Kuhn on Scientific Discovery

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Abstract

In this chapter I review Kuhn's account of discovery. Kuhn held that a scientific discovery requires both a discovery *that* an object exists and a discovery *what* that object is. Accordingly, Kuhn held that there are two kinds of discovery, which may be referred to *what-that* discovery and *that-what* discovery. The latter are Kuhn's focus in SSR but considering both kinds of discovery allow for a fuller understanding of Kuhn's view. Interestingly, Kuhn implied that one needs a correct conception of what one discovers, even though he failed to say how correct that conception needs to be. I propose a solution to this problem.

1 Introduction

In the philosophy of science, discovery is usually associated with the discovery of *ideas*: e.g., how did Newton arrive at his theory of gravitation, and Darwin at his theory of evolution? (Nickles 1980, Schickore 2022) A surprisingly neglected topic in the philosophy of science concerns the discovery of scientific *objects* or *natural kinds*, such as the discovery of the electron, nuclear fission, the DNA structure, black holes, tectonic plates, etc. Kuhn was one of the first philosophers to identify the discovery of scientific objects worthy of discussion.

This chapter will present and critically assess Kuhn's account of scientific discovery in chapter VI of *The Structure of Scientific Revolutions* (SSR).[2] In section 2 I introduce a basic distinction between two kinds of discovery that Kuhn introduced in an article in *Science*, which appeared in the same year as SSR (Kuhn 1962), and which formed the basis for chapter IV of SSR. In section 3 I characterize the kind of discovery that Kuhn called "more troublesome" (Kuhn 1962, 761), and which he focused in in SSR, in more detail. In section 4 I discuss a central issue in Kuhn's account

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[1] To appear in slightly shorter form in German as "Neuheiten in der Wissenschaft: Entdeckungen und Erfindungen" in *Thomas S. Kuhn: Die Struktur wissenschaftlicher Revolutionen*, Markus Seidel (ed.), in the series *Klassiker Auslegen*, De Gryter. See also my earlier "Scientific Discovery: *what-that's* and *that-what's*" (Schindler 2015), where most of the ideas of the current paper were first developed.

[2] Although Kuhn focuses on the discovery of objects in SSR, elsewhere he also writes about the discovery of ideas (Kuhn 1958; 1959).

of discovery, namely the *problem of correctness*. I discuss a solution proposed by Hudson and suggest my own amendment of Kuhn's account. Section 5 concludes this chapter.

2 Two basic kinds of discovery

Part of the reason why scientific discovery may have largely been neglected, Kuhn highlights himself: scientific discovery is nothing like the simple (and naively construed) act of seeing something for the first time (SSR, 55). Discovery, for Kuhn, requires more than observing a new phenomenon, or *that* something is the case; it also requires an understanding of *what* has been observed. That is why Kuhn also speaks of the distinction between discovery and invention being "exceedingly artificial" (SSR, 52).

The *that* and the *what* of a discovery have two possible sequences: *that-what* or *what-that*, forming two distinct classes of discovery. Kuhn himself did not give names to these two kinds of discovery, but I will refer to them as *that-what* and *what-that* discoveries in what follows. Unfortunately, the distinction Kuhn made in this paper did not survive the transition to SSR, but a full understanding of Kuhn's view of discovery requires engagement with this distinction.

It should be noted first of all, though, that in his *Science* paper Kuhn does not mention any of the central concepts of SSR: there is no mention of normal science, paradigms, revolutions, or incommensurability. The first three notions are clearly implicit in the paper, but the distinction between two kinds of discovery that Kuhn draws can be made sense of even without these notions (Schindler 2015). But for Kuhn, *that-what* discoveries are often (but not always) associated with paradigm change, whereas *what-that* discoveries are products of normal science. One may therefore refer to *what-that* discoveries as normal science discoveries and *that-what* discoveries as revolutionary discoveries. In what follows I will stick to the more general terminology, because it better highlights the two aspects that Kuhn held to be essential for discovery.

