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Multiple discoveries, inevitability, and scientific realism

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ABSTRACT

When two or more (groups of) researchers independently investigating the same domain arrive at the same result, a multiple discovery occurs. The pervasiveness of multiple discoveries in science suggests the intuition that they are in some sense inevitable—that one should view them as results that force themselves upon us, so to speak. We argue that, despite the intuitive force of such an "inevitabilist insight," one should reject it. More specifically, we distinguish two facets of the insight and argue that: (a) the profusion of multiple discoveries in scientific practice does not support the inevitabilist side of the inevitability/contingency of science controversy; and (b) the crucial role of background knowledge in scientific inquiry complicates the attempt to interpret the pervasiveness of multiple discoveries in realist terms.

1. Introduction

Multiple discoveries have an air of inevitability to them: when several scientists who investigate a certain domain independently from one another all arrive at basically the same result, the intuition emerges quite naturally that the result is in some sense inevitable. Consider the discovery of the periodic table of elements. Often associated with the names of Dimitri Mendeleev and Lothar Meyer, such a discovery is, in fact, a case of multiple discovery involving, to varying degrees, no less than six individuals—the less celebrated ones being Alexandre de Chancourtois, John Newlands, John Odling, and Gustavus Hinrichs. In the short timespan between 1862 and 1869 these researchers, living in different parts of the world, specializing in different fields, tackling the ordering of chemical elements with different approaches, and not communicating among them, independently arrived at basically the same result (see Scerri, 2007, 2015). This is certainly a clear-cut case where the converging of several individuals elicits the intuition that there is something inevitable to the result on which they converge (an intuition that we call the "inevitabilist insight").

While sociologists and historians of science have extensively studied multiple discoveries, philosophers of science have, with rare exceptions, neglected them so far. This is surprising. In this paper, we first offer a sort of crash course in multiple discoveries, arguing that they are an important phenomenon which should draw the attention of philosophers, if only because it is a pervasive feature of the scientific enterprise. Second, we suggest that multiple discoveries raise a number of interesting issues, two of which we investigate in order to propose an appraisal of the inevitabilist insight. More specifically, we distinguish two facets of the insight, the first relevant for the recent debate on the inevitability/contingency of science, the second for the age-old debate on scientific realism.

The first facet of the inevitabilist insight is best introduced by considering again the multiple discovery of the periodic table, which helps to elucidate the notion of inevitability deployed by Ian Hacking in the writings that initiated the inevitability/contingency controversy (1999, 2000; for the state-of-the-art, see Soler et al. 2015). A result of scientific inquiry is inevitable, in the sense in which Hacking used the term, if it is what we get, when we get things approximately right about a certain fragment of the world. To put it differently, when we are successful in the investigation of a given domain, the result forces itself upon us, so to speak, and is thus inevitable. The multiple discovery of the periodic table then suggests itself as case of a result of science that qualifies as inevitable in the relevant sense. More generally—here comes the first facet of the inevitabilist insight—it seems at least prima facie plausible that multiple discoveries instantiate the so-called "inevitability thesis" at the heart of the controversy. According to the thesis, if the result of the scientific investigation of a certain subject matter is correct, then any investigation of the same subject matter, if successful, will yield basically the same result. And if multiple discoveries—which pervade scientific practice—instantiate the inevitability thesis, then one may be

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tempted to take their pervasiveness to support the inevitabilist side of the controversy, thereby speaking against the contingentist side. In fact, according to the contingency thesis, there could be alternatives to our current science that, as successful as our current science, are nevertheless built around entities and processes radically different from those around which our best theories are built. Despite the obvious interest of the relationship between multiple discoveries and the idea of inevitability for the controversy, such a relationship has so far remained largely unexplored (a notable exception being Radick, 2005, pp. 21–47, whose work this paper attempts to complement).

The second facet of the inevitabilist insight has to do with the perennial issue of scientific realism. The fact of several competent individuals who work independently of each other converging on the same result typically increases our confidence that the relevant fragment of the world is (approximately) as science says it is. One may then suggest—this is the second facet of the inevitabilist insight—that the pervasiveness of multiple discoveries mandates an interpretation in realist terms, whereby science's overall impressive success in describing the world accounts for such a profusion of multiple discoveries.

In a nutshell, our analysis indicates that one should reject both of the above facets of the inevitabilist insight: the first, because champions of the contingentist side of the inevitability/contingency controversy can account for multiple discoveries just as straightforwardly as champions of the inevitabilist side; the second, because the role played by background knowledge in the life of scientific communities, central in both realist and antirealist accounts of the significance of multiple discoveries, complicates the interpretation in realist terms of the profusion of multiple discoveries in science. We suggest that the phenomenon of multiple discoveries is then neutral with respect to the realism/antirealism debate—as looking at multiple discoveries in the light of path dependence in the development of science helps to highlight.

We proceed as follows. Since the interest of our topic hinges on multiple discoveries being a pervasive feature of the scientific enterprise, in section 2 we address their standing. Against claims to the contrary, we argue that, appropriately construed as a matter of degree, multiple discoveries are indeed a pervasive phenomenon in the life of scientific communities. In section 3, we first distinguish between the inevitability of scientific results in the sense that matters here and inevitability as inexorability, whereby certain results of science are inevitable in the sense that, given appropriate conditions, they will be attained. We then turn to discussing the inevitability thesis, and argue that there is no reason to maintain multiple discoveries to instantiate it. In section 4, we attend to the explanation of the pervasiveness of multiple discoveries and caution against the temptation of making too much of the apparent cogency of a straightforwardly realist interpretation of said pervasiveness. In section 5, we offer some brief concluding remarks.

