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Discovery, theory change and structural realism

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Abstract In this paper I consider two accounts of scientific discovery, Robert Hudson's and Peter Achinstein's. I assess their relative success and I show that while both approaches are similar in promising ways, and address experimental discoveries well, they could address the concerns of the discovery sceptic more explicitly than they do. I also explore the implications of their inability to address purely theoretical discoveries, such as those often made in mathematical physics. I do so by showing that extending Hudson's or Achinstein's account to such cases can sometimes provide a misleading analysis about who ought to be credited as a discoverer. In the final sections of the paper I work out some revisions to the Hudson/Achinstein account by drawing from a so-called structural realist view of theory change. Finally, I show how such a modified account of discovery can answer sceptical critics such as Musgrave or Woolgar without producing misleading analyses about who ought to receive credit as a discoverer in cases from the mathematical sciences. I illustrate the usefulness of this approach by providing an analysis of the case of the discovery of the Casimir effect.

Keywords Discovery · Hudson · Achinstein · Casimir effect · Structural realism · Semirealism · Scientific realism

1 Introduction

When we say so and so discovered x, for example "Priestley discovered oxygen," we cannot mean that the discoverer's beliefs about the discovered are all either true or match exactly our current beliefs. After all, whomever you choose to call the discoverer of oxygen, Priestley or Lavoisier or someone else, presumably had lots of

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false beliefs about what they discovered. Priestley for example thought that oxygen was "dephlogisticated air". Lavoisier thought it to be very pure ordinary air, not a special separable component of it. The fact that our current understanding of oxygen, or any other discovered objects, is extraordinarily different from these preliminary descriptions has led some philosophers to consider the notion of "discovery" to be problematic. Kuhn (1977) and Musgrave (1976), for example, have both rejected the idea that discovery can be attributed to any particular instance, such as isolating a first laboratory sample. For both thinkers discovery is the end result of a complicated process involving more than identifying that something has been discovered but also involving the evolution of our contemporary understanding of what has been discovered. If such arguments are taken to their logical conclusion, the notion of discovery will probably need to be dispensed with altogether since our beliefs about things like oxygen or electrons are not going to be the final scientific word on the subject. Taking the full implications of Kuhn's point you might say that we still have not discovered oxygen since we still do not have a complete and final grip on what we have discovered.

Related objections to discovery come from the social constructivists such as Woolgar (1989). Scientific beliefs about the constitution and constituents of the world, in this view, are simply the end results of a process whereby scientific knowledge is socially constructed. In this way, you cannot really say something has ever been discovered, simply that it has been invented or constructed. However, in this view you also cannot sensibly talk about the unique originator of a concept in which we have come to believe. You could not say, for example, that Thompson was the originator of the concept of the electron as opposed to its discoverer, since the current concept is quite different from his. In a fashion parallel to the ideas of Kuhn and Musgrave, it is not simply a matter that a concept has been constructed, but it is also a matter of what exactly our concepts and beliefs about that constructed object are.

Nevertheless discovery does seem to be a central notion in science, and is frequently used by and debated among scientists themselves. Thus, some philosophers persist in trying to provide an account of discovery. In this paper I will consider two accounts that attempt to do so. I will assess their relative success and show that while both approaches provide some promising similarities they do not, as they stand, fully address the concerns of the discovery sceptic. To this I add the novel objection that these accounts of discovery sometimes provide misleading analyses about who ought to be credited as a discoverer. To make this point I consider in some detail the case of the discovery of the Casimir effect. Finally, in the last sections of the paper I work out some revisions to the accounts of discovery that I discuss by drawing from a so-called structural realist view of theory change in order to try to come to grips with what counts as being a discovery. I do not reject the accounts of discovery I deal with, but rather I will offer an alternative account that takes the limitations of these accounts as its point of departure. Specifically, I will show how these accounts, while fine for accounting for experimental discoveries, need to be supplemented by a modified understanding of discovery that can account for discoveries of a theoretical or mathematical nature. Finally I try to show how such a modified account of discovery can answer critics such as Musgrave or Woolgar without producing misleading analyses about who ought to receive credit as a discoverer.

2 Hudson and Achinstein on discovery

In this section of the paper I will review two similar accounts of discovery: Robert Hudson's "base description" account and Peter Achinstein's "epistemic situation" view. It will emerge that these two positions, while not identical, are substantially similar in important respects and together they can be taken to form the basic ingredients of a reasonably plausible account. However, it will also emerge that they suffer from some shortcomings that leave them unable to fully respond to possible objections that arise from the perspective of those sceptical of the notion of discovery.

2.1 Hudson

The key feature of Hudson's account of discovery rests on what he refers to as the "base description". For Hudson, what is central to being counted as discovering a given object is to be the first to provide a base description of the discovered object. The base description is a description of an object that suffices to identify it, "something that satisfies the base description is the object in question" (Hudson 2001, p. 77). In this view the base description must be a rough and ready affair. A base description can identify an object but does not specify identity conditions. For example a base description might fail to obtain but the object might still be present, or the description might obtain and the object might not be present. To illustrate this point Hudson considers the example of Sudbury's famous giant nickel as a base description suitable to identify the city of Sudbury. Should the nickel be moved the description would fail to obtain in Sudbury but the city would still be there, likewise the new location of the nickel would not be Sudbury. Nevertheless, according to Hudson that base description serves nicely under "normal circumstances" to identify the city. For Hudson this is enough given what he takes to be the general capacity of humans to recognise and distinguish objects and natural kinds from one another.

