How To Make Selective Realism More Selective (and More Realist Too)

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Abstract

Selective realism is the thesis that some wisely chosen theoretical posits are essential to science and can therefore be considered as true or approximately true. How to choose them wisely, however, is a matter of fierce contention. Generally speaking, we should favor posits that are effectively deployed in successful prediction. In this paper I propose a refinement of the notion of deployment and I argue that selective realism can be extended to include the analysis of how theoretical posits are actually deployed in symbolic practices.

1. Introduction

Among the several forms of realism, the so-called selective realism (SelRealism) is arguably the one that engages history of science more seriously. The driving idea of SelRealism is that, although theories as wholes are false and doomed to be abandoned, it is possible to select a certain number of theoretical posits (TPs) that are likely to be maintained in future theories and are therefore true or approximately true. How to determine these TPs is *partly* an empirical question—and this explains the historical character of the SelRealism program—but it cannot be *merely* an empirical question lest one end up in post-hoc rationalizations. A central issue of

SelRealism, hence, is how to specify criteria to properly conceptualize the TPs on which one should place one's realist commitment.

In this paper, I argue that contemporary approaches to SelRealism have neglected an important element related to the way in which theoretical claims are deployed in scientific theories (Section 2). In Section 3, I propose a refinement of SelRealism based on the distinction between deploying a TP fundamentally and deploying it in a non-accidental fashion. I use the concept of symbolic practices to articulate this distinction. Finally, in Section 4, I clarify my points by discussing the early development of perturbation theory.

2. Selective Realism: Theory and Practice

The upholders of SelRealism cherish two fundamental ambitions. First and foremost, they aim at making a good use of the so-called no-miracles argument (NMA) according to which one can justifiably infer the truth (or the approximate truth) of a successful theory, because, otherwise, the success would remained inexplicable. The NMA is considered to be the strongest support to realisms of any sort (Musgrave 1988; Psillos 1999, 68-94). A challenging objection to the NMA is the pessimistic meta-induction (PMI) originally formulated by Larry Laudan. According to this argument, the success of a theory is never a sufficient reason to infer even its approximate truth because history of science is replete with examples of very successful theories that wound up overthrown at some later stage. As it is likely the case that our most successful theories will suffer the same fate in the future, one has to conclude that the realist commitment is not justified (Laudan 1981). Among the several responses to the PMI, one consists in noticing that the failures of past theories, in fact, did not depend on those TPs that lead them to success. In other words, granted Laudan's point that successful past theories are false as wholes, it can still be argued that the constituents of those theories that were responsible for their empirical success have been retained in our current science. Thus, the realist needs only to shift her commitment from theories as wholes to those enduring TPs that, being essential for success, can be justifiably believed to be true or approximately true.

The next question is, of course, how to determine those TPs. Thus, the second ambition of the upholders of SelRealism is to solve the problem of selectivity in some principled way and so beat the PMI. In one of the first instantiations of SelRealism, Philip Kitcher argued that one must "distinguish between those parts of theory that are genuinely used in the success and those that are idle wheels" (Kitcher 1993, 143). The point of this distinction is that credit for the success of a theory should be due only to those TPs that effectively contribute to it. Elaborating on Kitcher's intuition, one can argue that the program of SelRealism is based on two major conditions:

(S) Success condition: the selection of the important TPs must hinge on their relation with some significant success of the theory.

(D) Deployment condition: one must select those TPs that were effectively used in scoring that success.

Let me briefly comment on these two conditions. While (S) is now a realist trademark, the deployment condition (D) is what sets apart SelRealism from other forms of realism, such as structural realism, also engaged in picking out enduring elements of scientific theories (Worrall 1989; Chakravartty 2011). It is also important to notice that (S) and (D) are independent conditions. Firstly, (S) refers to a relation between the selected TP and empirical success, while (D) refers to a relation between the TP and the rest of the theory. Secondly, either condition can be satisfied separately. (D) has been added precisely to avoid those cases in which idle TPs are involved in empirical success and, obviously, there are scores of examples of TPs used by theories which however never led to any success. It follows that, while (S) is supposed to meet the first ambition of SelRealism, the second ambition, to block the PMI, is on (D).

