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ARTICLE

From Unobservable to Observable: Scientific Realism and the Discovery of Radium

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Abstract

I explore the process of changes in the observability of entities and objects in science and how such changes impact two key issues in the scientific realism debate: the claim that predictively successful elements of past science are retained in current scientific theories, and the inductive defense of a specific version of inference to the best explanation with respect to unobservables. I provide a case-study of the discovery of radium by Marie Curie in order to show that the observability of some entities can change and that such changes are relevant for arguments seeking to establish the reliability of success-to-truth inferences with respect to unobservables.

Keywords Scientific realism · Inference to the best explanation · Observability

1 Introduction

There are many points of dispute in the debate between scientific realists and anti-realists. One of them concerns the claim that theoretical content from past empirically successful theories is retained in current theory. In a seminal paper against scientific realism, Laudan (1981) showed that scientific theories could be empirically successful and yet still be false, breaking the essential explanatory connection between empirical success and truth. Entities postulated in empirically successful but false theories—phlogiston, ether or crystalline spheres—were abandoned as science progressed. In response to Laudan's argument, contemporary versions of selective realism, for example the 'Divide et Impera' realism advocated by Psillos (1999), and the 'working posit' realism forwarded by Kitcher (1995), argued that we can infer the existence of theoretical entities posited in science in so far as they are indispensable for a theory's predictive success. The shift in focus from whole theories and empirical success to parts of theories and predictive success filters out many of Laudan's counterexamples and promises the inverse result: theoretical entities that satisfy these conditions of indispensability for predictive success have not been abandoned, but rather retained in the scientific image during theory-change, and are therefore likely to remain in our future scientific image. Thus, the realist concludes, we can be realists about

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these entities. The core of the debate is whether or not the conditions for explanatory inference set up by the realist only capture the entities (or structures, if you are a structural realist) that have been retained, or the abandoned ones as well.

Another point of dispute is the reliability of explanatory reasoning in general, or the legitimacy of reasoning in accordance with inference to the best explanation (IBE). This form of inference has been argued to be unjustified by virtue of vicious circularity (Fine 1991), and on the grounds that it fails to transmit warrant to the selected theory because of underdetermination (Stanford 2006). Realists have on their part provided various strategies in favor of IBE including explanatory defenses by Boyd (1983) and Psillos (1999; 2007), as well as inductive defenses from Kitcher (2001), Douven (2002) and Bird (2006). The core to this dispute resides in the fact that realists struggle to convince anti-realists that IBE is reliable on anti-realist terms. Anti-realists, in the form of empiricists, demand observable confirmation of the reliability of IBE when applied to unobservables. One can see why this demand has been hard to meet.

In this paper, I address an overlooked issue that has bearing on both these points of dispute—that the concept of an entity can change during the different stages of the scientific process. When I refer to 'conceptual changes' I mean the characterization or understanding of the entity from the scientist's perspective. I do not mean to say that a conceptual change physically or metaphysically alters or impacts the entity in any way, shape, or form. When I talk about the conceptual changes of an entity it should be understood as changes in the scientist's conception or characterization of the entity, not as a change in the entity itself. Plausibly, the concepts of entities sometimes undergo a change with respect to their status in a theory once theoretical or empirical progress is made. One of the ways in which these changes manifests itself is when an entity goes from being considered theoretical to being considered empirical, or for the observability of the entity to go from unknown to known. Such conceptual changes can make it difficult to assess whether one should categorize entities as abandoned or retained, thus impacting the first dispute. But they may also provide the empirical confirmation of the reliability of IBE that realists have been looking for: an entity of which the observability is unknown can be inferred at one stage in the process by virtue of its role as indispensable for predictive success. Later, the conceptual understanding of the entity is changed once one knows that it is, in fact, observable. The observability of the entity is then decoupled from the actual success of the explanatory inference, but can be used to confirm its reliability. As a case study of how this plays out I use the discovery of radium. Note that there are two separate claims pursued in this paper. The first claim is that the observability status of an entity can change in response to empirical and theoretical developments. The second claim is that cases involving such changes can be used in a proof of concept for a justification of IBE with respect to unobservables.[1]

2 The Two Problems

Much ink has been used in the long and intricate history of scientific realism. Even though many are familiar with the core arguments, it makes sense to devote some more ink to giving a recapitulation of the arguments central to the aim of this paper.