However, providing a base description is not all by itself sufficient to count as discovering something. In addition to providing a base description a discoverer must "materially demonstrate that the base description has been satisfied" (Hudson 2001, p. 79). The final criteria that must be satisfied in order for someone to qualify as a discoverer is that she must be the first in the group or community to which she belongs to find the object in question. In this way, Columbus was hardly the first person to arrive in America, but he was the first Renaissance era European to do so. Finally, the object discovered must really be what it is claimed. That is, a truth condition applies to the base description.

For Hudson, this account possesses significant advantages that permit it to respond to critics of the notion that specific individuals or groups can be identified as unique discoverers. And, claims Hudson, the account permits the adjudication of situations where discovery claims are in competition. He considers in some detail the various competing historical accounts of the discovery of oxygen, some advancing Priestley as the discoverer, others advancing Lavoisier. The upshot of the analysis is that he comes in on the side that defends Priestley. While Lavoisier isolated oxygen before Priestley did, he did not realise that he had isolated a separate component of air; he though he had simply obtained a very pure sample of ordinary air. Priestley, on the other hand, had not only isolated oxygen. He was the first to combine this with a sufficient and materially demonstrated base description. Priestley's base description, and subsequent material demonstration, not only included an account of oxygen's properties, its aiding combustion and its lightening of breath and so on, but also that oxygen was a separate component of air and not just especially fresh air. While Priestley held many false ideas about oxygen, he thought for example that it was "dephlogisticated" air, his base description is still satisfactory to identify the substance he discovered as oxygen. Moreover, he was the first in the scientific community to introduce and materially demonstrate this particular base description.[1]

To close this discussion of Hudson's ideas about discovery, it is fairly easy to see how his account responds to critics of discovery. The central notion in the Kuhn/Musgrave critique is that Priestley (or any other putative discoverer) did not, by anything resembling today's standards, know what he had discovered. However, for Hudson, this does not disqualify him. Priestley was the first to introduce the concept of oxygen as a separate substance with a base description that we can still use to identify what he had isolated as oxygen (even as it is understood today). Thus, in spite of our very different understanding of oxygen's properties, Priestley's base description is still adequate to describe and identify oxygen in a basic but nevertheless sufficient way. Woolgar's social constructivist notions can be dispensed with in a similar fashion. The fact that our ideas about our currently constructed notion of oxygen are very different from Priestley's does not disqualify Priestley as discoverer. After all, his description still holds true and brooks material demonstration that is not relative to a given social group.

2.2 Achinstein

Achinstein advances an account of discovery that, as we shall see, bears considerable resemblance to Hudson's. Like Hudson's approach, Achinstein's possesses both an ontological and epistemological component. First, to have discovered a thing x, x must exist and one must know that x exists. Recall that for Hudson x must exist and the discoverer must materially demonstrate its presence. Additionally, the account of the object, as far as it goes, must be true. Again in a manner similar to Hudson, Achinstein requires that a discoverer be the first person in a community to know of the presence of the discovered object. Thus, "P was the first person in some group to know that x exists, to be caused to believe that x exists from observations of x or its direct effects, and to have reason to believe that x exists, that x or its direct effects have been observed" (Achinstein 2001, p. 269). Achinstein also emphasises that to discover something you must be in an "epistemic situation", that is to say, you must

[1] Hudson indicates that there is some reason to believe that Priestley may actually have accomplished this after it had already been done by Carl Scheele, in which case Scheele ought to count as the discoverer (Hudson 2001, p. 83; cf. Also Scheele 1777; Patrington 1961). Leaving the details of that historical debate aside, between the two contenders Lavoisier and Priestley, Hudson is clear that his account comes down in favour of Priestley.

be caused to believe that x exists. By this Achinstein is referring to an experimental or observational situation that yields the data that count as reasons to believe in x.

The important feature of Hudson's account is his notion of the base description that serves to identify the discovered object. The parallel role in Achinstein's account is played by the observation of the object or its direct effects. Oxygen of course cannot be observed but its direct effects can, and the account of these makes up the base description. In Achinstein's account, the observation of oxygen's effects—its compressibility, its aiding combustion, its lightening of respiration, etc.—would count as the reason to believe that the substance in question is oxygen and that it is a separate component of air. Of course these very features of oxygen comprise Priestley's base description. The process of observing and describing oxygen's direct effects then neatly matches Hudson's notion of a base description and his requirement for material demonstration. In other words when Achinstein says the discoverer must know that the discovered object "x exists" amounts to something quite close, but not exactly the same, to knowing that "some x possessed of a given base description" exists.