So much for SelRealism in theory. Let us now examine how this program has been carried out in practice. One of the first philosophers to seriously elaborate on Kitcher's suggestion was Stathis Psillos. His criterion for selecting TPs works in the following way (Psillos 1999, 110). Let us assume that a certain successful prediction *P* can be obtained by combining the TPs *H*, *H'* and the auxiliaries *A*.[1] According to

[1] For virtually all writers, empirical success means "successful prediction". David Harker has leveled important criticisms against this tendency to interpret success in terms of individual predictions and has suggested that success should be understood as progress, i.e. in terms of the improvements a theory makes with respect to its predecessors (Harker 2008, 2013).

Psillos, the TP *H* is essential to success *P* and should be considered true or approximately true if and only if:

(1) *H'* and *A* alone do not lead to *P*.

(2) There is no alternative *H** to *H* such that:

  (a) *H** is consistent with *H'* and *A*;

  (b) *H**, *H'*, and *A* lead to P;

  (c) *H** is not *ad hoc* or otherwise purposefully concocted to lead to *P*.

This criterion is the bedrock of Psillos's *divide et impera* strategy. The driving intuition behind it is to capture the *indispensability* of *H*: we should place our realist commitment upon those TPs without which empirical success cannot be obtained. However, Tim Lyons has cogently argued that Psillos's criterion fails to characterize indispensability (Lyons 2006). The indispensability of *H* should be ensured by condition (2), which states, in brief, that *H* cannot be replaced by any other TP. But, Lyons notices, "there will always be other hypotheses, albeit some that we find very unappealing, from which any given prediction can be derived" (Lyons 2006, 540). More importantly, Lyons argues, Psillos's criterion is not even an effective means for credit attribution, because it does not tell us much about how *H* contributes to the empirical success *P*. In particular, condition (2) has no relevance whatsoever for *H*'s specific contribution, because it only concerns conceivable alternatives to *H*, alternatives that, if *H* is at hand, nobody would even bother to explore. Lyons perceptively stresses that the problem with Psillos's criterion boils down to the fact that it obliterates condition (D): "by introducing his criterion, [Psillos] has discarded the central idea of deployment realism—introduced by Kitcher and seemingly advocated by Psillos himself" (Lyons 2006, 541). It is interesting to note that, by dropping condition (D), Psillos's position becomes vulnerable to another form of PMI. One could think of getting around of Lyons's first objection by arguing that, even though an alternative to *H* is always conceivable, *at the present state* of our knowledge it is not, therefore the objection is empty. In other words, one could inject the time factor in Psillos's criterion and make it a statement of our actual best knowledge. But then the PMI crops up again, because history shows that there is no guarantee that what is indispensable today will be so tomorrow. The whole point of the PMI is that there is nothing special in our knowledge as far as it is considered *present*, because there have been a lot of *present knowledges* that have been blissfully abandoned. This is why one needs condition (D): what makes our present knowledge so special is not its happening at a certain time, but its having gone through a certain *process*, i.e., a form of deployment. The fact that our present knowledge has been deployed at lengths and it is still with us constitutes a reason to believe that it is true or approximately true.

3. Deconstructing Deployment

Having grasped that the flaw in Psillos's criterion is the dropping of the deployment condition, Lyons suggests to run to the other end of the spectrum and to inflate dramatically the notion of deployment. His “responsibility model” consists in discarding selectivity altogether and in considering responsible for the empirical success of a theory each and every element that was originally deployed: “credit will have to be attributed to all responsible constituents, including mere heuristics (such as mystical beliefs), weak analogies, mistaken calculations, logically invalid reasoning etc.” (Lyons 2006, 543). Clearly, Lyons’s proposal amounts to a crack-up of the entire SelRealism program. But, more importantly, I do not think that the responsibility model captures the correct significance of (D). As my previous considerations about the PMI show, the deployment condition is not merely supposed to tell us that a TP has been effectively used in obtaining empirical success (as opposed to be *dispensable*), but also that it has been robustly so (as opposed to be merely *accidental*). What makes it plausible that a TP will still play a role in future theories is the fact that its importance for empirical success has been tested by extensive and repeated deployment. It is therefore clear that there are two ideas nested in the deployment condition. One is the idea, captured by Psillos’s criterion, that significant TPs must play a fundamental role in success in order to distinguish them from idle hypotheses; the other is the idea that the deployment of a TP must ensure that its success is not accidental. These are two distinct ideas. It might happen, for example, that a TP plays an essential role in deriving a prediction in virtue of fortuitous factors cancellation or other favorable circumstances. So, while an *intensive deployment* ensure the *fundamentality* of a TP, an *extensive deployment* founds its *robustness*. Both fundamentality and robustness are ways to articulate the complex relation between a

TP and the rest of the theory, or at least some parts of the theory (more on this in a bit). Further, while fundamentality is an atemporal articulation of this relation,[2] robustness concerns precisely the temporal dimension of the deployment condition that escaped Lyons's analysis: robustness, as we shall see below, is achieved over time.