2. The pervasiveness of multiple discoveries

Multiple discoveries—epitomized by such famous cases as Wallace and Darwin both putting forward the theory of evolution by natural selection, several individuals independently discovering the periodic table, etc.—have attracted the attention of many over the decades.[2] In the relevant literature, to which sociologists and historians of science have contributed the most, the synonymous phrases "multiple discovery," "independent discovery," and "multiple independent discovery" are used, according to the writer's preferences, to designate the phenomenon. These phrases are oftentimes prefixed with the word "simultaneous," although one should note that such a term must not be understood literally: it is unanimously agreed that, as Merton put it, "discoveries far removed from one another in calendrical time may be instructively construed as 'simultaneous' or nearly so in social and cultural time" (1973 [1961], p. 369). In other words, when multiple discoveries are the topic of systematic study, the focus is not on chronological priority in specific cases—which of course is of central importance to researchers involved in disputes over priority of discovery and to historians of science investigating such disputes—but, rather, on the general phenomenon, whereby (teams of) scientists working independently from one another arrive at more or less the same result within an appropriately circumscribed timeframe.

Many of those who have studied multiple discoveries agree on their pervasiveness. For instance, Merton maintained multiple discoveries—to which he referred as "multiples"—to be the rule in science, and therefore "singletons"—discoveries arrived at only once in history—to be the residual case in need of special explanation (1973 [1961], p. 356; for consonant takes see Kuhn 1977 [1962]; Lamb & Easton, 1984, p. IX; Scerri, 2016, p. 174). It is also widely agreed that any kind of discovery—both discoveries involving a big, apparently revolutionary leap from established science and small, incremental additions to it—can be a Mertonian multiple.

Despite the attention that the phenomenon has drawn, it is far from unanimously agreed under what circumstances one can legitimately speak of a multiple discovery. Disagreements and doubts depend on a variety of reasons. For a start, even in those cases in which it is beyond dispute that two or more researchers independently arrived at the same result, only rarely has the result in question been attained—and presented by the discoverer—in the exact same way (for an illustration, see Scerri, 2015 on how the co-discoverers of the periodic table presented it). Moreover, there are cases of celebrated multiple discoveries—such as the alleged triple independent rediscovery of Mendel's principles on the part of de Vries, Correns, and von Tschermak—that historians have shown to be neither entirely independent of the prior, original discovery, nor equivalent to it (on the "independent rediscovery" of Mendel's principles see, e.g., Olby, 1985; Bowler, 1989). The suggestion has then repeatedly been made that, as soon as one starts digging deeper into the contributions published by presumed co-discoverers, the list of actual multiple discoveries gets significantly shorter than one would have expected initially (see, e.g., Brannigan et al. 1981, p. 268). Some have even argued that the canonical catalogues of multiple discoveries are "simply unsound" (Schaffer, 1994, p. 35), and suggested that the phenomenon is far from pervasive.

Attempts to dispute the pervasiveness of multiple discoveries have often traded on the fact that, if one deploys a strict criterion of identity of discovery, the phenomenon of multiple discoveries looks easily dispensable. To mention but one example, economist Don Patinkin suggested that, before declaring a certain episode an instance of multiple discovery, one should carefully examine "the extent to which the alleged co-discoverers 'really meant it'—the extent to which they incorporated the discovery in what I shall call their 'central message'" (1983, p. 306). On Patinkin's account, several contributions to a certain body of scientific literature qualify as reports of a multiple discovery only if: (*a*) their authors state in unequivocal terms what problem they are addressing, how they see the problem with respect to the current state of their discipline, and what solution to the problem they advocate; and (*b*) there is perfect coincidence in all of these three respects.

Such a strict construal of the notion of multiple discovery, we suggest, is far too strict. We agree instead with Jürg Niehans who, among others, claimed that in order for a number of contributions to instantiate a multiple discovery, "they need to have one newly discovered proposition in common" (1995a, p. 4): that their authors differed in other respects

[2] See, e.g., Ogburn and Thomas (1922); Kuhn (1977 [1959] and 1977 [1962]); Merton (1973 [1957], 1973 [1961], and 1973 [1963]); Lamb and Easton (1984). Interest in the phenomenon certainly predates such classic contributions (Merton, 1973 [1961], pp. 353–354 lists no less than eighteen studies devoted to multiple discoveries conducted between 1828 and 1922) and continues to flourish to this day: see, e.g., discussions of multiple discoveries in such diverse disciplines as botany (Troyer 1992, 2001), economics (Niehans, 1995a; but see also the ensuing critical discussion; De Marchi, 1995; Mirowski, 1995; Roncaglia, 1995; Niehans, 1995b), physics (Sarafoglu et al. 2012), and mathematics (Whitty, 2017).

(for instance, what led each of them to pursue a certain research question instead of another) does not change the fact that they hit upon the same result. Of course, as Niehans readily concedes, the identification of such a shared, common "proposition" (or whatever else one wants to call it), depending as it does on the interpretation of complex texts, is precisely one of the issues that tend to spark controversies concerning specific cases of presumed multiple discovery. Moreover, we submit, history of science suggests that scientific discoveries in general, and not only those that end up with being considered multiple discoveries, tend to be complex affairs, involving various researchers and often unfolding over extended stretches of time, that can hardly be reduced to cases of individuals grasping propositions. Still, it seems to us, Niehans is entirely correct in complaining that the deployment of strict criteria of identity renders the elimination of the notion of multiple discovery deceptively easy.

One way to get around the problem of how to distinguish genuine cases of multiple discovery is to focus, rather than on abstract criteria of identity, on the opinions of the individuals active in the relevant scientific field. Sociology of science, for instance, has followed precisely such a path. Sociologists generally view "discovery"—and "multiple discovery" alike—as a label that a scientific community retrospectively attaches to certain achievements, and take the social process of assessment of candidate (multiple) discoveries as the proper object of inquiry for sociology of science (see, e.g., Cozzens, 1989 on the co-discovery of the opiate receptor). Schaffer (1986, 1994, pp. 13–51) among others has complained that the notions of discovery and multiple discovery then turn out to be closely—and worryingly—connected with the way in which scientific communities recount their own histories, which is far from disinterested.