What-that discoveries are discoveries for which a scientific community has been conceptually prepared *before* the new object is observed: a paradigm theory predicts the objects to be discovered, or the paradigm otherwise leads scientists to expect the discovery of the objects in question. *What-that* discoveries are therefore "occasion only for congratulations, not for surprise" (SSR, 52).[3] *That-what* discoveries, on the other hand, are discoveries that hit the scientific community entirely off-guard: there is nothing within the paradigm that would conceptually prepare the scientific community for the novelty they discover. Kuhn also speaks of "unanticipated novelty" (Kuhn 1962, 762, 1962/1996, 96). The scientific community accordingly must first find out *what* it is that was observed or detected, before a discovery can be announced.

Kuhn attaches quite different characteristics to the two kinds of discoveries. Kuhn speaks of *that-what* discoveries as "not isolated events, but extended episodes" (SSR, 52), because when

[3] Kuhn at times even speaks as though things discovered by normal science (and predicted by paradigm theory) are no new sort of things at all (SSR, 61), although this is of course an exaggeration.

scientists are conceptually unprepared for the thing they observe for the first time, they necessarily are faced with a period of epistemic uncertainty about what it is they discovered; it will take time for scientists to make sense of the new entity they observed. Also, as Kuhn put it, in these kinds of discoveries "there is no benchmarks to inform either the scientist or the historian when the job of discovery has been done" (Kuhn 1962, 761). In *what-that* discoveries, all this is quite different: since scientists are conceptually prepared for what they discover when they first observe the new object, discovering *that* and discovering *what* can "occur together and in an instant" (Kuhn 1962, 762; see also SSR, 55-56). Scientists know what they are looking for, and once they have found it, their job is done.

It is worth noting here that Kuhn makes these descriptions of the two kinds of discoveries relative to the discovery-*that,* because obviously, relative to the discovery *what*, *what-that* discoveries are extended over time as well. But Kuhn's description is not arbitrary: the discovery *that* seems to be more essential to discovery in that without it, there would not be any discovery. In *what-that* discoveries there would just be an unconfirmed prediction. In *that-what* discoveries, in contrast, there would at least be a discovery of an anomaly.

The characteristics Kuhn associates with the two kinds of discoveries have implications for what we can and cannot know about them. In particular, Kuhn rejects questions such as "where did the discovery happen" and "when did it happen?" for *that-what* discoveries (Kuhn 1962, 761), because he believes that these questions do not do justice to the temporal extendedness of these kind of discoveries. The nature of *that-what* discoveries therefore sets hard limits to the research of the historian: "Even when all conceivable data were at hand [for the historian]", Kuhn concludes, "those questions would not regularly possess answers" (ibid.). In fact, Kuhn goes as far as saying that it is "always impossible" to attribute a *that-what* discovery to an instant in time, and that it is not possible to attribute a *that-what* discovery to a particular individual "often as well" (ibid., 762 and SSR, 55). For similar reasons, Kuhn believes priority disputes cannot be resolved in *that-what* discoveries (SSR, 54). One may refer to the sum of these distinctive features of *that-what* discoveries as *indeterminacy of time and space*. In contrast, *what-that* discoveries do not exhibit this indeterminacy. As Kuhn notes: "only a paucity of data can prevent the historian from ascribing them to a particular time and place", and accordingly, "there have been few priority debates" (Kuhn 1962, 761).

There is one important facet of Kuhn's account of discovery that unfortunately Kuhn does not explicate, namely the requirement that the discovered thing must be *correctly* identified. It is thus not enough, according to Kuhn, to discover that there is a new object, and to *somehow* conceptualize that object; rather, the conception must be at least partially correct. Again, Kuhn nowhere explicitly states this requirement, but it becomes apparent in the discussion of one of his examples, namely the discovery of oxygen, which we will discuss in the next section.