The central example that Achinstein uses to illustrate his account is Thompson's discovery of the electron. Like the earlier discovery of oxygen, several scientists can compete legitimately for the title of discoverer and Achinstein agrees that awarding the title must follow careful historical analysis. Nevertheless, for the purposes of this paper, let us assume that Thompson was the first to provide the main features of his account of electrons. Based on his observations of cathode rays, Thompson concluded, among other things, that he was observing the direct effects of charged particles much smaller than atoms and, a little later, that they were constituents of atoms. Now, this account clearly counts as a reasonably good base description in Hudson's sense. Thompson's experiments on cathode rays also count as observations of direct effects. For example, they are deflected in a way that a very small negatively charged body would do when passing through a magnetic field (cf. Achinstein 2001, p. 284). The two accounts give the same result: Thompson was the first to materially demonstrate a base description exactly as he was the first to be in an epistemic situation, through his experiments, to observe the direct effects of electrons.

Similarly, Achinstein's account responds to discovery sceptics in a manner parallel to Hudson. It is not enough, for example, to experimentally manipulate an object in question. After all, Lavoisier had isolated oxygen before Priestley. Similarly a number of researchers such as Lenard and Shuster had manipulated electrons before Thompson (cf. Achinstein 2001, p. 275). An account of the experimental manipulation such that it establishes a sufficient description (i.e. base description) of what is manipulated must also be provided. Likewise, it is not enough to hypothesise something on the basis of theory, you need also to observe the object or its effects (switching to Hudson's terms: you need to materially demonstrate the base description). Similarly Achinstein dispenses with constructivist accounts that doubt the idea of unique discovery. There is for Achinstein, as there is for Hudson, a fact of the matter of who was first (in Achinstein's parlance) to be in an epistemic situation to observe an object or its direct effects, however it might be conceived today.

In spite of their similarities, it must be noted that Hudson and Achinstein's accounts are not exactly the same. Achinstein for example makes some stronger requirements than Hudson about knowing the discovered object in question. Achinstein identifies a strong sense in which one knows what one has discovered. He does not go so far as to say Thompson would need to know that what he discovered were electrons, in our sense of the term, for him to be named the discoverer in this strong sense. But Achinstein (2001, p. 281) does note that he must at least know quite a bit about the constituents of cathode rays that makes up our current concept of the electron: that they are charged particles, smaller than an atom, with a mass to charge ratio is 10^{-7}. For Hudson, it is enough to have provided and materially demonstrated the base description that will *subsequently* be used to identify the discovered object. He does not require the discoverer to know that this is the base description that will count as the deciding factor in crediting the discovery or require knowing what has been discovered in a sense that would rule out having certain false beliefs about it. As Hudson (2001, p. 90) notes, Priestley mainly conceptualised oxygen as dephlogisticated air, a false belief, but he still materially demonstrated the base description of oxygen being a separate component of air which identifies him as its discoverer. Priestly would not quite measure up to the stronger sense of discovery that Achinstein identifies (but would count in a weaker sense that Achinstein also discusses). Hudson does not draw a distinction between two grades of discovery as Achinstein does.

3 The limitations of the Hudson/Achinstein view

As we have seen, although their views are by no means identical, Hudson and Achinstein do defend very similar positions. What is most appealing about the resulting view is the notion of a base description that need not be identical to what we believe in today, yet is still close enough to identify the object in question.[2] However, it is precisely this slightly vague account of the base description that leads to some shortcomings in the view. The modifications of the view that I will defend at a later point will focus on a more defensible account of the base description.

The main feature of the Kuhn/Musgrave critique, as Hudson and Achinstein present it, is the emphasis on knowing what has been discovered. However, a sceptic of this type would be unlikely to be convinced by the Hudson/Achinstein account of the "basic enough" description. The reason for this is, I think, that the Hudson/Achinstein view provides an inadequate account of theory change. The reason why a sceptic like Kuhn claims that a discoverer such as Priestley did not know what he had found is because our conception of oxygen has changed so much that we cannot really say that what we now call oxygen is the same thing. Priestley lacked a molecular understanding of chemistry and he lacked any atomic theory. The current understanding of oxygen has a profoundly different basis. Given all this, oxygen cannot really be said to be the same thing Priestley believed in. And, since Priestley was not even the first to isolate a sample of what we would now call oxygen, what is the basis on which to name him discoverer? Similar arguments might be made against Thompson. After all, he was not the first to work with cathode rays or electricity for that matter, and is the electron as

[2] The phrase "base description" is Hudson's. Achinstein speaks of describing "causal effects of X that can yield knowledge of X's existence" (p. 270) however the ideas are close enough for my purposes. I will use Hudson's terminology for the rest of this paper.

it is described by contemporary quantum electrodynamics in any way the same thing conceived by Thompson?