Still, we argue, the fact that the scientific community *qua community* is crucially involved in certifying—or not certifying—an alleged (multiple) discovery as such does nothing to suggest, let alone show, that multiple discoveries are not a pervasive feature of the scientific enterprise. And in any case, the relevance of the community has always been one central focus of the investigations concerning multiple discoveries. Famously, the starting point of Merton's work on the issue was the observation that history of science is punctuated by "frequent, harsh, and ugly" (1973 [1957], p. 289) disputes over priority of discovery. On Merton's account, such disputes are aggravated by the fact that science is a social institution viewing originality as a supreme value, thereby rendering the public recognition of one's contribution to the increase of the stock of accepted knowledge a chief concern for any practicing scientist. Analogously, Kuhn (1977 [1959], 1977 [1962]) insisted that any achievement that ends up with qualifying as a genuine (multiple) discovery has to go through a complex process of validation on the part of the relevant community. In the same vein, Lamb and Easton railed against the use of any narrow criterion of identity of discovery, suggesting that when it comes to scientific discovery, "absolute identity" is nothing but "a philosophical chimera" (1984, p. 94). A viable account of the phenomenon of multiple discoveries, they argued, ought to openly acknowledge not only the similarities, but also the differences among the research programs of various scientists. Multiple discoveries are then a matter of degree, the area of overlap between the work and the results of the researchers involved varying from case to case. In most cases, the object of a multiple discovery—the result independently arrived at by several individuals—is a complex entity, process, or property of an entity or process, and each of the scientists connected with a multiple discovery will have grasped to a greater or lesser degree the significance of (aspects of) the relevant achievement. In consequence of this widely acknowledged circumstance, Lamb and Easton argued, strict criteria of identity of discovery play no role in the life of scientific communities (1984, p. 96; see also Scerri, 2016, p. 185).

In short, we maintain that appropriately construed as a matter of degree, multiple discoveries are indeed a robust, indispensable feature of the scientific enterprise. As Merton put the point long ago, the "race to be the first in reporting a discovery" (1973 [1961], p. 361) shows that scientists themselves take multiple discoveries to be a fact of scientific life. To mention but one recent example of the enduring concerns that multiple discoveries generate among practicing scientists, consider the editorial policy introduced in 2018 at the journal *PLoS Biology*, concerning the case of "scooped," or "complementary," manuscripts. For the purposes of said policy, being scooped is defined "as when two independent groups studying the same system produce the same or similar results" (Alvarez-Garcia et al. 2018, p. 1), and one of the groups publishes them first. Provided that the second group submits to *PLoS Biology* a manuscript presenting the same results (or an extension thereof) within six months from the publication of the first manuscript, the second group's manuscript is eligible for consideration. This editorial policy of course aims at mitigating what is perceived as an undesirable, unfair aspect of the reward system currently in place, the so-called "priority rule" whereby the first to publish a result is the winner that takes it all, while scooped researchers are relegated to the role of also-rans.[3]

The editorial team of *PLoS Biology* moreover maintains that the journal's being a venue that welcomes scooped or complementary manuscripts for consideration will help in addressing the so-called "reproducibility (or replicability) crisis" (on which see, e.g., the survey by Fidler & Wilcox, 2018). As is well-known, the crisis arises from cases in which a certain research group announces the discovery of a result, but other researchers cannot, despite thorough attempts, reproduce it—a circumstance that generates doubts concerning its soundness. Scooped or complementary manuscripts, authored by research groups working independently of the group that won the publication race, can therefore perform the function of replication studies, which the editorial team of *PLoS Biology* views as the "gold standard for demonstrating that an article is based on solid results" (Alvarez-Garcia et al. 2018, p. 1). The new editorial policy then treats multiple discoveries as a (partial) remedy for the crisis: cases of multiple discovery can solve the problem of the reproducibility/replicability of the relevant result before it even arises. In any case, reproducibility/replicability and multiple discovery are neatly distinct concepts, and in what follows we deal only with the latter. In particular, in section 3 we distinguish between two senses of the notion of inevitability, as it has been deployed in connection with the phenomenon of multiple discoveries, and then discuss whether in fact multiple discoveries can be viewed, in virtue of their pervasiveness in scientific practice, as somehow favoring the inevitabilist side of the inevitability/contingency controversy.

3. Multiple discoveries, inexorability, and the inevitability thesis

A result of science is inevitable, in the sense that matters for the inevitability/contingency controversy, if it is what a successful investigation of a certain domain yields. It is with reference to this sense of the notion of inevitability that multiple discoveries suggest themselves as results of science instantiating the inevitability thesis—on which, of course, we say more below. First of all, though, we need to note that the concept of inevitability has been used by various authors, in connection with multiple discoveries, in at least another, significantly different sense, to which one can refer with the label "inevitability as inexorability."

To mention but one instance, inevitability as inexorability is at the heart of a paper by historian of chemistry Aaron J. Ihde entitled "The inevitability of scientific discovery" (1948), devoted to the analysis of the question of what explains the pervasiveness of multiple discoveries in history of science. On Ihde's account, "the relentless pressure of accumulating knowledge" (1948, p. 429) features prominently within the

[3] See Kim and Corn (2018) for reflections concerning the benefits of such a policy on the part of the leaders of two research groups involved in a case of multiple discovery, and the editorial announcing that *Biological Communications* has adopted a similar policy (Malashichev, 2017). Thanks are due to Federico Boem for drawing our attention to the new editorial policy of *PLoS Biology*.

explanation of multiple discoveries. More specifically, Ihde argued that when the background knowledge that is necessary in order for a community to make a certain discovery has accumulated, the discovery will be made, typically by more than one individual. As he put it: "Once the accumulation is complete the next step [i.e., the discovery] becomes inevitable" (1948, p. 429). Not that the discovery will be made immediately but, according to Ihde, it is "sure to come" (1948, p. 428).