In both SSR and his *Science* paper, Kuhn squarely focusses on *that-what* discoveries, to which we will turn in a moment. *What-that* discoveries, on the other hand, he discusses at no depth in either of these places. Only in passing does Kuhn mention the discovery of new chemical elements predicted by Mendeleev (SSR, 58), and the discoveries of the neutrino and radio waves (Kuhn 1962, 761). But further examples of *what-that* discoveries are not hard to come by. Consider for example the discovery of the Higgs boson. The Higgs boson was first predicted in 1964 by Peter Higgs (and several others) as part of the Higgs mechanism that provides the particles of the standard model with their masses. The Higgs particle became an integral part of the standard model, which very successfully brought order into the "particle zoo" and correctly predicted several subatomic particles (e.g., the top quark and the tau neutrino). The Higgs particle was not found until 2012-3, i.e., almost 50 years after its prediction, when the Large Hadron Collider at CERN generated enough energy to produce the Higgs. Despite the tremendous resources that had to be invested in its discovery, physicists very much expected the discovery to happen at some point. One cannot but recall Kuhn's memorable phrase that what-that discoveries are "occasion for congratulation, not for surprise" when reading the following, contemporaneous comment by Sean Carroll (then a physicist at Caltech, who was not involved in the discovery): "It's a bittersweet victory when your theory turns out to be right, because it means, on the one hand, you're right, that's nice, but on the other hand, you haven't learned anything new that's surprising" (cited in Schindler 2015).

Before moving on to a more detailed discussion of *that-what* discoveries, it is worth noting that Kuhn readily admits that the two kinds of discoveries identified by him do not exhaust all the kinds of discoveries that there are (Kuhn 1962, 761, n3). I am myself more optimistic about the particular example he mentions as falling in between the two stools, namely the discovery of the positron (Hanson 1963).[4] Regardless, it seems reasonable for Kuhn not to insist that *all* scientific discoveries fit his mold. It is enough that a substantial number of discoveries do.

3 *That-what* discoveries

In chapter VI of SSR, Kuhn discusses three examples of a *that-what* discovery: the discovery of oxygen, x-rays, and the Leyden jar. Each example is meant to highlight a slightly different aspect of that-what discoveries, namely (i) the indeterminacy of time and place, (ii) the instrumental dimension of paradigms, and (iii) extra-paradigm, theory-induced discoveries, respectively. Furthermore, Kuhn argues that *that-what* discoveries have three stages, which he illustrates with

[4] Anderson discovered the positron without much theoretical guidance in 1932, for which he later received the Nobel Prize. Blackett and Occhialini at the same time discovered the positron on the basis of the Dirac equation, which predicted antimatter, but apparently did not have the confidence to publish their results until a year after Anderson's paper. Anderson apparently was not aware of the relevance of the Dirac's equation to his work. See Hanson (1963). The discovery of the positron is thus special in that it was a *that-what* discovery in the hands of Anderson, but a *what-that* discovery in the hands of Blackett and Occhialini. But we do not have to invoke a third category of discoveries.

an experiment from psychology. In the final part of chapter IV Kuhn discusses an apparent paradox of his account related to the emergence of novelty and the nature of normal science. I what follows, I discuss all of these points in their order.

3.1 Oxygen and the indeterminacy of time and place of *that-what* discoveries

Kuhn starts this case by noting that there are three men who may lay claim on having discovered oxygen in the 1770s: Scheele, Priestley, and Lavoisier. Kuhn focuses on the latter two, because Scheele did not manage to publish his results on time (but see Hudson 2001). Priestley produced oxygen by heating the red oxide of mercury, or simply mercuric oxide, but first misidentified the gas as nitrous oxide in 1774. By 1775 he believed he had isolated "dephlogisticated" air, that is, as air with a lower than standard amount of phlogiston, the non-existent "principle" of combustion. After receiving a hint from Priestley, Lavoisier started working on similar experiments and first took the gas to be a purer form of common air, and only in 1777 concluded that he had identified a new, distinct form of gas.