[1] Thanks to an anonymous referee for suggesting a clarification of these claims.

2.1 Are Entities in Predictively Successful Theories Retained or Abandoned?

The core of what is known as the 'Divide et impera' or selective approach to realism, championed by Psillos (1999) and to a certain extent Kitcher (1995), can be viewed as a way to deal with the problem of empirically successful but false theories in the history of science. Laudan (1981) famously argued that since there are many examples of theories that are empirically successful but false, this fact undermines the realist idea that we ought to believe that empirically successful theories are true and that their entities exist. Entities like phlogiston, ether and crystalline spheres were part and parcel of once predictively successful theories but have since been abandoned, and justifiably so. One of the ways in which the realist responded to the historical challenge was to argue that one should not view the empirical success of a theory as a function of the *whole* theory, thereby throwing out the baby with the bath water. Instead, one should focus on the part of the theory that was indispensable for the predictive success. If one does, or so the realist says, it will become apparent that these parts of the theory have actually been retained in succeeding theoretical frameworks:

> [...] even a quick glance at current science suggests that there is a host of entities, laws, processes and mechanisms posited by past theories—such as the gene, the atom, kinetic energy, the chemical bond, the electromagnetic field, etc.—which have survived a number of revolutions to be contained in our current theories. (Psillos 1999, 104)

So, the argument goes, we *do* have reason to believe that there is a connection between predictive success and truth, it was just a much more specific connection than it was previously thought to be. It is important, however, to point out that the selective realism associated with this move does not generate any general realism with respect to science or scientific theories—one has to identify which theoretical parts essentially contributed to the predictive success of a theory and then show that, and how, these parts have been retained.

The success of the selective realist strategy is, as Lyons (2006) points out, dependent on two central questions: (i) exactly what the criteria are for determining whether entities or structures in scientific theories are 'essential', 'indispensable' or 'responsible' for novel predictive success (as well as how 'novelty' should be understood), and; (ii) whether an answer to (i) is even applicable to any scientific context. Part of the problem associated with (ii) is that it can be hard to understand when an entity is retained in a succeeding theory. If the understanding of the entity and its properties change when theoretical or empirical progress is made, does that constitute retaining or abandoning?

2.2 The Reliability of IBE

IBE, being an ampliative inference, is always going to be fallible. Nevertheless, one may argue that it is reliable in the sense that it gets it right most of the times. If it does, one may use it to form rational warranted beliefs about the world. While there may be many ways to defend IBE, I will focus on two defenses coherent with the selective realist position: the explanationist and the inductive defense.

2.2.1 The Explanationist Defense of IBE

In the 1980s, Boyd (1983; 1980) provided several different versions of the no-miracles argument in order to defend scientific realism. The main line of reasoning being that realism about scientific theories is the only plausible scientific explanation for the instrumental reliability of scientific methodology. Boyd's refinement of the NMA focuses on the success of theory-driven scientific methodology. The best explanation for this methodological success is, according to Boyd, scientific realism. Fine (1991) rejects the strategy of defending scientific realism by using IBE, claiming it to be question-begging and viciously circular:

> To use explanatory success to ground belief in realism, as the explanationist defense does, is to employ the very type of argument whose cogency is the question under discussion. In this light the explanationist defense seems a paradigm case of begging the question, involving a circularity so small as to make its viciousness apparent. (Fine 1991, 82)