Ihde did somehow temper the "inexorabilist" flavor of his account of multiple discoveries by adding that it is only with the benefit of hindsight that one can view a certain discovery as the inexorable outcome of the relevant phase of accumulation, and the bits of knowledge accumulating along the way as belonging to a unitary process. In any case, let us note that his claim concerning the inevitability as inexorability of discoveries is at least in some tension with the fact that there are cases in which, despite the time for a certain discovery apparently being ripe—in virtue of the availability of the required material and technical conditions—it took centuries before it was in fact made (see, e.g., the discussion in Lamb & Easton, 1984, Ch. 5). More to our point, one needs to emphasize that the notion of inevitability, in the sense relevant for our purposes, does not concern the timing of discovery. Nor is it suggestive of arguments to the effect that, provided that an appropriate state of background knowledge obtains, the relevant discovery will ensue. Inevitability here does not mean that, if an adequate amount of ingenuity, time, and resources is poured into scientific inquiry, then science will succeed in making new discoveries. What the notion of inevitability refers to here is the idea, encapsulated in the inevitability thesis, of the results of successful science being '"essentially' and 'implicitly' contained in end-run science" (Hacking, 2000, p. S60).

To clarify, let us recall that the controversy initiated by Hacking revolves around two conflicting theses. According to the contingency thesis, the historical development of the scientific enterprise could have led to alternatives to our current science exhibiting two features: (*a*) being as explanatorily and predictively successful as our science; and (*b*) being radically different from our science, in the sense of being built around entities and processes incompatible with the ones around which our best theories are built (think, for instance, of a physics with no quarks, or of a biology without genes). According to the inevitability thesis instead, any science as explanatorily and predictively successful as ours, and devoted to the investigation of the same subject matter that our science investigates, will yield essentially the same results of our science. The inevitability thesis then amounts to an affirmative answer to the following question: "If the results R of a scientific investigation are correct, would any investigation of roughly the same subject matter, if successful, at least implicitly contain or imply the same results?" (Hacking, 2000, p. S61).

Hacking adds that, in light of the inevitability thesis, one can view the results of successful science (the correct results of the scientific investigation of a certain domain) as belonging to "imagined end-run science" (2000, p. S60). While he devotes no space to further elucidating the latter notion, it seems natural to think of end-run science as a regulative ideal, equivalent to the Peircean ideal of the completion of the project of scientific inquiry, reached, as Nicholas Rescher put it, when proceeding further would make no difference at all, "because inquiry has come to the end of the road" (1999, p. 155). The inevitability thesis, then, says that when science is successful, it yields results that, by virtue of being correct, are like tributaries that feed into the final stage of the scientific development, and so will feature within the definitive scientific image of the world—the end-run science that, when the dust settles, will collect all the correct results of science.

Talk of end-run science offers a useful point of entry into the discussion of the first facet of the inevitabilist insight. In fact, inspection of end-run science is of course out of the question, so that one cannot know what results of science will feature within the definitive scientific image of the world. For someone embracing the inevitability thesis, it is however quite natural to view the instrumental and predictive success of our current sciences (for instance, of our physics built around such entities as quarks) as an indicator that their results are obvious candidates to be preserved up to the final stage of the scientific development—as an indicator that their results are inevitable, by virtue of possessing the correctness required to feature within end-run science. When it comes to multiple discoveries, they appear by the same token good candidates for the role of results of science that qualify as inevitable—at least, according to the first facet of the inevitabilist insight. In fact, in light of the overall success of our sciences, that a result is arrived at by several competent individuals independently investigating a certain domain can be viewed as an indicator that the result possesses the correctness required to feature within end-run science: multiple discoveries seem to force themselves upon scientists, just as one would expect an inevitable result to do. For someone harboring inevitabilist sympathies, it may then seem natural to suggest that history of science as a whole tells in favor of the inevitability thesis: replete with multiple discoveries, it offers countless instantiations of the thesis.

However, we suggest, it is far from obvious that many results in history of science being multiple discoveries tells in favor of inevitabilist conclusions. In fact, the pervasiveness of multiple discoveries in scientific practice is not something that proponents of the contingency thesis deny. Quite on the contrary, they can claim that such a pervasiveness is precisely what one should expect in light of the contingency thesis, and given how scientific communities operate. More specifically, proponents of the contingency thesis can agree that the vast majority of discoveries in science are multiple discoveries, and yet put forward a different interpretation of the phenomenon. For instance, they can point out that when multiple competent individuals who independently investigate a certain domain D all start from the same "initial point" IP_1 (by embracing the same background knowledge or, in Kuhnian terms, the same paradigm), and ask the same questions, it is only natural to expect that they will all reach the same result—let us refer to it as MD_1. Calling MD_1 "inevitable," and taking it to tell in favor of the inevitability thesis, though, is misleading. In fact, it is easy to imagine that, had all the relevant individuals independently investigated domain D starting from a significantly different initial point IP_2, and asked different questions, they would have all reached a result different from MD_1—let us call it MD_2.[4] In other words, that scientific practice is replete with multiple discoveries does not rule out the possibility of alternatives to actual history of science in which multiple discoveries are as frequent as in actual history, and yet the results arrived at are not the same as those of our current sciences. Multiple discoveries being a pervasive feature of science does not rule out, for instance, the possibility of a physics with no quarks, or of a biology without genes. In short, we maintain, interpreting the multiple discoveries arrived at in actual history of science as candidates to feature within end-run science by virtue of their possessing the required correctness, thereby instantiating the inevitability thesis, begs the question against proponents of the contingency thesis.