Kuhn argues that there is no answer to the question "who first discovered oxygen?" and that the priority dispute between the three men, accordingly, is not resolvable. The reason has to do with his view that it does not suffice for somebody to observe *that* X occurs; one must also demonstrate understanding of *what* X is. It is quite obvious that Priestley did not understand what he had detected: dephlogisticated air and phlogiston do not exist. Kuhn even denies Priestley the claim of being the first to isolate oxygen, because his sample apparently was not pure: "if holding impure oxygen in one's hands is to discover it, that had been done by everyone who ever bottled atmospheric air" (SSR, 54). Suppose, though, that Priestley had managed to produce a pure sample, in his mind a fully "dephlogisticated" sample of air. Could we then not say that Priestley would have been the first to isolate oxygen, even though he did not know what he had isolated? Perhaps, but on Kuhn's account this would not have sufficed for a justified discovery claim either.

Kuhn also denies the discovery claim to Lavoisier: "if we refuse the palm to Priestley, we cannot award it to Lavoisier" (SSR, 54). Although Lavoisier both isolated oxygen and understood that it was a distinct species of gas, and even gave it a new name, Lavoisier's conception of oxygen was mistaken too. Lavoisier conceived of oxygen as a "principle of acidity" (oxygen literally means "acid forming") that reacted with caloric, the non-existent substance of heat, to produce oxygen gas. As Kuhn points out, the principle of acidity was not given up upon until after 1820 and caloric not until the 1860s. However, oxygen had become an accepted chemical substance long before that. Kuhn concludes, perhaps somewhat unsatisfactorily, that oxygen was discovered sometime between 1774 and 1777, "or shortly thereafter" (SSR, 55).

What emerges from Kuhn's discussion until this point is something rather interesting: not only does Kuhn hold that a discovery requires both a discovery *that* and a discovery *what*, but he also requires that the discovery *what* be of the *right kind.* It is not enough for the discoverer to have any old conception of what is being discovered: the conception must be a *correct* conception. This aspect of Kuhn's account of discovery is entirely unarticulated, but central nevertheless, because without it, there would be no problem awarding the discovery claim to Priestley. For a moment Kuhn considers the possibility (SSR, 55), but ends up insisting that both the *that* and the (correct!) *what* aspect are required for a discovery.

It is perhaps no wonder that Kuhn does not explicate his requirement of correctness, for it seems at odds with his view that paradigms are incommensurable. If paradigms are incommensurable, then we have no grounds for deeming the conceptions associated with a particular paradigm correct. And yet, without the correctness requirement, we cannot make much sense of Kuhn's arguments about the Priestley-Lavoisier episode.

Kuhn's arguments, however, also shows that a requirement on a successful discovery cannot be that the relevant conception be *entirely* correct, for then we would have to postpone the discovery until long after the entity in question (e.g., oxygen) had already been accepted by the scientific community as discovered. This raises a further question: *how* correct must a description be for a discovery claim to be justified? To this question, as Hudson (2001) first pointed out, Kuhn provides no answer. We shall return to this issue in section 4.

Lastly, let us note with Kuhn that the discovery of oxygen resulted in a paradigm change from the phlogiston theory to Lavoisier's oxygen theory. But Kuhn also emphasizes that it must not generally be the case that *that-what* discoveries result in paradigm change, at least if paradigm change is understood to involve a change in reigning paradigm *theory*. The next example is a case in point. It illustrates that paradigms and the expectations that come with it imbue a scientific community's *entire* practice.

3.2 X-rays and paradigmatic instruments

X-rays were discovered in 1895 by Roentgen pretty much by chance. Roentgen was experimenting with cathode rays and noticed by accident that barium platino-cyanide screen started glowing when the cathode ray tube discharged. Roentgen, unlike others, noticed this and then explored the properties of the new form of radiation. In contrast to the discovery of oxygen, the discovery of X-rays did not require an overthrow of paradigm *theory*; instead, it necessitated a change in the expectations associated with the instruments and experimental setups of the previous paradigm.

Kuhn argues that "paradigms subscribed to by Roentgen and his contemporaries could not have been used to predict X-rays … [nor did they] prohibit the existence of X-rays" (SSR, 58). It is interesting that Kuhn here speaks of paradigms in the plural, for, normally, there is supposed to be only one paradigm per scientific field. He mentions Maxwell's electromagnetic theory and the "particulate theory of cathode rays" (J.J. Thomson's, presumably), neither of which, he claims, was fully accepted at the time, which makes Kuhn's use of the term paradigm in this context doubly curious – paradigms are supposed to be accepted by the scientific community by definition.