The difficulty lies with the vagueness of what Hudson calls the base description. Essentially, the base description must be suitable to identify the object in question but whether it does so or not is, for him, a matter to be settled on a case by case basis. The context of material demonstration, or epistemic situation for Achinstein, is supposed to settle this. For example, performing Thompson's tests still works to identify the constituents of the deflected cathode rays as electrons. This is perfectly true; however, a Kuhn-style sceptic could simply point out that such tests and actions combined with the base description serve to identify something *other* than what Thompson thought he had discovered. In other words, the base description can attach to quite different things. And Hudson, as we have seen, is happy to admit this. Consider the oxygen case again. Lavoisier's base description of his sample includes the observations that it aids combustion, is compressible and lightens respiration. Such a rough and ready account is more or less as sound by today's standards as Priestley's. The only difference is Priestley's contention that oxygen is a separate component of air, but given the hugely different understanding that we now have of what that means, it hardly serves to identify him as originating today's concept. The overlapping parts of Lavoisier's base description might also serve in an experiment to identify something as oxygen.

None of this is to contend against Hudson that Lavoisier is the real discoverer of oxygen, or that Thompson ought not to be identified as discovering the electron. My point here is simply that the account of the base description of the discovered object that we find from Achinstein and Hudson is not, as it currently stands, adequate to address some of the concerns that arise from the point of view of the discovery-sceptic.

The difficulty noted above is not the only problem with Hudson and Achinstein's account. While the view accounts for historical examples such as the discovery of oxygen or electrons well, it is not clear that their criteria do an equally good job of dealing with other quite common situations where we might plausibly attribute the title of discoverer to someone. The two examples we have looked at, oxygen and electrons, are similar in several very important respects. First, Priestley was not possessed of a detailed account of either oxygen or its direct effects when he began his investigations, nor was Thompson possessed of a similar account of electrons. Their descriptions emerged from their laboratory investigations. Now, for Hudson and Achinstein, if someone had speculated about the existence of such objects this would not be enough to count as a discovery because it lacks the material demonstration necessary to count as a discovery. While this seems perfectly appropriate for those two examples, the material demonstration requirement might not seem so intuitively plausible when applied to the objects that are investigated by much contemporary science.

To be sure, one might imagine lots of situations in chemistry where someone might produce an unfamiliar molecule resulting from a novel reaction, then describe and materially demonstrate that molecule. However, in physics, a large number of situations exist where an object or effect is specifically being sought because it is mandated by theory. In such a situation we might pose the question of whether or not the first person to materially demonstrate the presence of an object or its effects ought to count as its discoverer. After all, this title might be well claimed by the person who demonstrated that the object or effect is mandated by well-confirmed theory. In the next section I will develop this point by considering one very well known example of this sort of expected discovery: the discovery and demonstration of the Casimir effect. I will show that the Hudson/Achinstein account actually produces results that contradict the mainstream opinion of the scientific community.

Hudson and Achinstein are both clear that their account of discovery does not account for purely mathematical discoveries, and my discussion that follows does not claim that either writer hoped to do so with their account. However, as it will become clear, in fundamental physics if not in many special sciences, such mathematical discovery is an inseparably important part of physical discovery. I propose, then, not a rival to the Hudson/Achinstein view but rather an extension of it (through an improved account of the base description) that can deal with such cases.

4 The discovery of the Casimir effect

The Casimir effect is a consequence of quantum theories of the vacuum. According to quantum field theory vacuums, even at absolute zero, are not devoid of energy. The reason for this is the continuous appearance and disappearance of so-called virtual particles. All fields, and electromagnetic fields in particular, fluctuate in their energy around a mean number. The fluctuations result from the appearance and disappearance of virtual particles at all wavelengths. In the case of the electromagnetic field in a vacuum at absolute zero the mean energy is half of the energy of a photon. Thus, the vacuum contains, among other things, a non-zero electromagnetic force even at absolute zero.

In 1948 Hendrik Casimir was investigating the movement of molecules in colloids, in particular the effects of van der Waals forces. Casimir's investigations led him to conclude that understanding the electromagnetic forces acting on molecules in colloids must take into account the finite speed of light (that is, the finite speed of the photon, the particle carrying the electromagnetic force). Casimir realised that this result could be re-interpreted in terms of quantum vacuum fluctuations and it gave rise to his now much discussed thought experiment. The thought experiment considers the results of positioning two perfect plane mirrors facing one-another at an extremely close distance in a vacuum at absolute zero. While classical physics states that nothing at all will happen, Casimir takes into account the affects of the vacuum fluctuations.

While a vacuum is filled with virtual photons of all energies (i.e. electromagnetic waves of all wavelengths) only the effects of those wavelengths that fit a whole number of times into the space between the mirrors ought to be taken into account. Wavelengths that do not fit a whole number of times into the gap are not reflected back by the mirrors, and wavelengths that do are multiplied. Electromagnetic radiation, like all radiation, exerts pressure on surfaces, including the mirrors in the thought experiment. Thus, at certain distances the amplification of waves that fit into the cavity, that match the cavity resonance, exert more pressure on the mirrors than the waves outside the cavity. However, as the mirrors are moved closer together, fewer and fewer waves match the cavity resonance and thus fewer contribute to the vacuum pressure. When this occurs the pressure exerted by the surrounding space becomes greater than the pressure in the cavity and the net result is a force pushing the mirrors together (cf. Casimir 1948;

Lamoreaux 1997). The strength of the force is proportional to the area of the mirrors and increases by a factor of 16 when the distance is halved. When other fundamental constants such as Plank's constant and the speed of light are taken into account the force can be calculated according to the following relation $F = \pi$ h c A/480 d^4 where h is plank's constant and c is the speed of light, a is area, and d is the distance between the mirrors (cf., Casimir 1948).