In this connection, Léna Soler has pointed out that formulations of the inevitability thesis always involve implicit or explicit "reference to the 'long run' or to a clause of the type with 'enough time and effort,' with 'further evidence,' after a 'sufficiently deep investigation,' after 'sufficiently hard and sustained effort,' or the like" (2015b, p. 79; see also 2008b, Section 4). Appeal to the long run on the part of inevitabilists, Soler argues, is however deeply problematic, among other things because they have nothing to offer in support of the claim that in the long run science will be in a certain way instead of another—nothing, that is, but their confidence that it will. The idea that multiple discoveries in actual history of science are the kind of result that qualifies as a credible candidate to be preserved up to the final stage of scientific development,

[4] Such a criticism of course relies on the devising of counterfactual histories of science, a topic which has generated quite some discussion in recent years (see, among others, Bowler, 2013; Dagg, 2017, 2019; Fumagalli, 2018; Hesketh, 2014, 2016; Jamieson & Radick, 2013; Radick, 2005, pp. 21–47, 2016; Tambolo, 2016, 2020a, 2020b).

we suggest, is analogously problematic. In fact, pointing to cases of success such as multiple discoveries and claiming that they instantiate the inevitability thesis is easy, but deceptively so, as our sketch of a contingentist account of their pervasiveness illustrates. What is more, in light of the crucial role that the notion of success plays in the very definition of that of inevitability, it is not even clear that one can appeal to cases of success as a non-question begging benchmark for the inevitability thesis. We thus maintain, against the first facet of the inevitabilist insight, that the phenomenon of multiple discoveries is of no help in the arbitration of the inevitability/contingency controversy.

In "Other histories, other biologies" (2005), Gregory Radick has dealt at quite some depth with the issue of the significance of independent trajectories in history of science converging on a result. At first, one may be tempted to think that, the greater the number of the trajectories, and the greater the independence of the trajectories from one another, the more plausible is the conclusion that the result on which the trajectories converged was inevitable. Nevertheless, Radick forcefully argues, this in-principle idea faces a number of important problems. In particular, it is often extremely difficult to establish, in practice, that allegedly independent trajectories are in fact independent: as Radick's case studies show, common inheritances in the history of science are so frequent that it is far from clear that one could rule them out entirely. The focus of our discussion has been on the separate claim, for which we argued with more abstract resources than concrete case studies, that one cannot take the pervasiveness of multiple discoveries in history of science as an argument favoring inevitabilism. Still, the reasoning deployed here to criticize the first facet of the inevitabilist insight is entirely in the spirit of Radick's compelling case against convergence as an argument for inevitability, so that in this regard the present contribution should be viewed as an attempt to continue his work.

Let us conclude the present section by briefly detailing how, to our mind, the foregoing contributes to the inevitability/controversy. The contrast between the inevitability thesis and the contingency thesis, as Hacking formulated them, was only the starting point of a discussion that has become more and more sophisticated over the years. In particular, it has become increasingly clear that science can be viewed as inevitable (contingent) along various dimensions.[5] In short, today it is almost unanimously agreed that sweeping theses concerning the inevitability (contingency) of scientific knowledge are of dubious analytic utility, and that claims concerning its inevitable (contingent) character should be carefully circumscribed. Our discussion leads to a conclusion in line with this consensus. In particular, we suggested that the idea of multiple discoveries as instantiations of the inevitability thesis, while possessing some intuitive force, does not provide champions of inevitability with significant ammunition against the contingentist side of the controversy. The above, then, offers yet another illustration of the fact that attempts to establish wide-ranging claims concerning the inevitability (contingency) of science are unlikely to bear fruit.

4. Multiple discoveries, background knowledge, and realism

Our discussion of the first facet of the inevitabilist insight, besides showing that multiple discoveries are so to speak "neutral" with respect to the inevitability/contingency controversy, suggests that appeal to end-run science is of dubious utility for the purposes of that controversy. There is, however, a good reason why Hacking (2000, p. S60) mentioned end-run science when introducing the concept of inevitability, namely, that the idea of a result featuring in (or being inferable from) end-run science is one way (although perhaps not the most perspicuous) to express the metaphysical kernel of scientific realism.[6] In fact, one key and basic component of realism is the claim that the world which our theories aim at describing exists independently of our minds. The correctness of the answer to any given scientific question is then determined by the mind-independent, objective properties of the world. This means that if the result of the investigation of a certain fragment of the world is correct, any successful investigation of it will yield, over and over again, basically the same result. It then makes sense to say that correct answers to scientific questions will be preserved in the longest possible run—up to the final stage of scientific knowledge.

The second facet of the inevitabilist insight emerges precisely in connection with the above realist tenets. Typically, when several competent individuals who investigate a problem independently of each other converge on the same result, our confidence in the correctness of the result increases: as Merton once wrote, multiple discoveries perform "several and varied social functions for the system of science," one of which is that "[t]hey confirm the truth of the discovery" (1973 [1963], p. 380).[7] From the viewpoint of the scientific realist, it is natural to suggest that the pervasiveness of multiple discoveries in scientific practice mandates an interpretation in terms of science's success in describing the world. In fact, the realist accounts for the explanatory and predictive success of individual theories in terms of their ability to provide a genuine, if imperfect, description of the fragment(s) or aspect(s) of the world that the theory aims to describe. When it comes to multiple discoveries, the realist will then suggest what they view as an inference to the best explanation: the reason why researchers so often arrive at the same result is that science is, overall, successful in providing us with (approximately) true descriptions of selected fragments or aspects of the world. To put it differently, the pervasiveness of multiple discoveries is a feature of the scientific enterprise that one can view as straightforwardly following from (and instantiating) realist accounts of science.