At any rate, even though scientists were well aware of different forms of radiation at the time (visible, infrared, and ultraviolet), Kuhn argues that X-rays could not just be added to the list of known forms of radiation: they were not only surprising for scientists, but even "shocking" (SSR, 59). Shocking, because scientists had experimented with equipment that had unwittingly produced radiation that scientists had failed to control for. X-rays thus did not just constitute a new phenomenon that scientists now could explore and employ, but their discovery also required a redoing of previous work and "changed fields [of study] that had already existed" (SSR, 59).

3.3 The Leyden jar and extra-paradigm, theory-induced discovery

Kuhn's final example is the discovery of the Leyden jar. The Leyden jar as such – basically a capacitor, i.e., a device that can store electricity – differs from the other two aforementioned discoveries in that the Leyden jar is a man-made instrument rather than a part of nature. But Kuhn does not think the discovery of the Leyden jar is worth discussing for that reason. Instead, Kuhn suggests that the Leyden jar was discovered by means of "speculative and unarticulated theories", whereby the discovery often is "not quite the one anticipated" (SSR, 61). More specifically, of the many competing theories there was one which conceived electricity to be a fluid, resulting in attempts to "bottle" electricity into water-filled glass vials. Experimenting with this device, practitioners noted, for example, that the jar needed an inner and outer conducting coating, and ultimately, that electricity is "not really stored in the jar at all" (SSR, 62). Insights like these, Kuhn suggests, led to revisions of the fluid theory and ultimately to the "first full paradigm for electricity" in the hands of Franklin (SSR, 62).

The example of the Leyden jar illustrates Kuhn's broad view that science is thoroughly theory-laden: there are no neutral sense data and discovery is driven by theoretical concerns. One may wonder whether thence all discoveries are in a sense *what-that* discoveries. But I think there are good reasons not to dilute our categories in this way. What-that discoveries are discoveries where a conception of X is formed before the observation of X and the conception of X is at least partially correct. Although in the case of the Leyden jar there is *a* conception of X, the conception is not correct: electricity is not a fluid. The conception thus had a heuristic role in the discovery, but became no part of the discovery per se, namely that glass bottles equipped with electrical conductors may under certain circumstances store electricity.

3.4 The three stages of *that-what* discovery

That-what discoveries, according to Kuhn, proceed in three stages: (i) awareness of anomaly, (ii) exploration of the anomaly, both observationally and conceptually, and (iii) adjustment or overthrow of the paradigm, which is often accompanied by resistance (SSR, 52-53; 62). Kuhn illustrates these stages with a psychological experiment, which has become well known among philosophers of science. For clarity, I here describe it in slightly more detail than Kuhn does.

In the experiment by Bruner and Postman (1949) subjects were exposed either to normal playing cards (e.g., black spade, red heart) or to anomalous playing cards (e.g., red spade, black heart). Each card was presented three times, with the time of exposure of each card being initially 10ms and increasing successively in certain intervals after each trial until the subjects correctly identified the card or until 1s was reached. Subjects had to make two correct identifications of each card for the identification to count as correct. As their "most central finding", Bruner and Postman report that the threshold of correct identification was four times higher for anomalous than normal cards (114ms vs. 28ms). Even after the maximal exposure of 1s, still 10% of subjects failed to correctly identify the anomalous playing cards.

Bruner and Postman also report four different ways in which subjects reacted to the anomalous cards. Kuhn highlights two of these, namely *dominance*, where the anomalous cards are just categorized as normal cards and *disruption*, where subjects, after an increased level of exposure, become confused about what they are observing (SSR, 63-64). Dominance was the most frequent reaction (27 of 28 subjects), whereas disruption occurred in 16 of 28 subjects (Bruner and Postman 1949).