In response to this explanationist counterpart of Hume's justificatory challenge, Psillos (1999) uses a distinction between rule-circularity and premise-circularity, claiming that only the latter is obviously vicious.[2] According to Psillos, a premise-circular argument is one where the conclusion is identical to, or a paraphrase of, one of the premises in the argument, while a rule-circular argument is one where the argument is itself an instance of the rule vindicated in the conclusion. (Psillos 1999, 82) The difference between the two, Psillos claims, is that while premise-circularity must be viciously circular given that the conclusion is presupposed in the premise, rule-circular arguments are at least not obviously viciously circular given that the conclusions of ampliative inferences are always stronger than their premises. He then defends the claim that rule-circular arguments are, in fact, not vicious. Since, according to Psillos, Boyd's argument is an instance of the former, Fine's objection misses the mark. Recently, Carter and Pritchard (2017) argue that a further distinction between narrow rule-circularity and wide rule-circularity provides reason to doubt that the rule-circularity of Boyd's kind is innocuous. Even supposing that Psillos' rebuttal of Fine's argument is sound, there are grounds to worry about the dialectical efficacy of the explanationist defense. How is the empiricist supposed to be convinced that IBE is legitimate when the rule used in the argument arriving at that conclusion is IBE? If you are an IBE skeptic, you are not likely to accept a justification of it that relies on IBE, even if it is not viciously circular. The explanationist defense of IBE appears, at least in so far as it is meant to convince an empiricist, to be a dead end for realists.

3 The Inductive Defense of IBE

Prima facie, giving up on the explanationist defense for an inductive defense of IBE has two benefits. First, it does not face any threat of vicious circularity, which means that it bypasses Fine's challenge.[3] Second, it promises to be dialectically useful against

[2] This distinction is also present in Papineau (1997), and Boghossian (2001).

[3] One may of course argue that because both IBE and induction are ampliative inferences, there are reasons to think that justifying one using the other could lead to circularity (perhaps of the vicious kind). Such an argument would have to be made under the premise that IBE and induction are sufficiently similar, by virtue of being ampliative inferences, for circularity to become a potential issue. This premise is, to my knowl-

empiricists by virtue of using an inference rule that empiricists accept. I will review two inductive approaches, and two corresponding objections to them, which provide a good basis for evaluating the overall plausibility of inductive defenses of IBE.[4]

3.1 The Galilean Strategy

The main proposal in the so called 'Galilean Strategy' by Kitcher (2001) is that we can test whether or not IBE has been successful in observational contexts, and then generalize that reliability to include unobservable contexts. Thus, the Galilean Strategy is a two-step argument that we may recreate in its generalised form:

1. IBE is reliable in observable contexts.
2. We have no good reason to suppose that it will stop being reliable in unobservable contexts.

∴ IBE is reliable in unobservable contexts.

The first step needs to establish the reliability of IBE in observable contexts. To this end, Kitcher offers a rather plausible set of arguments of the following sort:

> People find themselves in all sorts of everyday situations in which objects are temporarily inaccessible, or are inaccessible to only some of the parties. Detectives infer the identities of criminals by constructing predictively successful stories about the crime, bridge players make bold contracts by arriving at predictively successful views about the distribution of the cards, and in both instances the conclusions they reached can sometimes be verified subsequently. (Kitcher 2001, 176)

Kitcher's suggestion is this: in observational contexts where objects are temporarily unobservable, we can entertain a host of theories, some of which will prove to be successful and others not. At some later time, when the same objects are no longer unobservable, one will find out which theories were true and, according to Kitcher, also find a strong correlation between success and truth. The core of the argument, then, is that:

> [...] realists think that everyday experience supports a correlation between success and truth. They deny that empiricists can simply stipulate the limits of reliability of this correlation. (Kitcher 2001, 178)

Kitcher's argument is inductive in so far as the reliability of the success-to-truth inference is premised on its past successes, whereby success can be confirmed by observation. The second step of the argument is that it is 'metaphysical hubris' to suppose that IBE's limit of reliability should happen to correlate with the limits of human perception.

It is in the second step of the argument that things start to become vexed. Why, exactly, should the empiricist have to accept that the lack of a defeater for the continued reliability of IBE from observable to unobservable entities provides reason to believe that this

Footnote 3 (continued)

edge, not accepted by any of the participants in the realism debate. See Boghossian (2001; 2007) and Carter and Pritchard (2017) for a general discussion on inferential justification and circularity.

[4] Similar ways of defending IBE inductively have been presented, but not necessarily defended, in Harré (1986), and Douven (2002).

reliability holds? As Magnus (2003) argues, the lack of such reasons is not enough to establish that such inferences are reliable regarding unobservables. Empiricists may simply remind Kitcher that no inductive evidence for the reliability of IBE with respect to the unobservable has been presented. While it might be true that there are no reasons to think that the reliability of IBE stops at the limit of human perception, there are no reasons to think that it continues either. The reliability of IBE with respect to unobservables cannot, on Kitcher's account, be confirmed observationally.