While we are quite sympathetic with an approach along the lines sketched above—which in any case would demand further elaboration and qualifications[8]—we suggest that one should not make too much of its apparent cogency. In fact, the pervasiveness of multiple discoveries is not something that proponents of antirealist views of science need to deny, or view as a potential problem for such views. Quite the opposite: antirealists can readily explain why science is replete with multiple discoveries by appealing to the crucial role that background knowledge plays in the life of scientific communities. More specifically, antirealists can easily offer an account of the profusion of multiple discoveries in science, revolving around the idea that scientific inquiry is typically carried out by communities of researchers who, all trained within a certain discipline of which they accept the basic commitments, and equipped with basically

[5] See esp. Soler (2008a, 2008b); Martin (2013); Kinzel (2015); Allamel-Raffin and Gangloff (2015); Aylward (2019). For an open-ended list of the "ingredients" of a discipline that can be viewed as inevitable (contingent), see Soler (2015a, p. 8): "experimental facts; observational and experimental data; scientific laws; mathematical theorems; theoretical hypotheses or whole theories in mathematics, physics, cosmology, biology, geology, the biomedical sciences, the engineering sciences, interdisciplinary inquiries, and in fields contested as genuinely scientific such as parapsychology; empirically equivalent but incompatible physical theories; mathematical formalisms and linguistic formulations of scientific claims in vernacular or specialized languages; scientific questions; human sense data and human modalities of experience; instrumental devices developed and used in the empirical sciences; technical tests, experimental recipes, and other reproducible connections between well-defined initial and final empirical conditions; mathematical proofs; methods in psychology; identity and boundaries of a whole discipline like mathematics; scientific frameworks, scientific paradigms, robust fits, worldviews, or similar multidimensional integrated units …".

[6] As Hacking put it: "We might say that our present successful results, if correct, must be 'essentially' and 'implicitly' contained in the end-run science. That, at any rate, is what many realistically inclined philosophers and scientist would maintain, as a point of metaphysics" (2000, p. S60).

[7] Merton, however, readily acknowledged that "on occasion errors *have* been independently arrived at" (1973 [1963], p. 380).

[8] Consider, for instance, the issues arising in connection with the assessment of the convergence of several individuals, dealt with in quite some depth within Bayesian epistemology of testimony; see, among others, Goldman (1999, Ch. 4), and Bovens & Hartmann 2003, Ch. 5).

the same instruments, investigate the same questions. In light of such a structural feature of the scientific enterprise, one can claim that the fact of several individuals happening to hit upon more or less the same result(s) is a matter of course, without thereby making specific commitments concerning the results that the enterprise yields. Indeed, not only can this be done, but it has been done by the few philosophers of science who devoted systematic attention to multiple discoveries.

As mentioned in the introductory section, multiple discovery—studied for decades by historians and sociologists of science—is a topic still today "largely unexplored" (Scerri, 2016, p. 186) within philosophy of science. One notable exception is represented by the work of Thomas Kuhn, whose theory of science is a classic of the discipline, certainly well-known to the reader. For our present purposes, it is however important to recount, if briefly, how multiple discoveries appear, in light of said theory, as a fall-out of paradigm-governed scientific inquiry. In "The historical structure of scientific discovery," espousing ideas abstracted from chapter 6 of the then-forthcoming *Structure of scientific revolutions* (1962/1970), Kuhn railed against the view that discovery is "a unitary event, one which, like seeing something, happens to an individual at a specifiable time and place" (1977 [1962], p. 165). He pressed his criticism of such a view by deploying a distinction between two kinds of scientific discovery. The first concerns "objects" whose existence had been predicted, based on then current scientific knowledge, before they were in fact discovered—one notable example being the elements that filled the gaps in the periodic table. The second kind of discovery concerns "objects"—such as, for instance, oxygen, X rays, and the electron—whose existence could not be predicted based on accepted scientific knowledge, and therefore surprised the relevant communities.

One remarkable feature of discoveries of the second kind is that they begin "with the experimental or observational isolation of an anomaly, that is, with nature's failure to conform entirely to expectation" (1977 [1962], p. 173). In other words, the emergence of anomalies is only possible when scientists have a very firmly established idea concerning how their instruments, on the one hand, and the world, on the other hand, are supposed to behave. This happens during periods of normal science, namely, when the work of the whole scientific community is conducted under the umbrella of a paradigm reigning supreme, and the attempt is made to force nature into the box that the paradigm offers to categorize it. The circumstances in which anomalies emerge and are recognized as such by the relevant community, Kuhn claimed, "may help us understand the extraordinarily large amount of simultaneous discovery in the sciences" (1977 [1962], p. 174, fn. 17). Note, however, that talk of "simultaneous" discoveries on the part of Kuhn must not be taken literally. According to him, the most important discoveries—those that surprise the relevant community, belonging to the second kind mentioned above—are not events about which one can appropriately ask such questions as "Where?" and "When?". In fact, awareness of an anomaly is only the starting point of a complex process during which several individuals attempt to accommodate the anomaly within the paradigm or, to put it differently, to exhibit it as compatible with the community's background knowledge. Sometimes such attempts succeed, sometimes they fail. When they fail, in order for the anomaly to be certified as a discovery, "additional observation or experimentation as well as repeated cogitation" (1977 [1962], p. 174) are required. As a result, in most instances it is simply impossible to identify who arrived at a certain discovery and when—as the case of the multiple discovery of oxygen, discussed by Kuhn at some length, is aimed at illustrating.[9]

The role of background knowledge in the life of scientific communities quickly comes to the fore also when one considers the other kind of discovery distinguished by Kuhn, namely, that of objects whose existence had been anticipated. In fact, the reason why an object is expected to be discovered is precisely that the members of the relevant community agree, based on accepted background knowledge, on what will result from a properly conducted investigation of a certain aspect of the world. On Kuhn's account, multiple discoveries are then just an ordinary feature of science, rooted in the fact that several researchers may well arrive at a certain result independently of each other, but certainly not independently of a given status of scientific knowledge. This is a central point of agreement among the authors who fostered the minority tradition of study of multiple discoveries in philosophy of science commenced by Kuhn, namely, David Lamb and Susan Easton (1984) and Eric Scerri (2016).