The analogy Kuhn draws between this experiment and science is that "novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation" (SSR, 64). Just like in the playing card experiment, Kuhn believes that in science the anomalous is often perceived as normal. But there can be disruption of our attempts to categorize the phenomena in the usual ways. When there is awareness that something has gone wrong, there is opportunity to explore and understand the effect, until "the initially anomalous has become the anticipated" (SSR, 64).

3.5 Novelty and normal science

Of course, science is disanalogous to the playing card experiment in that scientists do not have the constraint of minimal exposure to their visual stimuli that subjects had in the experiment. Instead, the reason that novelty only emerges with difficulty in normal science has to do with the nature of normal science: it does not seek the anomalous. Instead, normal science aims to increase the scope and precision of the paradigm that the scientific community has accepted, and accepted for good reason, one should not forget. As Kuhn mentions, "the first received paradigm is usually felt to account quite successfully for most of the observations and experiments" (SSR, 64). And this is not just a feeling, as Kuhn points out elsewhere in SSR, but the paradigm's early empirical and explanatory success is real; otherwise the consensus on a paradigm would not form in the first place (see Schindler ms).

But if normal science does not aim for novelty, how does science ever discover anything new? The apparent paradox is resolved by the idea that anomalies can only be discovered with a very strong sense of what is normal, i.e., with a very strong sense of what the world should be like. Normal science, with its high precision instruments, intricate conceptual apparatus, and rigid predictions, is extremely good at detecting even the smallest aberration from the expected. But even when anomalies occur, they need not always result in paradigm change. As Kuhn explains:

"By ensuring that the paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core" (SSR, 65). When exactly anomalies lead to paradigm change and discovery rather than being bracketed off cannot be answered a priori and of course also depends quite centrally on the nature of the anomaly (Hoyningen-Huene 1993, 225-6).

4 The *problem of correctness* and a solution

There is one central problem with Kuhn's account of discovery. As we have seen earlier, on Kuhn's account, a discovery of X requires a correct conceptualization of X. What Kuhn does not address at all, though, is *how correct* the conceptualization of X has got to be; obviously, the requirement cannot be that the conceptualization must be *entirely* correct, otherwise, as Kuhn points out himself, we would have to date the discovery of oxygen way beyond the actual acceptance of the existence of oxygen as a distinct gas. As Hudson remarks: Kuhn has "left us with the quandary concerning how well one must conceptualise the discovered object" (Hudson 2001, 78). Let us call this problem the *problem of correctness*.

4.1 Hudson's proposal

Hudson proposes a solution to the problem of correctness. According to him, a discovery of X requires both a "base description" of X and a successful "material demonstration" of X, whereby Hudson defines a base description as "a description of the object that suffices to identify it: something that satisfies this description is the object being considered" (Hudson 2001, 77). Moreover, a base description is "one that, in the normal course of affairs, is useful as an indicator of a particular object—a description such that, if satisfied, we would anticipate the presence of the object" (ibid, 87–8). Crucially, "one need only possess enough conceptual resources" to recognize the presence of the discovered object; one does not need to conceptualize the object (entirely) correctly (78). Hudson even goes as far as saying that accuracy of base descriptions is "not necessary" (88). In sum, base descriptions need not be correct and must merely be *heuristically useful* in identifying the object of discovery.

With regard to the discovery of oxygen, Hudson's view is that Priestley both had an apt base description and materially demonstrated it. Since oxygen was a new gas to the scientific community at the time and since oxygen exists, Priestley counts as the discoverer of oxygen, despite his false beliefs about the object he discovered being dephlogisticated air. More specifically, Hudson attributes to Priestley the base description that "a species of air, highly respirable and combustible, which is a constituent of common, atmospheric air" (Hudson 2001, 82). Priestley materially demonstrated this base description by showing that the gas was not carbon dioxide or "fixed air", as it was known then. This was easy, as carbon dioxide has opposite effects to oxygen: it extinguishes fire and kills animals. To show that oxygen was not just some "purer" form of common air, Priestley showed that the "nitrous air" test, which he discovered and used to measure the "goodness" of common air, yielded better results for this new gas. Lavoisier, on the other hand, is not the discoverer for Hudson, because his base description of "good common air" in 1775 was no base description: it "did not suffice to pick out oxygen" (Hudson 2001, 83). Lavoisier also failed to materially demonstrate his base description, because he did not perform the same thorough 'nitrous air' tests (ibid.). Hudson concludes, contrary to Kuhn, that "[*that-what*] discoveries have definite discoverers and discovery times" (Hudson 2001, 91).