Casimir's exact experiment has remained a thought experiment since his calculation assumes a temperature of absolute zero and perfect mirrors, and thus it is impossible to conduct the experiment. Moreover, since changes in temperature and imperfections on the mirror surface change the calculation of the force and because the force is extremely slight, many technological advances were necessary before the force could be measured in the laboratory. However, in 1996 Steven Lamoreaux successfully measured the Casimir force between a small 4 cm sphere and an optical lens, both coated in copper and gold. A torsion pendulum, a twistable metal bar, connects the two objects. When the objects are moved very close to each other they are drawn together by the Casimir force, measurably twisting the pendulum. Lamoreaux's results match theoretical predictions within five percent (Lamoreaux 1997). Since that experiment was conducted a large number of papers have confirmed Lamoreaux's result. That work is already having an impact on the study of microelectronics where distances are getting close enough to require taking the Casimir force into account. The research is also influencing the study of gravity at close distances between small objects where the gravitational force needs to be parsed from the Casimir force. In spite of these successes certain features of the force are still poorly understood, for example its exact effect on the insides of very small hollow spheres is not currently known.

Now it is perfectly clear that both Hudson and Achinstein would name Lamoreaux as the discoverer of the Casimir effect and would set the date of discovery as the date of his torsion pendulum experiments. After all, Lamoreaux was the first to materially demonstrate the presence of the effect or observe and describe its direct effects. Casimir predicted the force but never observed it, so he cannot count as discoverer in their view. However, the basic account of the Casimir force was provided by Casimir himself and Lamoreaux's contribution to the base description of the Casimir force just consists in compensating for the fact that he was not doing his experiment at absolute zero or with perfect mirrors. Moreover, the scientific literature on the subject universally credits Casimir with the discovery and Lamoreaux has never contradicted this or claimed credit as the effect's discoverer (characteristic examples from the scientific literature that name Casimir as discoverer include Lamoreaux 1997, 2008; Mohideen and Roy 1998; Andrews and Romero 2001). He simply claims to have been the first to observe and measure the effect and is happy to directly identify Casimir as discoverer noting, "The last great fundamental discovery in quantum mechanics was made in 1948 by Hendrik B. G. Casimir" (Lamoreaux 2008, p. 1). In fact, unlike the situation with oxygen or the electron, there is a remarkable amount of unanimity in the scientific community on the subject of the Casimir effect. The interesting feature of the Casimir effect example is that the Hudson/Achinstein account of discovery unambiguously names a discoverer who does not claim to have discovered the effect! And, it does this against the nearly unanimous opinion (that includes Lamoreaux's) that credits Casimir with the discovery of the effect that bears his name. This result is unfortunate for the

Hudson/Achinstein account since it is supposed to help settle disputes by providing an account of what counts as discovery, but in this case it seems to artificially create a controversy where none actually exists.

As I noted earlier, Hudson and Achinstein explicitly note that their account of discovery covers those situations where an effect or object is observed and not situations where the existence of something is mathematically derived. However, this restriction is of little help, since of course Lamoreaux did observe the Casimir effect yet to name him its discoverer seems wrong since he does not claim the title. There are discoveries that are not strictly mathematical in nature but nevertheless don't correspond with the Hudson/Achinstein account of material demonstration. The lesson here seems to be that in the highly mathematical sciences discovery often involves the sort of purely mathematical derivation made by Casimir even if it not exclusively comprised by it. Similar processes attend much recent discovery and research in particle physics such as the searches for the top quark and Higg's boson. Before considering what additions ought to be made to the Hudson/Achinstein account, let us look in a little more detail at the Casimir effect's discovery and examine who discovered what using as much of Hudson and Achinstein's language as we can.

Casimir can be said to have discovered that the effect ought to exist and that he provided the base description (short of its material demonstration). Yet given the well-confirmed theories he was using, he felt he had reason to believe in it. To borrow terminology from Achinstein, Casimir felt himself to be in an epistemic situation that warranted his claim. He thus felt he had sufficient evidence to believe in the effect. Obviously, we cannot say anyone could at that time know for sure that the effect was genuine since it had not been demonstrated. In Achinstein's account of discovery for example, Casimir must always be ruled out since he could not know for sure that his effect existed. Nevertheless, we can say that Casimir and also most scientists in the field accepted the arguments for the effect's existence. What we might say for Lamoreaux is that he demonstrated *that* Casimir was correct. His own experiments add much to our knowledge (such as clarifying the effect's behaviour at higher temperatures), but his experimental set up openly acknowledges its use of Casimir's base description of the effect and its causes and its properties that can be exploited to detect it. Lamoreaux was the first to observe the effect, and he claims to be. But in some situations this is not quite enough to qualify you as the discoverer, since account must be taken of experiments that exploit a mathematically derived base description of the discovered object's observable effects. We might still express this lesson in Hudson's or Achinstein's terms and add this caveat to their position: if the first to experimentally demonstrate something relied on another's earlier base description, being the first to materially demonstrate that base description is not sufficient to claim credit as discoverer. However, as the next section makes clear we still need a clearer account of the base description if we are to answer the objections of the discovery sceptic.