More specifically, in *Multiple discovery. The pattern of scientific progress* (1984), Lamb and Easton put forward an account whereby one should view "as part of the evolutionary drift of science" (1984, p. 97) also those cases of (multiple) discovery in which chance, or the contribution of some uniquely talented individual, seems to factor decisively. Consider, for instance, the emergence of the idea of universal gravitation, supposedly prompted by Newton's watching an apple falling from a tree. Over the centuries, countless individuals before him had watched apples falling from trees, without ever conceiving of universal gravitation. This fact alone suffices to illustrate that while accidents do assist discovery, something more is needed in order for a discovery to occur, such as, for instance, an observer prepared to make something of a casual observation. More importantly, in the case of Newton, an accurate historical account of how he actually arrived at universal gravitation highlights that the theory emerged only gradually, through empirical and conceptual work, and "in communication with contemporaries" (1984, p. 117)—so that individual creative flashes are less decisive for the development of science than many tend to believe. On the evolutionary account that Lamb and Easton defend, (multiple) discovery is ultimately a collective achievement, occurring as the result of the endeavors of communities of researchers that aim at meeting "the needs of science" (1984, p. 125). The scientific development, Lamb and Easton argue, has its autonomy, and every discovery, whether revolutionary in nature or not, owes much to the work of the discoverer's predecessors. The pervasiveness of multiple discoveries that one observes in science is then explained by the fact that "there is a drift, in the evolutionary tide, which favours some interventions and discoveries at the expense of others" (1984, p. 199), and that science progresses in a Lamarckian fashion: hypotheses do not emerge at random, but rather, grow out of previous accomplishments, and are selected by researchers based on "existing canons of plausible inference" (1984, p. 27). For our present concerns it is important to emphasize that, on this evolutionary account, the phenomenon of multiple discoveries is far from lending itself to simplistic realist readings, since according to Lamb and Easton, discoveries "are not 'out there' awaiting the receptive mind" and scientific practice "*creates* as well as *uncovers* the objectively real" (1984, p. 129).

In *A tale of seven scientists and a new philosophy of science* (2016), Scerri analogously rails against the traditional view—which, he argues, pervades philosophy of science to this day—that great scientists are the basic unit of progress. What really drives the scientific enterprise forward is, instead, "the faceless 'organism' that we call the scientific community" (2016, p. 9). More specifically, Scerri likens science to "a unified giant organism that is constantly evolving and in so doing is experimenting with slightly new ideas or theories" (2016, p. 7) in order to adapt to the environment. The growth of science then happens in a gradual way: unlike Lamb and Easton (and Kuhn before them), Scerri rejects the view that there are significant discontinuities (i.e., revolutions) in its development. Once the organic, collective nature of science is taken into consideration, Scerri argues, it comes as no surprise that multiple discoveries are pervasive, since they are made by individuals who work independently of each other but not independently of "the state of scientific knowledge" (2016, p. 178). On this evolutionary account, just like on the account put forward by Lamb and Easton, the ubiquity of multiple discoveries does not provide an argument for realist interpretations of the scientific development: progress, Scerri argues, is not a matter of "theories being right or wrong, just as biological evolution is neither right nor

[9] See also Kuhn (1977 [1959]), leading to the same conclusions with regards to the case of energy conservation.

wrong," and scientific theories evolve "in order to adapt to the particular times that they exist in, rather than in order to conform to some objective or 'out there' criteria of eternal truth" (2016, p. 191). Scerri's antirealist position is then akin to that of Kuhn, who famously suggested that the development of science is best characterized not as that of an enterprise getting nearer and nearer to "some goal set by nature in advance," but rather, "in terms of evolution from the community's state of knowledge at any given time" (1962/1970, p. 171).

In short, every next writer in the minority tradition of study of multiple discoveries in philosophy of science aims at correcting or ameliorating the Kuhnian account, and yet, in spite of other disagreements, everyone concurs on two intertwined claims that Kuhn put forward. The first is that, in view of the essentially communal nature of scientific inquiry, one ought to vigorously reject the myth of the lone individual who, by their sheer brilliance, propels the progress of science, which is a product of the community as a whole. The second is the most important for our present purposes: background knowledge, providing scientific communities with the shared platform that they need to conduct their business, plays a crucial role in the explanation of the pervasiveness of multiple discoveries, which does not lend itself to straightforward realist readings, but instead, can be explained in antirealist terms pretty easily. Not that realists need to deny the important aspects of science as an organized, collective enterprise that trigger multiple discoveries, so crucial in the accounts by Kuhn, Lamb and Easton, and Scerri. Quite on the contrary, realists will readily acknowledge that shared background knowledge—the "weight of the past" on current scientific practice, so to speak—has a role to play in the explanation of their pervasiveness. As mentioned, though, they can suggest that one should supplement such an explanation with a further, more committing claim, whereby since success is a fallible indicator of (approximate) truth, the profusion of multiple discoveries has to be viewed as a consequence of science's overall success, and so vindicates realist accounts of science.