It is worth reminding ourselves at this point that Kuhn was not interested in resolving priority disputes (SSR, 54), that is he was never interested in determining who should be rewarded the title of discoverer. Instead, Kuhn discusses the issue of priority as a proxy for narrowing down on an issue that is of importance to him, namely the properties of *that-what* discoveries, and in particular, the inherent extension in time of *that-what* discoveries. None of what Hudson says really goes to the heart of this, for one can agree with Hudson that Priestley had a base description that sufficed to identify oxygen and that he materially demonstrated it, and at the same time appreciate the point made by Kuhn that the gas discovered by Priestley was *not* dephlogisticated air. The discovery was therefore by no means complete when Priestley successfully demonstrated his base description.

4.2 Why heuristically useful descriptions are not enough

There is another issue with Hudson's account. Even though prima facie his account may seem reasonable when his homely examples and the discovery of oxygen is concerned, it fails quite spectacularly for other examples. For example, consider the discovery of electrons, which is usually attributed to J.J. Thomson in 1897 (but see Arabatzis 2006). Crucial to Thomson's discovery were cathode ray tubes that produce beams of electrons discharging from the cathode through its anode into the body of the glass tube. The base description "cathode rays cause a green glow on the wall of the glass tube", or "cathode rays can be deflected by a magnetic field", or ""cathode rays cast shadows", etc. all would have sufficed to identify electrons. One could have demonstrated this base description also very straightforwardly by pointing to the glass tube in which cathode rays discharged. And yet, it would be clearly absurd to say that the first person to ever produce cathode rays discovered electrons (presumably Plücker in 1858, or Hittorf in 1868, or perhaps even Faraday 1838). Not even Crookes would normally be considered the discoverer, who in 1879 was the first to notice that cathode rays would deflect in a magnetic field, was the first to assign a negative charge to cathode rays (or rather to the molecules he thought constituted them), and who defended the particulate nature of electrons (as opposed to a radiation akin to light, a view which others held). Hence a discovery requires more than just having a concept that allows one to pick out the scientific object of interest.

4.3 Another proposal

With Kuhn, I think, there ought to be a more robust sense in which scientists must understand the thing they discover; it does not suffice that they merely possess *some* description that helps them (heuristically) to identify the object they discover. On the other hand, Hudson is right that Kuhn's account is underspecified regarding the correctness of the conceptualization of X. I therefore want to make a proposal that I take to be both in line with Kuhn's account requiring a discovery *that* and a discovery *what* and as offering a solution to the problem of correctness. My proposal is this that a to discover X, one must not only discover *that* X exists by observing X or its direct effects, but also that one correctly conceptualizes *what* one discovers by correctly identifying *at least some of X's essential properties,* i.e., properties that are individually necessary and jointly sufficient for the individuation of X. What properties suffice for the individuation of X at a particular moment in time depends on the state of knowledge at that time (Schindler 2015).

Consider again the discovery of the electron. There are many complications to the story, but Thomson is usually considered to be the discoverer of the electron (Falconer 1987, Arabatzis 2006). Thomson managed to deflect cathode rays in an electrical field in 1896 (after Hertz and others had earlier failed to do so), effectively demonstrating the negative charge of electrons. He measured the electron's charge-to-mass ratio as a thousand times lower than that of hydrogen ions, which is at the correct order of magnitude. He also established that the charge-to-mass ratio was independent of the gas used in the tube, which showed that the charge was not a property of the molecules, as had been previously thought. Thomson finally also correctly conceptualized the electron as a subatomic, material particle. These are all essential properties of the electron. With some justification, one could thus say that Thomson discovered in cathode ray experiments both *that* the electron exists and correctly described at least part of *what* it is, i.e., he correctly identified some of its essential properties (but see Achinstein 2001, Arabatzis 2006).[5] Moreover, Thomson identified essential properties of electrons that *at the time* sufficed to individuate electrons as a new species of subatomic particles: there were no other known negatively charged subatomic particles (this changed later with the discovery of the muon in 1936).