In order to clarify the distinction between fundamentality and robustness, I introduce the notion of *symbolic practices*. By symbolic practices I mean all the methods customarily used in science to manipulate symbols.[3] These include, but are not limited to, mathematical methods, formal tools, approximations procedures, models, heuristics, solution tricks, and any sort of way by which one can transform a symbolic expression into another symbolic expression. Symbolic practices are the set of methods adopted by a theory to "put to work" a certain TP or, in other words, to deploy it in order to set problems and to interpret solutions. By using the concept of symbolic practices, one can reformulate the two ideas of the deployment condition in the following way:

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[2] Of course the fundamentality of a TP can change over time because it can become more or less fundamentally used. However, the relation in itself does not concern this change.

[3] My discussion is especially tailored on the case of mathematical physics. I do not exclude, however, that it can be suitably extended to other branches of science by taking an appropriately enlarged notion of symbolic practices.

(F) Fundamentality: A TP must be *embedded* in a set of symbolic practices that lead to empirical success.

(R) Robustness: The symbolic practices adopted to deploy the TP must be *reliable*.

Let us begin with (F). This idea hinges on the "embeddedness" of a TP into a set of symbolic practices. An empirical success, a successful prediction or an explanation, is obtained by starting with one TP—or, better, its symbolic codification—and by deriving from it the phenomena to be treated by means of suitable manipulations. In their analysis of the path from TP to success, philosophers usually disregard the epistemic role played by symbolic manipulations of TPs. But if we neglect this important factor of the process of predicting/explaining, we are left with no other option than characterizing fundamentality as a relation between TPs, i.e., a 'Psillosian' criterion and then a 'Lyonsnesque' argument can easily prove that this falls short of providing a satisfactory notion of fundamentality. In my proposal, fundamentality is rather a relation between TP and the symbolic practices adopted to transform and manipulate it. Although intuitively clear enough, the concept of embededdness admittedly needs further philosophical analysis. In Section 4, I provide a historical example to clarify what it means for a TP to be embedded into a set of symbolic practices.

Before discussing the example, however, I need to analyze briefly the idea of robustness. Condition (R) states that reliability, and hence robustness, is a property of the symbolic practices themselves. In other words, and this is the central point, a TP can be made more robust by means of *historically and rationally describable strategies* conceived to enhance the reliability of symbolic practices adopted to put it to work. One way to appreciate this point is to notice that the concept of reliability has three main components. First, there is an *empirical component*, that is its connection with success. It is expected that reliable symbolic practices have led and will lead to empirical success. This is unsurprising, because it is still part of the relation between (D) and the NMA. Second, there is a *conceptual component*: reliable symbolic practices allow us to distinguish between real facts of nature and artifacts. This is the component that accounts for the non-accidentality of success and it depends on the adoption of strategies to enhance reliability. Applying symbolic practices to multiple cases, relating them with other, better understood, sets of practices (e.g., by showing structure similarities), generalizing solution methods, simplifying computation procedures, introducing redundant check routines, improving the symbolic notation, multiplying proof procedures are just a few examples of strategies used to ensure that the result of symbolic manipulation is a real information and not an artifact generated by the practice itself.[4] Finally, there is a *historical component*. As I said above, deployment is a process extended over time. When are we justified to consider a result as reliable? This is an agent- and a context-dependent component of reliability.

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[4] This component of the concept of reliability is closely connected with the usual notion of robustness (see, e.g., (Soler et al. 2012) for an overview). Indeed, robustness has to do with the multiplications of methods of check and control as a way to distinguish what is real and what is fabricated by practices.

I submit that this component can be clarified in terms of *control*. We develop theories because we need to manipulate symbols in order to make predictions and explanations. It is reasonable to state that an agent considers reliable a theory when she has control on it, when she knows how to do things, where the theory can be applied, to what extent, what kind of information she can obtain, what kind of epistemic risks are involved in it, how to improve progressively the performance and a lot of other things related to the general idea of knowing what is going on. Thus, reliability can change over time in virtue of new information and further inquiry. This component accounts for the fact that science is an ongoing human endeavor.