What the foregoing suggests, to our mind, is that the phenomenon of multiple discovery as such is neutral with respect to the realism/antirealism debate, precisely as it is with respect to the inevitability/contingency controversy. Such a neutrality, we submit, depends on basically the same reason in both cases. Radick (2005, p. 24) has argued that while realism and inevitability, on the one hand, and antirealism and contingency, on the other hand, seem to suggest themselves as natural, obvious pairings, one can put forward an account of science and its history that is both antirealist and inevitabilist, as well as an account that is both realist and contingentist. Soler (2008b, p. 231) has analogously suggested that since there is no full overlap between the inevitability thesis and realism, on the one hand, and the contingency thesis and antirealism, on the other hand, the inevitability/contingency controversy is logically independent of the realism/antirealism debate (see also Kinzel, 2015, pp. 58–60). When one attends to the pervasiveness of multiple discoveries in science, the situation is entirely similar from the structural point of view, in that the phenomenon does not align in natural, obvious ways with either realism or antirealism. This, we maintain, speaks against the second facet of the inevitability insight, and suggests that realists should resist the temptation of making too much of multiple discoveries as such. Consider again, for instance, the multiple discovery of the periodic table of elements, mentioned at the beginning of this paper. What makes that result particularly noteworthy is not so much the circumstance that it was independently arrived at by several individuals: after all, also errors can be independently arrived at (see Seeman, 2018 for the first systematic investigation of the phenomenon of multiple errors). From the viewpoint of the realist that result is important, rather, because it exhibits a number of attractive properties, chief among them that it allowed to make confirmed predictions, for instance, concerning the existence of yet to be discovered chemical elements and some of their key properties (see, e.g., Barnes, 2008). Despite possessing some intuitive force, we maintain, also the second facet of the inevitabilist insight must then be rejected.

Let us conclude the present section by noting that the claim that multiple discoveries are neutral with respect to the realism/antirealism debate gains further plausibility when one looks at the phenomenon in light of path dependence in the development of science, which manifests itself in various forms, one of which is the important role played by background knowledge. As mentioned above, one can describe accepted background knowledge as the weight of the past on current scientific practice—as the outcome of the accumulation of the choices made by the relevant scientific communities in the course of time. To put it differently, background knowledge is part and parcel of accepted scientific results' laying on the developmental path followed by the branch of science to which they belong. A result's laying on such a path, however, does not in itself tell anything conclusive concerning whether the result (and indeed, the whole path) should be interpreted in realist or antirealist terms.

To clarify the point, let us adapt for our purposes Mark Peacock's (2009, pp. 118–120) discussion of path dependence in the production of scientific knowledge. Imagine a scientific community that accepts theory T_1 as the best explanation of a phenomenon of interest, P. Imagine, moreover, someone suggesting that the community's acceptance of T_1 must be accounted for purely in terms of the vagaries of history: had the relevant branch of science followed a different path (something that is entirely conceivable, had certain boundary conditions been different), today the community may accept the alternative theory T_2 instead of T_1 as the best explanation of P. Were it possible to show that T_2 is in fact, in some relevant regard (e.g., predictive accuracy, closeness to the truth, and the like), superior to T_1, this would amount to a case of "market failure" in science, analogous to the cases in which the past technological choices made by the competitors in a given market lead to an inferior technology dominating at the expense of a superior one. As Peacock points out, however, a judgment concerning the superiority of either T_1 or T_2 needs to be based on a criterion that is independent of the positions that T_1 and T_2 respectively occupy. The fact that T_1 lays on the actual path of development of a certain branch of science certainly helps to explain its acceptance, which one can view as the endpoint of a process in which background knowledge (the decisions made in the past by the relevant communities) accumulates. The fact that the community embraces T_1, however, does not in itself suffice to show its superiority. Nor does the fact that T_2 lays outside of the path culminating with the acceptance of T_1 in itself suffice to show the superiority of T_2 (or its inferiority, for that matter). In Peacock's terms, there may be market failures in science, but it is far from clear that we have ways to ascertain whether, in instances that may interest us, a market failure has in fact occurred or not. Applying such a line of reasoning to those results of science that are called "multiple discoveries," we suggest, highlights that one should not make too much of a multiple discovery's occupying a certain position in the developmental path of a branch of science.

5. Concluding remarks

One would expect multiple discoveries to draw the attention of philosophers of science, since they raise, among others, interesting questions concerning both the relatively recent debate on the inevitability/contingency of science and the age-old realism/antirealism debate. In particular, multiple discoveries quite naturally elicit what we call the "inevitabilist insight"—the insight that there is something inevitable to results on which several individuals working independently from one another converge. We contend that one should reject both the two facets of the insight analyzed here, since the phenomenon of multiple discoveries does not speak in favor of the inevitabilist side of the inevitability/contingency controversy, nor does its pervasiveness speak in favor of realist accounts of science.

The contribution of this paper has been twofold. First, we put forward the idea that multiple discoveries should not remain a topic cultivated only by a minority tradition. Second, we addressed some questions arising in connection with the phenomenon of multiple discoveries and with its implications for important philosophical debates. If successful, then, our arguments dispel the air of inevitability surrounding multiple

discoveries and exhibit some apparently viable answers to those questions as, in fact, dead ends. These negative results, we maintain, are nevertheless important, in view of the intuitive force of the idea that a connection obtains between multiple discoveries, inevitability, and scientific realism.

Let us conclude by emphasizing the conviction underlying both of the negative conclusions defended here: the path that a certain (branch of a) science has already followed heavily contributes to molding its further development, whose inevitability, however, one should not overstate. As we suggest in section 3, given points of departure and research questions significantly different from the ones of our actual sciences, one could get significantly different results—multiply arrived at as frequently as those of our sciences. And as we point out in section 4, accepted background knowledge—the "weight of the past" on current practice—plays a crucial role in the development of science, in light of which a realist account of the pervasiveness of multiple discoveries turns out to be less immediately obvious than some may be tempted to believe. This is not to imply that a certain (branch of a) science could successfully develop in any direction towards which one chooses to push it. In fact, various proponents of sophisticated versions of scientific realism (see, e.g., Niiniluoto, 1999; Kuipers, 2000) have shown that one can describe the world by means of indefinitely many conceptual systems, none of which enjoys a privileged status, but not all of them will have the same success. As the late Paul Feyerabend once put it, "not all approaches to 'reality' are successful. Like unfit mutations, some approaches linger for a while—their agents suffer, many die—and then disappear" (1999, p. 215). Still, we suggest, there is far more room for alternative developments of science than the inevitability thesis and certain crude forms of realism allow.

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