Clearly, not all of Thomson's beliefs about electrons were correct. For example, he convinced himself that electrons have particulate and *not* wave-like properties (Achinstein 2001, 274). We now know better. Of course, Thomson had no inkling of (other) quantum properties of electrons, such as electron spin. Thomson thus clearly did not discover all of the electron's essential properties. For all that we know, not even we may know all of the electron's essential properties. It would therefore be absurd to say that the electron was not discovered until all of its known

[5] Achinstein (2001) argues that Thomson "knew of the existence of things that happen to be electrons" (p. 279), but that he probably was not the first to do so. Achinstein is reluctant to embrace the stronger claim that Thomson knew what he had discovered, particularly with regard to the subatomic nature of the electron. Arabatzis (2006) argues that Thomson's work constitutes only *one* contribution to the discovery of the electron and that particularly Zeeman, but also Lorentz and Larmor made other important contributions. There is probably a good case for Zeeman being the first to discover *that* electrons exist in his spectral line splitting experiments of 1896, even though he initially reported that electrons (or "ions", as he knew them) are *positively* charged (Arabatzis 2006, 84, fn 41). Although Lorentz and Larmor's theories played an important heuristic role in analyzing Zeeman's experiments, they were significantly modified in the process, and they certainly did not predict the electron, which they would have had to for discovery to count as a *what-that* discovery.

quantum properties were discovered in the early 20th century, or even that we *still* may not have discovered the electron. It seems much more reasonable, instead, to say that the electron was discovered by Thomson around 1897 (the officially accepted discovery date) because Thomson discovered some essential properties of electrons.

It goes without saying that such a realist view of discovery does not sit all too well with Kuhn's overall view; Kuhn was no realist.[6] Yet if we do not require that the descriptions of the scientific objects in discoveries are correct (e.g., "electrons have negative charge"), then it makes little sense to talk about discovery in the first place (but see Arabatzis 2006, 23f.).

5 Conclusion

I think Kuhn did us a service in emphasizing a scientific discovery requires both an observation of a new object and an understanding of what that object is. Clearly, without the former, there could not be a discovery, but without the right conceptualization of what is being discovered, there would just be an anomaly for normal science. Kuhn is also right, I think, in pointing out that discoveries often cannot be attributed to a single scientist at a specific point in time.

There are three areas where Kuhn's account is wanting. First, Kuhn requires (albeit implicitly) that the discovered object be conceptualized correctly. However, he fails to say how correctly that ought to be the case, as first pointed out by Hudson. I offered a solution to this problem. Second, I think Kuhn was also too optimistic about *what-that* discoveries rarely giving rise to priority disputes, as can be seen in the recent discovery of the Higgs boson. Here were several parties laying claim to having discovered what the Higgs boson is in the mid-1960s, only some of whom ended up receiving the Nobel Prize (Merali 2010). Since it is not clear what, if anything, should be untypical about this case, priority disputes may then not be something that helps us tell apart the two kinds of discovery. However, Kuhn considered priority debates only a proxy, so this observation does little to affect the core of Kuhn's account. Third, despite Kuhn's claim that it is impossible to attribute a *that-what* discovery to any particular individual, there seems little difficulty in saying that e.g., Priestley discovered *that* oxygen exists, and that Lavoisier discovered *what* oxygen is – namely a new form of gas with its own mass that reacts with other substances in combustion. At the same time one can still agree with Kuhn that neither part of the discovery alone is sufficient for the discovery of oxygen.

Regardless of these issues, Kuhn's account of scientific discovery remains the most important starting point for anybody desiring to think and write about a topic that deserves much more attention from philosophers, namely the discovery of scientific discovery of objects.

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[6] See Psillos (1999) for a standard work on scientific realism.

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