To sum up, I propose to extend SelRealism in the following way:

(SelRealism+) We are entitled to consider the TP *H* as true or approximately true at time *t* if and only if:

1. *H* is embedded into a set of symbolic practices *S*
2. *S* is reliable
3. *H* and *S* lead to significant success

This is a more selective version of SelRealism, because the philosophical and historiographical program stemming from it extends the inquiry to the strategies adopted to improve the reliability of symbolic practices and the contingent conditions for control. As stated in condition 3, the units of analysis of SelRealism+ are TPs-*cum*-

practices rather than TPs only. In the following section, I provide an example of what I mean by intensive and extensive deployment.

4. The Coming of Age of Perturbation Theory

The *Principia Mathematica* are a supreme example of how to embed a TP, in this case the gravitational law, into a set of symbolic practices.[5] However, Newton's mainly geometrical methods were fantastically complicated and notoriously difficult to master. A significant breakthrough in what came to be called celestial mechanics happened in the mid-1740s, when Leonhard Euler laid down the foundations of analytical perturbation theory. Euler made a number of decisive steps forward. First, he used the gravitational law to formulate general equations of motion for celestial problems. Second, he introduced the use of trigonometric series to construct approximate solutions. The use of these series also depended crucially on the gravitational law, because it satisfied the assumption that planetary orbits, even under perturbations, can be represented by a combination of periodic functions. Finally he introduced manipulation practices such as the method of the variation of

[5] In what follows, I consider perturbation theory as the set of practices conceived to put to work the gravitational law. It must be noted that other TPs were involved (e.g., Newton's laws of dynamics) and that the gravitational law can be decomposed in further assumptions such as the action-at-a-distance, the instantaneous propagation and so forth. These considerations affect the level of detail of my example, but not the structure of my argument.

constants and the method of successive approximations to solve the equations of motion. Perturbation theory is therefore a clear example of a set of symbolic practices conceived to cast a TP into a manipulable form and to applied it to specific problems.

For the purpose of this paper, I distinguish two phases in the early history of perturbation theory. The first phase goes roughly from the mid-1740s to the mid-1760s and it concerns the cause of numerous astronomical anomalies. Newton had left behind a few conundrums that even his genius was unable to unravel. The most conspicuous of these problems was the precession of the Lunar apogee. Newton's Lunar theory, elaborated in Book I and III of the *Principia* only managed to obtain half of the observed value. In the 1740s, there were two approaches to the issue of the Lunar apogee. The analytical approach adopted the gravitational law, or a slightly modified form of it, and tried to calculate the observed precession by analytical methods only. The physical approach supposed that the observed anomalies could be due to material causes such as a resisting medium or interplanetary vortices. It is important to realize that these approaches were compatible. Euler himself supported both the resisting medium hypothesis and the analytical approach and occasionally also proposed the use of vortices (letter to Clairaut, 30 September 1747). For several years, the best mathematicians of Europe struggled with the riddle of the Lunar apogee (Bodenmann 2010) until, on 21 January 1749, Alexis Clairaut showed that if one pushes the approximation to the second order of the perturbation, some terms that are negligible at the first order become sizable and generate the missing half of the precession (Clairaut 1752).

Clairaut's success was surely an impressive breakthrough, but what made it so impactful was not the brute fact that gravitational law had eventually led to a successful explanation. Physical hypotheses such as vortices and resisting medium also provided an explanation of the observed precession. The crucial difference lies in the fact that the gravitational law could be fully integrated with the analytical practices and then manipulated to provide suitable symbolic expressions of the precession of the apogee. That did not happen with the physical hypotheses, although not for lack of trying. Euler, for instance, tried hard to integrate the hypothesis of the resisting medium in perturbation theory, but the ensuing equations of motion were simply unmanageable (Euler 1747). Clairaut's success is eminently a story of intensive use of the gravitational law: he managed to integrate it with a set of symbolic practices and to accommodate effectively the observations.

Clairaut's feat did not close the debate on the gravitational law, tough. His calculations used many case-based assumptions, simplifications, and shortcuts and its straightforward extension to more complex cases, such as the behavior of Jupiter and Saturn, was doubtful to say the least. But there was also a deeper problem. At some point in his analysis, Clairaut obtained an "arc of circle", i.e., a trigonometric function multiplied by time. Such terms are obviously unbounded and hence make the whole trigonometric series diverge. Clairaut got rid of it by ad-hoc assumptions, but the status of these unbounded terms remained unclear: they could represent an artifact of the theory, a limitation of its predictive power or even a dynamical instability of the system.

