Revisiting Discovery and Justification

Historical and philosophical perspectives on the context distinction

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ON THE INEXTRICABILITY OF THE CONTEXT OF DISCOVERY AND THE CONTEXT OF JUSTIFICATION

1. INTRODUCTION[1]

Before the historicist turn in philosophy of science, it was generally regarded that scientific activity takes place within two distinct contexts, the context of discovery and the context of justification. The former consists in the processes of generation of scientific hypotheses and theories; the latter in their testing and validation. According to Reichenbach, who codified the distinction, the context of discovery was the province of historians, psychologists, and sociologists and was not susceptible to logical analysis: "The act of discovery escapes logical analysis; there are no logical rules in terms of which a "discovery machine" could be constructed that would take over the creative function of the genius" (Reichenbach 1951, p. 231). On the other hand, the context of justification was an area which could be rigorously explored and formalized and thus fell within the province of logic and philosophy.[2] Popper introduced a very similar distinction in *The Logic of Scientific Discovery* (Popper 1968, p. 31). His notion of discovery, however, was different from Reichenbach's (see note 12).

This distinction[3] historically derived from several premises. First, it was based on a conception of philosophy of science as a normative enterprise, i.e., an enterprise whose aim was to lay out rules that should govern any activity that deserves to be called science. Second, it was grounded on a conflation of scientific discovery with the generation of novel ideas. Thus, the study of discovery had to be the study of scientific creativity. Third, it rested on the widespread view that there are no rules whose application can enhance one's creativity. The latter two assumptions precluded the possibility of a normative theory of discovery and, along with the first one, rendered impossible the philosophical exploration of discovery. Finally, the distinction required justification to be a rule-governed process so as to be the subject of a normative project. The ultimate aim of such a project would be to find logical rules in terms of which a "justification machine" could be constructed that would take over the justificatory practices of the scientists.[4]

All of these assumptions have for some time now been under attack and, consequently, the distinction has been undermined. To begin with, there has been a gradual shift towards a more "naturalistic" conception of philosophy of science, namely a conception that stresses the descriptive and hermeneutic aspects of the philosophical study of science as opposed to its normative ones (Kitcher 1992; Hoyningen-Huene, this volume). Furthermore, the conflation of discovery with generation has been

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exposed and criticized. This point is crucial for my purposes and will be extensively discussed later. Even if this conflation and the concomitant identification of the study of discovery with the study of creativity were valid, one could still deny that creativity is an unanalyzable, totally mysterious phenomenon that precludes the possibility of a normative theory of discovery. Indeed, there has been overwhelming evidence that hypothesis generation and theory construction are reasoned processes whose explication can (and should) be carried out by philosophers of science.[5] Some have even argued that it is possible to devise a normative theory of scientific discovery that would specify heuristic procedures which would improve the efficiency of scientific inquiry and, thus, facilitate the discovery process.[6] Finally, the notion of justification as a rule-governed process has been challenged. Justification itself requires many discovery tasks. For example, to justify a hypothesis one needs to "discover" an appropriate test as well as the appropriate auxiliary statements to render the hypothesis testable (Nickles 1980a, p. 13; Nickles 1985, p. 193; Nickles 1990, p. 162; cf. also Putnam 1991).

The distinction has also been undermined on different grounds. It has been argued that the kind of reasoning that is involved in generating a hypothesis is not fundamentally different from the kind of reasoning employed in justification (Achinstein 1980). Moreover, hypothesis generation and theory construction are extended problem-solving processes with many stages, each of which involves (partial) justification. At each particular stage one's aim is to satisfy some of the constraints posed by the problem. The satisfaction of those constraints amounts to partial justification of the evolving solution (Langley et al. 1987; Nickles 1980a). Furthermore, justification in science often takes the form of heuristic appraisal, i.e., of evaluating the future problem-solving potential of a theory (Nickles 1985, p. 194; Nickles 1987, pp. 47–48; Nickles 1989a; Nickles, this volume). For someone who views discovery as an instance of problem solving, this form of theory appraisal amounts to judging the capacity of a theory to generate discoveries and, therefore, it is closely linked with discovery itself. Finally, Nickles has stressed the importance of "generative justification" or "discoverability", namely a form of appraisal that justifies a claim by deriving it from already established knowledge. Justification in this case amounts to specifying a rationally reconstructed (not necessarily the actual) discovery path (Nickles 1984; Nickles 1985, pp. 194–195; Nickles 1988, p. 394; Nickles, this volume). Thus, it is reasonably established that justification and discovery are much more closely related than formerly thought.

The distinction has also been attacked from a historical perspective. Thomas Kuhn, for instance, has argued that

> Considerations relevant to the context of discovery are ... relevant to justification as well; scientists who share the concerns and sensibilities of the individual who discovers a new theory are ipso facto likely to appear disproportionately frequently among that theory's first supporters.[7]

However, many of the critics of the distinction continue to share with its proponents the same conception of scientific discovery. The context of scientific discovery, on this view, consists in the processes which lead to the formulation of new hypotheses or theories. In other words, both sides of the debate equate discovery either with the "generation"[8] of hypotheses or with the construction of scientific theories.[9] With some notable exceptions, justification is still not seen to be part of the discovery process.[10]

In this paper, I argue that this view of scientific discovery is misleading and without it the debate on the validity of the distinction between the two contexts could not even start. The focus of my discussion will be the discovery of unobservable entities and the discovery of new phenomena, using as examples the discovery of the electron and the discovery of the Zeeman effect.