5 Discovery and structural realism

Still using Hudson's terminology, let us look closely at some details of Casimir's base description to see how it can be used to respond to the discovery-sceptics such as Kuhn or Musgrave. Essentially, Casimir noted that even in a perfect vacuum at absolute zero there is some energy. He also identified the relation that this energy obeys when acting on parallel reflective plates (the resulting force varies inversely with the distance). These are the main ingredients of the base description that were exploited by subsequent scientists when they sought to confirm the effect's existence. Now Casimir also provided an explanation for the effect based on his understanding of the quantum field theory of his day, and he also acknowledged that gaps existed in his account's ability to predict the effect's behaviour in certain circumstances such as on the interior of a hollow sphere. The basic explanation of the effect today is more or less the same Casimir's. Nevertheless, a sceptic might point out that a radical alteration in our understanding of quantum theory (something that is by no means impossible) could alter our understanding of the effect to the point where it cannot be said to be the same thing Casimir described. However, not everything in the base description would change in such a circumstance. One example is the basic relation describing the attraction between the plates. And of course it was this basic relation that was exploited by Lamoreaux and others in their experimental manipulations that make use of use the effect. What we can say Casimir discovered under any circumstances, then, is that a force ought to exist between reflective plates in vacuums at absolute zero and the relation that describes the behaviour of this force.

Moving from this conclusion we can be more specific than Hudson or Achinstein about what features of a base description do the work of identifying its originator as a discoverer. What is essential in Casimir's base description, then, is not necessarily the interpretation of the explanation of the object or discovered effect, but the relations that the effect obeys, regardless of the explanatory interpretation. Thus, even major theoretical upheavals do not dethrone discoverers from their title since what is important is their identification of basic relations that are exploited later in experiments, not their explanatory interpretations of why the particular relations obtain. Since even a very different understanding of why Casimir's relation obtains still acknowledges that the relation obtains, no subsequent theoretical upheaval could change the fact that it was he who provided the first account of that relation.

This account of what is important in a base description matches neatly the increasingly popular account of theory changes favoured by the so-called structural realists such as Worrall (1989). Structural realism is a view about theory change whose supporters claim that equations that are retained across instances of theory change identify relations that are at least approximately true. This forms the explanation of why exactly the equations are in fact retained. Nevertheless, while the structural realist commits to knowledge of structure (i.e. the equations that describe relations), she does not commit to knowledge of any "nature" of the entities in the retained equations that goes in any way beyond the relations engaged in. The relations believed in are those that exist between measurable quantities, as characterised by measurement procedures and that can be expressed in mathematical terms. For structural realists the view represents a compromise position. It can accommodate major conceptual breaks in our understanding of theoretical constituents during instances of theory change while at the same time avoiding the difficulties of conceptual relativism that attach to the early views of Kuhn. From a structural realist perspective, the history of science is replete with instances where despite major conceptual disruptions, equations are retained in new theories. The explanation for this is that while a new theory or framework might revise many of the features of the entities named by equations, it still needs to accommodate the observed relations accounted for by the previous theory. Thus, while structural realism identifies quite a lot of continuity between old and new theories, it also recognizes that many of the constituents named by theories do not survive. Examples are plentiful and well known: they include things such as the “ether”, “caloric”, “phlogiston”, etc.

From a structural realist perspective what can be known about theoretical entities are the relations they engage in, and in fact this is how they are defined. A useful presentation for my purposes is Chakravartty’s (1998, 2003). He defines the properties of a theoretical entity as the dispositions for law-like interactions that it engages in. Following from this, he holds that what can be known about a theoretical entity are those properties that are exploited in its detection. Science provides knowledge of those dispositions for regular behaviour (expressed in law-like terms, i.e. as equations) that are involved in experimental interactions with the entity. If we adopt this view we can suppose that the equations of old theories that survive theory change (that express these dispositions or interaction properties) describe (approximately) real relations that hold between whatever happens to exist in situations where old equations are still utilised in the new theory. Since the constituents of theories are defined in terms of their properties, i.e. structurally, and since some structure survives theory change, some continuity exists between old and new theories.