Soon, the problem of the arcs of circle become more troublesome. Euler found the same terms in his analysis of the motion of Jupiter and Saturn and in 1766 Lagrange proved that they are actually a necessary consequence of the method of successive approximations applied to astronomical problems (Lagrange 1766). Thus, in the mid-1760s, perturbation theory appeared to be a fragile set of practices which had scored some important success, but was still marred with problems of unreliability under certain conditions. From the late 1760s onwards, the issue of improving the robustness of perturbation theory became a central preoccupation of the leading mathematicians interested in physical astronomy.

There were two programs inspired by this issue. On the one hand, Lagrange tried to improve the reliability of perturbation methods *as a mathematical theory*. He carried out this project by means of multiple strategies: (1) enhancing the relation between perturbation theory and other branches of mathematics (e.g., potential theory); (2) elaborating arguments to extract information from the equations of motion without solving them (e.g., by using integrals of motion); (3) improving methods to simplify the solution procedure (e.g., Lagrange's coordinates); (4) introducing new symbolic codifications to manipulate the equations of motion (e.g., the perturbing function); (5) making the notation less cumbersome (Lagrange's coefficients). Around the same years, Laplace was also working to improve the reliability of perturbation theory, but his program adopted a different approach. He concentrated on methods to make perturbation theory a more reliable *problem-solving tool*. He developed his own method to eliminate the arcs of circle—which was based on the recalculation of the integration constants—he imported probability theory and the equations of condition to deal with astronomical observations and devised several strategies to identify in concrete cases those elements of the equations of motion that were likely to produce sizable perturbation terms at higher order. Both Lagrange's and Laplace's programs scored their own successes. In the early 1780s, Lagrange proved a very general result of stability according to which the three more important orbital elements (mean motion, eccentricity, and inclination) are invariable or bounded (Lagrange 1781). Laplace, on his part, explained the decades-long problems of the anomaly in the motion of Jupiter and Saturn as well as the secular acceleration of the Moon (Laplace 1785, 1787; Wilson 1985).

5. Conclusions

In several places, Kyle Stanford has argued that any selection of enduring TPs is ultimately ungrounded and, consequently, the entire SelRealism program is unviable (Stanford 2003, 2006). In his view, there are two possible ways to select essential TPs. The first way is to trust scientists when they say that a certain posit is fundamental. However, neither commonsense, nor, more importantly, historical records support the hypothesis that scientists' take on this matter is or should be particularly reliable. The other option is to wait and see: when a theory is superseded, one can check which TPs have survived. The reason why a selective realist cannot go with this option, however, has been summarized effectively by Peter Vickers:

> If we cannot identify the working posits of a theory until it has been superseded by some other theory, then realism is no longer about identifying what we ought to believe to be true: one is always waiting for the next theory to come along to tell us which parts of our current theory are working posits. (Vickers 2013, 207)

From this, Stanford concludes that SelRealism without prospectively applicable selectivity criteria is empty and should be replaced by a more modest form of realism. But Stanford's wait-and-see stance is neither necessary nor sufficient to do the job it is supposed to do, i.e., to pick out essential TPs. It is not sufficient because there is no guarantee that the TPs survived one theory change will survive the next ones. It is not necessary because we do not need the next theory to form reasonable judgements about essential TPs. As I have shown above, science provides a variety of strategies to improve the reliability of the TP-*cum*-practices and hence good reasons to believe, *within the actual theory*, that a certain TP intensively and extensively deployed is in fact essential.

From this perspective, Stanford's argument simply sets the epistemic bar too high. By stating that the essentiality of a TP can be adjudicated only from the vantage point of the superseding theory, he implicitly challenges the realist to provide a "superselection rule" able to capture the whole history of science, a task that the realist is neither willing, nor actually requested to accomplish. By contrast, the historical and philosophical program of SelRealism+ moves from the conviction that TPs and symbolic practices follow a dynamics able to filter out inessential components. Consequently, SelRealism+ is committed to historically identify and philosophically analyze this dynamics and to trace the genealogy of our theories in terms of the processes of codification, manipulation, and stabilization of TPs. Ultimately, this program aims at producing new and interesting historical narratives of theory change. It remains true that the strategies making up the theoretical dynamics only provide good reasons to allocate the realist commitment. It might happen that the judgement on the reliability of the TPs-*cum*-practices change over time in virtue of further inquiry or new information. This fact, as stated above, follows from the fallibility of science as a human endeavor and, as such, should not trouble the realist.

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