3.2 Bird's Defense

Bird (2006) defends the legitimacy of explanatory inference in science by pointing to the fact that being an unobservable is a contingent fact. An inductive defense of IBE can exploit these contingencies to show that inferences to unobservables can be made, and later be confirmed observationally:

> [Explanatory] inferences to the existence of unobservables have later been verified by direct observation once observational techniques have improved. We can now observe microbes and molecules, the existence of which was once a purely theoretical, explanatory hypothesis. (Bird 2006, 160)

The argument draws on the fact that technological progress has increased our epistemic boundaries so as to allow previously unobservable entities to become observable, suggesting that we can observationally confirm inferences to unobservables. Bird's argument seems to contain precisely the element that was lacking in Kitcher's—the success of IBE with respect to unobservables. To convince the empiricist, however, it too fares poorly. The cases clearly lack *observational* confirmation of the success of IBE with respect to the unobservable, even supposing that IBE is successful. Bird references successful inferences to microbes and molecules, but those are precisely the kind of entities that empiricists would say are unobservable. Such instances of IBE, while possibly successful, cannot be shown to be successful on empiricists terms.

3.3 The Problem with Inductive Defenses of IBE

It is now clear what the underlying problem of the inductive approach to defending IBE really is: the strategy of providing successful inferences to unobservables implies a failure of confirmation of that success by observation. The strategy of providing observational confirmation of successful inferences, in terms of observing the inferred objects or the conclusion of the inference, only works when objects are observables. We may think of the inductive defense as amounting to two claims:

(A) Instances of IBE with respect to unobservables have been successful.
(B) Confirmation of inferential success in such cases can be obtained by observation.

For the inductive defense to work against the empiricist, both (A) and (B) must be the case, since (A) is the claim that is defended, and (B) is the only dialectically viable route to defend (A). It is easy to see, however, that if (A) is satisfied then $\neg$ (B) must be the case. This is because whatever epistemology one may proceed with in order to satisfy (A), it could not possibly be (B) given an empiricist understanding of the epistemic salience of observability. Conversely, if (B) is satisfied then $\neg$ (A) must be the case. This is because the methodological assumption that (B) is satisfied leaves no possible route to satisfying (A). Again, this is due to the empiricist understanding of the unobservable/observable distinction. If we *can* confirm that inferences have been successful by observation, those inferences were made to observables. Any justification of such inferences does not extend to unobservables.

The very specific class of inferences that realists need to be reliable—inferences to unobservables—have conclusions that cannot be observationally confirmed. To argue for the general reliability of IBE, of which inferences to unobservables would be a subset, would run into two objections. The first objection is the empiricist classic rebuttal of sheep and lambs, which states that there is no reason for empiricists to venture beyond what the actual evidence, in this case successful inferences with respect to observables, shows. Even if some available epistemology shows that IBE is a generally reliable mode of reasoning, empiricists simply do not have to accept that this entails the reliability of IBE with respect to unobservables. It is entirely possible that the reliability of IBE is context-dependent and restricted to observables, and there is no evidence of the sort empiricists accept that can show that this possibility is false. The second objection, targeting Kitcher's defense, is that a generalized defense of IBE would imply that metaphysicians using IBE in their theorizing would be as justified as scientific realists are. In other words, there would be no way to stop the metaphysical inflation of scientific realism, given that metaphysicians use IBE. If IBE is generally reliable, there is no reason why this reliability should not extend to metaphysics as well.[5] Between these two objections, a generalization of the reliability of IBE appears to be too costly a route to venture for realists.