Chakravartty’s version of structuralism, that I follow here, does not adopt a thoroughgoing structuralist metaphysics or deny that objects or concrete particulars exist. In this way it retains some features of more traditional scientific realism; thus Chakravartty dubs his version of structuralism “semirealism”. In this view an unobservable entity is known to science through experimental interactions that exploit its dispositions to act in certain ways under certain circumstances. Semirealism divides the properties of unobservables into two types: detection properties and auxiliary properties. The former represents, as noted in the previous paragraph, the law-like causal properties of an unobserved entity whereby its existence can be detected. Auxiliary properties, on the other hand, represent other causal properties the entity may have but that are not involved in detection (Chakravartty 1998, pp. 394–395; 2007, pp. 64–65). In this view science gives us knowledge of detection properties but not auxiliary properties. When reference to such entities survives an instance of theory change, their detection properties will be retained but not necessarily their auxiliary properties. These detection properties are just what the structural realist has in mind by structural relations. This sort of position implies that the nature of an entity can be defined by its properties and what is known of it is its detection properties. However, an entity can have a detection property (a disposition to behave in certain circumstances in a law-like way) without instantiating that property at all times. As Chakravartty notes, “quantities of gas are not always expanding” (2007, p. 85).

Chakravartty is contrasting his view with versions of structuralism that deny the existence of objects or particulars and hold instead that only relations exist. This position has grave problems that Chakravartty is keen to avoid.[3] The metaphysical view associated with his position is one of concrete particulars defined as "property instances that cohere at specifiable space–time locations" (2007, p. 81). A particular can be either an object or event. This view has certain advantages: first it avoids the problems of its main structuralist rival that takes relations to be all that exists. And, as we shall see below, its feature of focusing on detection properties (dispositional properties for certain law-like behaviours which serve to identify a given object) that survive theory change will become important in forming an answer to the discovery sceptic.

Let us return briefly to the imaginary scenario where our basic understanding of quantum vacuum undergoes a fundamental revision due to some major overhaul of basic physical theory. As we noted earlier, it is possible that this might cause a major revision in our understanding of the Casimir effect. In fact, the explanation of the effect might be wholly revised such that its explanation seems unrecognisable from the point of view expressed in Casimir's initial papers on the subject. Nevertheless, certain features of the base description provided by Casimir would remain under any circumstances. Specifically, these include the essential relation describing the effect's behaviour that has subsequently been exploited in laboratory interactions. To rephrase this in the structural realist terminology I have been proposing, what is essential in a base description is the identification of basic interaction properties of measurable quantities (i.e. dispositions for law-like behaviour) that are utilised in experimental and detection situations.

Someone might object here that while Casimir's account identifies the relations obtaining between measurable quantities (the dispositions of what is measured), it couldn't count as a discovery in the event of a major shift in theory. This could be claimed because Casimir relies on old theory that might be revised so much that

[3] Although a complete overview of the structural realism (SR) literature and associated metaphysical debates is beyond the scope of this paper, a brief survey is warranted to justify the use of the version of SR that I favour. Worrall's (1989) version has been much criticised by Psillos (1995, 2001), Ladyman (1998) and others on the basis that it implies a false distinction between structure and nature, the latter remaining mysterious in Worrall's version. Chakravartty avoids that difficulty by identifying the nature of an entity with the (mathematically expressed) properties exploited in its detection, Chakravartty also claims in his early (1998) paper that his view can also be reconciled with entity realism. While McArthur (2006a) has shown that this might be problematic, this feature is not emphasised by Chakravartty's (2003, 2007) current work on dispositional properties nor is it required for my account in this paper. Ladyman (1998) and French and Ladyman (2003a,b) have proposed an alternative formulation of SR with a metaphysics that denies the existence of any nature, entities, objects or particulars, insisting instead than only structure exists. This is a much more controversial metaphysical proposal than Chakravartty's, who defines an entity or particular's nature in terms of its properties but does not deny that there is any nature or particulars that have a nature. As such the French/Ladyman view has several grave problems not shared by Chakravartty's. As Psillos has noted (2001) their view must maintain a notion of isomorphism without the notion of paired entities. Cao (2003a,b) has noted that since it denies the existence of anything but structure it can stifle the search for any potential underlying causes for the observed structure. McArthur (2006a) has noted that it suffers from a regress problem since claiming that only structure exists amounts to claiming that all relations are internal. Moreover, Chakravartty's metaphysics explicitly contains features (dispositional properties that persist across instances of theory change) that can answer the discovery sceptic and Ladyman's has no such feature. Given all this, and since it solves no problems that Chakravartty's does not (cf. McArthur 2003, 2006b), recourse to Ladyman's implausible version of SR is unnecessary as is the adoption of the attendant controversial and implausible metaphysics.

his account amounts to only a lucky suggestion and not really a discovery since it follows from rejected theory. However, it must be noted that to derive the effect, Casimir was drawing from already (and subsequently) well-confirmed relations (relating to the electrodynamics of vacuums). Thus, his prediction relies on the exact sort of structural relations that do survive theory change. So Casimir's view that the effect "ought" to be observed relies largely on (even in his day) established relations between experimentally measurable quantities, relations that would have to be preserved and accounted for in any subsequent theory. In other words, his account relies on what Chakravartty calls detection or dispositional properties. And, of course there are very many well-confirmed experiments and effects, in Casimir's day and earlier, that attribute a non-zero energy to the vacuum and that also establish its distinct dispositional properties. And of course it is the dispositional properties of the vacuum that Casimir uses to derive his account of the effect. What is important in identifying Casimir as the discoverer of the effect that bears his name, then, is not just that he used the theory to derive the effect, but that he was the first to use some of its already-confirmed relations to provide the base description of the effect that was later exploited in experiments. Of course nobody could say that Casimir's discovery amounted to more than the identification of a consequence of a hitherto very well confirmed theory (replete with many established structural relations between measurable quantities) until *after* Lamoreaux's material demonstration that Casimir was correct. However, after this confirmation of the effect, we can unproblematically identify Casimir as the discoverer of his effect and as providing the base description that was eventually used to make that demonstration.

