "further away from the Sun," where it is "moved by a weaker power," and "faster" when "closer to the Sun," where it is "subject to a stronger power" (1596, 217). This constituted the novel predictions that planetary motion is non-uniform and, more specifically, that each planet will reach its highest speeds at its perihelion and its lowest at its aphelion. Brahe's data confirmed this for Mars, Jupiter, and Saturn, rendering the predictions at least use-novel, and eventually for Mercury, Venus, and the Earth, instances of temporally novel success. These predictions would later be accepted as holding for the planets unknown to Kepler, Uranus and Neptune. Although some of his predictions may have been articulated later, the posits I've flagged as pivotally deployed toward them predate his access to Brahe's data.

In his (1609), directing his theory of the *anima motrix* toward what came to be known as his second law, Kepler supplemented his reasoning with the posit that the planet's own inclination to be at rest takes over as the sun's push diminishes (1609, 384). He also looked to Gilbert's theory of magnetism, reasserting, however, that

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