We may now take stock of the defenses of IBE that I have considered. The explanationist defense was charged with vicious circularity because it uses IBE as a rule to justify the claim that IBE is justified. Even if Psillos and others have argued that this rule-circularity is not to be conflated with its viciously premise-circular cousin, the defense will ultimately suffer from dialectical issues. Perhaps such a strategy is effective against someone who is on the fence with respect to IBE, but anti-realists in general, and constructive empiricists in particular, are not. The inductive defense of IBE came in two different forms. Kitcher's defense tried to establish the reliability of IBE by testing it in an observable environment, a move the consequences of which opponents would have to accept. Once this reliability could be established, the defense was supplemented with an argument claiming that since we have no reason to think that reliability of IBE correlates with observation, IBE is reliable with respect to the unobservable. Bird's inductive defense claimed that we may observe the successes of IBE with respect to unobservables when observational technology enables us to catch up with our inferences. As we have seen, Bird's defense neglects the empiricist's epistemic divide between observable/unobservable and so cannot prove the alleged success of IBE with respect to unobservables without begging the question against the empiricist. Kitcher's defense suffers from the inverse problem. He can prove the success of IBE, but only with respect to observables.

The empiricist distinction between observable and unobservable appears to be insurmountable for a dialectically successful defense of IBE. Any evidence for the reliability of IBE with respect to unobservables must somehow contain successful inferences to unobservables that can be observationally confirmed. A tall order, to say the least.

[5] This line of argument has been expressed by many metaphysicians who aim to establish the epistemic legitimacy of metaphysics by reference to scientific realism. See Paul (2012), Swoyer (2008), and Colyvan (2012; 2006).

4 Change in Conceptual Understanding

One move in Bird's defense of IBE was to employ the shift in the status of an entity from unobservable to observable as related to different technical improvements. Even though the claim that microbes and molecules are observable would not ultimately suffice to convince the empiricist, the shift is an interesting phenomenon worth exploring. Is it really the case that the scientific conceptual understanding of what entities are unobservable is rigid? Or can the concepts sometimes change? The interesting class of entities that realists claim we ought to be realists about are precisely those that are indispensable for a theory's predictive success, so in this context a theoretical entity should be construed as whatever entities occupy the intersection between unobservables and theoretical entities.[6]

In what follows, I claim that objects posited by theories based on IBE have been reconceptualized with respect to their observability. The reasoning here departs from Bird and in this sense I aim to bridge the dialectical gap between realists and empiricists by staying within the boundaries of observability. To get a proof of concept, the challenge is to provide a case of successful use of success-to-truth inferences to entities not yet considered observable such that those inferences can be observationally confirmed. There are, in fact, many contenders for this sort of case. Consider the missing elements inferred by Mendeleev from his original model of the periodic table. According to Mendeleev, the gaps in his model corresponded to actual elements (Stewart 2019). Eka-aluminium, eka-boron, and eka-silicon were theoretical entities at the time, inferred from structural properties of the scientific model. Their observability was wholly unknown, given that the observability of the elements was dependent on the specific physical and chemical properties of the elements themselves. Though they may have been *detectable* at the time, detection is not sufficient for empiricist commitment. The inferred elements were simply placeholders with values assigned by the structural properties of the model. The subsequent discovery and production of these missing elements, given the names Gallium, Scandium, and Germanium respectively, prompted a conceptual change with respect to their observability. The process of the change in conceptual understanding of Gallium went via detection to observation, since the first evidence for its existence was obtained by spectroscopic analysis, before subsequent observable evidence in the form of a sample of the free metal was obtained by electrolysis. I will focus on a particular instance of such a case—the discovery of radium—and its affinity with explanatory reasoning.

5 A Case Study of the Discovery of Radium

When looking at the history of the discovery of radium, it is clear that its postulation entered when trying to explain an unexpected parameter value in an experiment. When quantifying the levels of radiation of uranium by measuring the level of electric current in an apparatus (an electrometer with a piece of piezo-electric quartz) Marie Curie saw that the pure uranium sample was less radioactive than the mineral compound from which the uranium was refined. The unexpected levels of radiation could be

[6] We need not be concerned with entities that are unobservable but do not figure in scientific theories, such as spirits or ghosts, since such objects are not claimed to exist by the realist. Similarly, we need not be concerned with entities that figure in scientific theories but are observable, like zebras or trees, since these are precisely the entities that realists and empiricists agree are real.