At this point it is worthwhile to highlight where my account of discovery departs from the other accounts I have discussed. Since I emphasise the use of well-confirmed theoretical relations to derive a base description of something not yet experimentally observed, my approach represents a significant departure from either Hudson or Achinstein's view. When deciding how to credit a discovery claim, both place a heavy emphasis on the material demonstration of the discovered thing and not on the details of the creation of the base description. In this sense the account I argue for here should not be regarded so much as an extension of the Hudson/Achinstein type of account but rather as an alternative approach that is meant to cover theoretical or mathematical discoveries not covered by the Hudson/Achinstein view. The need for such a theory of discovery that covers this sort of case is highlighted by our discussion of the Casimir effect.

6 Lessons from structural realism

Although I have contended all along that the Hudson/Achinstein account does provide us with a firm basis for understanding experimental sorts of discovery, there are some limitations in the position. First, while the Hudson/Achinstein account can rebut the objections from discovery sceptics such as Kuhn or Musgrave, structural realism lets us put that response on a more specific footing. Looking at discovery from the structural realist perspective lets us be very specific about what in the base description permits a response to the accusation that current theory cannot be said to be referring to the same object that was discussed by the putative discoverer. Put another way, to answer the discovery sceptic, any theory about base descriptions that underpins discovery claims must be able to provide an account of how the base description fares in instances of conceptually disruptive theory change. Hudson and Achinstein are correct that their materially demonstrated base description fares well enough in cases of theory change to answer the sceptic, but structural realism lets us zero in on exactly why this is so. The second more important limitation of their view is just that it is limited to experimental discoveries. When one tries to apply their view outside that domain, problems quickly arise. As the Casimir case shows, in some sciences, especially mathematical physics, the requirement for material demonstration sometimes produces a skewed analysis of who ought to be counted as a discoverer. In both cases I think that the discussion of structural realism provided in the last section provides a congenial improvement.

A structural realist view of theory change lets us identify what part of a theory can be regarded as a least approximately true even after a very comprehensive theory change. It identifies, according to the version of structural realism that I have drawn from, the dispositions for law-like behaviour that are possessed by whatever happens to exist as holding true (as at least limiting cases) across instances of theory change. Since this part of a base description remains stable no matter what conceptual revisions might subsequently take place, a putative discoverer who is identified as the first to provide the basic disposition properties retains this title regardless of later developments. After all, regardless of how we come to conceptualise the quantum vacuum or the electron in years to come, the basic law-like dispositions (expressed as equations) prevailing between measurable quantities that are used in experimental interactions will still hold. While a structural realist must acknowledge that other aspects our understanding of the discovered object might well change, this at least remains stable. Thus, a structural realist account of discovery identifies the discoverer as the first to provide a description of the relevant dispositional properties and relations. Given this, a theory change cannot dislodge a discovery claim. Thus, the discovery-sceptic is answered. Adopting a structural realist account of discovery lets us focus on what is essential in the base description, the experimentally exploitable dispositional properties. As such, in the Casimir effect case being the first to provide a material demonstration that the properties hold true, as Lamoreaux was, is not relevant to being counted as a discoverer. This is so because by itself this does not generate the crucial part of the base description of the discovered object: the experimentally exploitable dispositional properties.[4]

The structural realist view, when applied to either experimental or theoretical discoveries, clarifies what we can say we know about what has been discovered. We know that at least the base description of what has been discovered obtains and that this is comprised of those equations that represent the experimentally exploitable dispositional properties. It is just these properties remain part of our understanding of the discovered entity even in cases of theory change. This lets us answer sceptics who insist that conceptual breaks undermine a discoverer's claim to have discovered the

[4] We will say of Lamoreaux exactly what he said of himself, that he was the first to observe and record the consequences of Casimir's theoretical discovery. Theoretical or mathematical discoveries are just that, theoretical, as such they do not require material demonstration, although if they entail observable effects, experimental demonstration firmly secures the correctness of those predictions.

thing (such as the electron) that we now conceive very differently. With regards to theoretical discoveries specifically, such as the Casimir case, whatever else we might someday say about it, we know who discovered it and provided us with some of its experimentally exploitable dispositional properties and when he did. If we identify a discovered object with its experimentally exploitable properties then not only can we answer the sceptic but we can also focus explicitly on exactly what a putative discoverer must provide in order to be credited with the title.

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