accommodated if the compound contained an unknown additional radioactive element that was discarded in the process of extracting the uranium. The line of reasoning here looks much like IBE, inferring the existence of a novel element based on the fact that it would explain the data. At this stage, the element is hypothesized, but is crucially still not considered an observable element in the sense that no proper detection or observation of it had been made. As was custom at the time, any claim regarding the existence of novel elements had to be subjected to spectroscopic analysis. If there really was a new element, evidence of this would show up in the analysis. The Curies took their samples to spectrum specialist Eugène-Anatole Demarçay, who found a strong line in the spectrum at 3814.8 Ångström, concluding that:

> It does not seem possible to me that this line can be attributed to any known element [...] Neither barium nor lead from elsewhere [i.e. from sources other than the Curies' material], as I have assured myself, give any line which coincides with it. (Demarçay 1898, 175–178)

At this point, one may associate the hypothesized element with a measured empirical value—the lines in the spectroscope. In other words, there is an isolated frequency at a certain value in the spectrum not attributable to any known element. There is a clear sense in which the hypothesis at this point enjoyed a certain degree of predictive success—the hypothesis predicted the presence of a unique line associated with a unique wavelength (although not an exact location in the spectrum). In addition, the hypothesis could accommodate previously known phenomena such as the results from the electrometer experiment. The results from Demarçay's analysis prompted the Curies to make an inference from the success of the radium hypothesis to its truth:

> The various reasons which we have just enumerated lead us to believe that the new radioactive substance contains a new element to which we propose to give the name radium. (Curie et al. 1898, 1216)

At this stage, the inference is made very clearly, and a certain degree of predictive success for the hypothesis has been established. We can see how confidence in the truth of the hypothesis correlates with its increasing empirical success. Despite the predictive success associated with the spectroscopic results, the scientific community at the time disagreed with the Curies' conclusion. The existence of radium had to be conclusively demonstrated. The Curies famously labored for years in a makeshift laboratory trying to isolate a quantity of pure radium chloride. In August 1902, after processing over 8 tons of the ore pitchblende discarded from mines in St. Joachimsthal, Marie and Pierre Curie had finally produced one decigram of pure radium chloride, the unique glowing properties of which confirmed its existence. Marie Curie and André Debierne would some years later also manage to isolate radium in its metallic form from a solution of radium chloride using electrolysis, adding to the total conceptual change of the understanding of radium as an observable (Ropp 2012).

It is important to point out that there were no theoretical or experimental reasons for anyone in the scientific community at the time to think that radium was an observable prior to the laborious efforts of the Curies. It was still a possibility that the critical mass of radium would be below the point of observability, in which case any experimental attempt to isolate an observable amount of it would have failed. The very reason why experiments like the one carried out by the Curies was, and still are, carried out in science is to generate new information about the inferred object. In this case, part of what was sought to know was if radium was stable enough that one could amass an observable amount of it to prove its existence to those who doubted it, and to better study its properties. There were a lot of things about radium that the Curies did not know, among them whether it was observable or not, which was precisely the motivation behind their experiment:

> The results from the study of radioactive minerals that I described in the last chapter drove Mr Curie and myself to try to extract a to date unknown radioactive substance. [My translation] (Curie 2020, 34)

The take-away point here, which is sufficient for the present purposes, is that no contemporary theoretically informed reason existed to think that radium was observable. One might think that insofar radium was considered an element, it would have been considered a not-yet-observed observable. This reasoning strikes me as anachronistic. At the time, there was no understanding of how elements were constituted, given that atomism was a highly contested and underdeveloped theory about the constitution and structure of matter. Additionally, the periodic table was still a living scientific document which lacked a proper theoretical underpinning, so could not be used in order to provide definitive statements about the observability of putative elements. In fact, even after Curie produced radium salt, she remained uncertain about the prospects of repeating this process with other radioactive elements:

> Of all the newly discovered radioactive substances radium is the only one that has been managed to be produced in the form of pure salt. [My translation] (Curie 2020, 36)

What makes the radium-case especially interesting is that Marie and Pierre Curie empirically tested the radium hypothesis by making radium observable in the empiricist's sense rather than by extracting characteristic numbers and testing those empirically.[7]

6 Implications for Realism

The radium case shows that entities can undergo a series of conceptual transformations as a theory progresses, which shows that entities considered unobservable are not doomed to end up on the scrapyard of science, but are neither retained as unobservable entities in subsequent theoretical frameworks either. Importantly, these changes matter a great deal to the progress of science and the epistemic status of entities in science. Empirical regularities have corresponding underlying causes. Sometimes, these causes are observables and sometimes they are not. Inferences to those causes are made by experts in the field, with relevant background knowledge of facts which restrict theory space with respect to possible explanations for the regularities. When inferred causes are successfully used to generate novel predictions, the scientists know their inference was good. Fallible, perhaps, but justified nonetheless. The evidence for the warrant of this inferential practice can be clearly seen in cases where scientists' conceptual understanding of entities changes as theories develop. The immediate intuition about such changes is that they may be used by the realist in order to show how explanatory inferences to theoretical entities can be observationally confirmed. After all, this looks precisely like the kind of evidence that an inductive defense

[7] The discovery of helium had a similar structure, where Norman Lockyer in 1868 inferred the presence of a new element—helium—from a unique line in the spectrum of light from the sun. This inference was confirmed in 1895 by Sir William Ramsey. Many thanks to John Norton for bringing the similarity between these cases to my attention.

of IBE needed. An inference from the predictive success of a theory based on the postulation of an entity to its truth is made in circumstances where the observability status of the entity is unknown. This inference can later be confirmed once the entity has been established to be an observable.

In the radium-case, the important question to ask is whether the Curies where justified in believing that radium was real—because the theory of radium was predictively successful and could explain previous experimental results—even though the observability of radium was unknown. My claim is this: their explanatory success-to-truth inference was made prior to knowing the observability of radium, and it was justified. It would have been equally justified even if radium had turned out to be unobservable due to, say, having a different critical mass. To argue that they were not justified at the time, even when they were correct, would imply that the Curies simply got lucky, which would be a strange claim. The realist claim must be viewed from the contemporary perspective of the Curies themselves. *They* inferred the existence of radium based on explanatory power and predictive success, and their inference was both justified and correct, observability or not.

Under the assumption that realists can show that in most cases, IBE has been able to pick out the relevant entities prior to scientists knowing their observability, their much-needed justification of IBE with respect to unobservable entities can be inductively defended. I will not consider the obvious objection against the feasibility of amassing enough cases for a proper induction. Since my aim is to show that there are no conceptual or logical issues that prohibit using cases of the above kind as the base in an inductive defense of IBE, I will instead focus on two possible objections to that claim. The first is that the change in observability constitutes a version of a failure of referential stability during theory change. The second is that the radium-case shows that radium was an observable all along, so could not constitute a successful case of success-to-truth inference to unobservables. I will address both objections in order.

6.1 An Empiricist Objection

One troubling aspect of realism is how to deal with entities during theory change. This argument is applicable to the context of conceptual changes to entities as well. If the status of an entity changes from unobservable to observable, which is what realists would argue in this case, then the object which is allegedly picked out by the term ‘radium’ is not considered the same object before its observation as after. In other words, empiricists would say that once an entity is observable, it is a fundamentally different entity. Radium as an unobservable, or theoretical postulate, is abandoned, while radium as an observable is discovered. So what should the realist say about radium? Is it a successful instance of a case where predictive success and indispensability led to radium being retained? Or is it in fact the case that radium *as an unobservable entity* became abandoned once its observability became known? The first inference to the existence of an additional element did not contain any reference to observability and selective realists must make it clear that there is a sufficient overlap in how the scientists perceive the entities in order to substantiate the claim that such entities are retained through the different stages in the scientific process. There is a straightforward answer to this challenge. The only conceptual change that radium went through via the process of producing observable quantities of it was precisely that it was observable. The stability of the hypothesis did not depend on radium being observable since all features of radium could have been identified without generating an observable amount of it. Observability is certainly not a sufficient criterion for distinguishing between entities and so cannot be used to separate pre-observed radium from post-observed radium in order for referential instability to ensue.

6.2 Another Empiricist Objection

The empiricist may object and say that the radium-case is not all it promised to be, since the entity in question was an observable all along. If we could observe it, which Curie showed that we can, then it is clearly an observable. This means that the inference was to an observable all along, and so cannot constitute a case for the reliability of IBE with respect to unobservables. This is essentially a slightly altered re-run of Magnus's (2003) argument against Kitcher. The question is whether it makes sense for a constructive empiricist to say that radium was an observable at the time when its observability status was unknown. In order to claim that it was observable all along, empiricists need to say that observability is objective, or factive, i.e. a relation that holds between humans and entities whether we know it or not. We might call this notion 'factive observability' and say that an object is observable iff its observability follows from a set of facts about the actual world. We may distinguish factive observability from 'epistemic observability', where the latter denotes our knowledge about these facts, or our knowledge about what is observable. Since epistemic observability is a subset of factive observability, they are not denoting the same things: objects can be observable in the factive sense without being observable in the epistemic sense, as the radium-case clearly demonstrates. Now, empirical adequacy says that we should believe what science tells us about observables, but in what sense of observable? Here, Monton and van Fraassen (2003) explicate how to understand claims of observability:

> Consider the claim 'if the moons of Jupiter were present to us (in the right kind of circumstances) then we would observe them'. [...] The way to understand the claim is to note that, even though it is a counterfactual, it is entailed by facts about the world: facts that the moons of Jupiter are constituted in a certain way, and facts that we are constituted in a certain way. These facts can be disclosed by empirical research. In practice, not all the empirical research has been done, so we have to rely on our current best theories to determine what these facts are. (Monton and van Fraassen 2003, 415)

I see no problem in determining the observability of objects when the empirical facts about them *have* been disclosed, i.e. when we have knowledge about the facts that entail their observability. But what should we say about objects which are observable according to some set of facts which is *not* yet available to us? Before 1902, nobody had access to the facts that entailed that the constitution of radium made it observable—neither atomism nor the periodic table contained any fact from which one could deduce the observability of radium. Radium, then, was inferred to exist before it was determined to be an observable. This means that the success-to-truth inference was operating "in the blind" as it were. No facts about the observability of radium entered in the inference machinery, and yet the inference was successful. The success of the inference, however, is entirely decoupled from the observability of the inferred object. The only thing that the observability of the object contributes in such cases is to our evaluation of the legitimacy of the inference. An inference is made to the existence of an object, the observability of which is unknown, based on predictive success and explanatory power. The success of the inference is also unknown at this time. The subsequent empirical research shows that the object, in fact, is observable. This fact about the objects observability is detached from the initial inference but can be used to determine that the inference was, in fact, successful. It is easy to see why in the counter-factual case. If radium had turned out to be an unobservable, it would have had no impact whatsoever on the actual success of the inference, but it would have had an impact on our determining the success of the inference.

Furthermore, if empirical adequacy is supposed to tell us what we should believe but furnishes those beliefs with observables only once we know that they are observable, it is hard to see what philosophical and epistemic benefits constructive empiricism offers over its naïve counterpart. The break with naïve empiricism is precisely the commitment to the observ*able* rather than to the *observed* (Contessa 2006), but basing observability on what we know to be observable collapses the distinction between them. The factive notion of observability also implies that scientists should have believed in the existence of radium even before they knew that it was observable. In other words, they should have believed in the existence of an entity that was not considered to be observable. But applied to our current theories, this is precisely the dispute that empiricists have with realists—whether or not to believe in the existence of currently postulated entities, the observability of which is not yet disclosed. To use factive observability as a guide to ontological commitment implies belief in (some) entities currently considered not to be observable. Surely this goes against the spirit of constructive empiricism.

7 Summary

One of the core issues in contemporary scientific realism is how to convincingly argue for the reliability of IBE with respect to unobservables. In this paper, I provided a diagnosis of the problems facing different such defenses of IBE as well as a possible solution for how to deal with them. The solution, building on work by Bird, was to look more closely at cases in the history of science where explanatory inferences from predictive success to truth were made to entities whose observability was undetermined at the time, but were at a later stage found to be observable. This move was able to bridge the dialectical gap that Bird's defense of IBE could not. As a case-study, I used the history of the discovery of radium by Marie Curie, which contained explanatory inferences from predictive success to truth prior to the time when she established the observability of radium. The case showed that success-to-truth inferences can be successful in a way that ought to be dialectically efficient against the empiricist. Two potential empiricist objections to the radium-case as an instance of an observationally successful success-to-truth inference to unobservables were considered but were either too weak or coupled with internal problematic implications for the empiricist.

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