2. THE INEXTRICABILITY OF THE TWO CONTEXTS

The term "discovery" is used to designate many different kinds of processes: the discovery of phenomena through controlled experiment (e.g., the discovery of the Zeeman effect—the magnetic splitting of spectral lines); the discovery of entities which are accessible to immediate inspection (e.g., the discovery of a previously unknown species); the discovery of objects which are not accessible to unaided observation (e.g., the discovery of the planet Neptune); the discovery of entities which are unobservable in principle (e.g., the discovery of the electron); the discovery of new properties of well established entities (e.g., the discovery of electron spin); the discovery of new principles/laws (e.g., the discovery of energy conservation); and the discovery of new theories (e.g., the discovery of the special theory of relativity).[11]

In all of these cases the two contexts are inextricably linked. Consider, for example, the discovery of unobservable entities. An individual or a group can acquire the status of "the discoverer" only after they have convinced the rest of the scientific community of the existence of the entity in question. A mere hypothesis to the effect that a new entity exists would not qualify as a discovery of that entity. The justification of that hypothesis would be a constitutive characteristic of that discovery.[12] The context of discovery is "laden" with the context of justification because "discovery" is a term which refers to an epistemic achievement: if one succeeds in discovering something then, no doubt, this something exists.[13] That this is the case is witnessed by the fact that in the historical literature the historiographical issue of scientific discovery has been discussed only in relation to entities that remain part of the accepted scientific ontology (e.g., oxygen). No historian or philosopher, to the best of my knowledge, has ever used the term "discovery" to characterize the proposal and acceptance of an entity (e.g., phlogiston) that we now believe was a fictitious one.[14] The example of phlogiston is instructive. Contemporary historians and philosophers do not think that phlogiston was discovered, despite the fact that some 18th century chemists referred to phlogiston as one of the most significant discoveries in the history of chemistry. In Joseph Priestley's words, phlogiston "was at one time thought to have been the greatest discovery that had ever been made in the science."[15] This suggests that there is a retrospective dimension to discovery. Only beliefs that have remained immune to revision can be designated with that term. "Discovery" is an evaluative category, which has *realist presuppositions*.[16]

Despite the critical remarks that have been raised against the distinction between discovery and justification, one can still distinguish between the original historical mode of hypothesis generation and the "final" form of justification. These two aspects of the discovery process need not coincide. The actual path that led to the hypothesis (theory) in question might be "edited" out of the presentation of the hypothesis before the community.[17] Furthermore, justification itself is a constantly evolving process: it is rarely the case that the justification of a hypothesis retains its original form. As science develops the justification of scientific beliefs undergoes continuous reconstruction.[18] Thus, the distinction becomes a temporal, as opposed to a logical, one between two aspects of the discovery process.[19]

This brings me to the application of the term "discovery". "Discovery" should not be confused with either "generation" or "construction". Even though the terms "generation" and "construction" (or "extended generation") do not preclude that the outcome of the corresponding processes is a true statement about nature, they do not imply it either. Furthermore, they carry the connotations of "creation"; with construction something comes into being as a result of human action. "Discovery", on the other hand, implies truth. Moreover, it carries the connotations of "revelation"; some truth about nature is disclosed to a passive intellect (Stachel 1994, pp. 142, 146; Caneva 2001, p. 19). It should be noted that by using the term "construction" I do not thereby commit myself to the view that scientific facts are socially constructed (see below, p. 226). Much of the work carried out under this approach is strongly relativist and anti-realist. While this is not the place to take up the challenge posed by contemporary sociology of scientific practice, I should point out that viewing science as a constructive activity does not necessarily carry relativist or anti-realist implications. The neutrality of such a view vis-à-vis the issue of relativism is shown by the fact that one might be able to specify canons of sound construction that would transcend the local practices of particular scientific groups. Moreover, it is conceivable that one could come along and show that sound constructions result in genuine facts about nature (realism). This possibility shows that constructivism, properly understood, is neutral with respect to the realism debate.[20]

In view of the distinction between discovery and construction, I would like to revise and extend the tentative classification of scientific discoveries that I offered above. Some of those discoveries (D) will be re-classified as constructions (C) or inventions (I). The utility of this revision will become apparent later. In the domain of application of the term "discovery" I will include individual observable entities (e.g., Neptune), observable natural kinds (e.g., tigers), and phenomena (e.g., the Zeeman effect). The term "construction" will apply to problems, problem-solutions, theories, theoretical entities (e.g., the representation of the electron), principles (energy conservation), and representations of unobservable properties (electron spin). Finally, I will use the term "invention" to characterize the proposal of novel theoretical and experimental techniques, and the creation of new instruments. All these different kinds of discovery and construction are interrelated. For instance, the construction of a problem (e.g., the incompatibility of two established scientific theories) might result in the construction of a new theory that will resolve the problem in question.

It is worth exploring the similarities and differences between these kinds of discoveries and constructions. The question would be to determine whether the generative and justificatory procedures are similar in all cases and whether different kinds of discoveries are valued differently by the scientific community, i.e., whether they are assigned a different social status. It seems to me that the items in the above classification differ from each other in important respects. For instance, discovery cannot be identified with problem solving. The proposal of, say, phlogiston, solved several of eighteenth century chemical problems, but it does not count as a discovery. In what follows I will focus on the discovery of phenomena and unobservable entities, simply because these are the processes that have been significant for my own historical research.

Starting with phenomena, their discovery involves the following circumstances: the observation of a novel situation and the construction of an argument to the effect that the observations obtained are not artifacts of the apparatus employed and that all perturbing factors ("noise") have been eliminated. Furthermore, the validity of the argument in question must not be affected by subsequent theoretical and experimental developments. I will discuss further this issue later in connection with the discovery of the Zeeman effect.

Proceeding to unobservable entities, their "discovery" can be seen as the first stage of the construction of their representation. During that stage scientists construct a representation of a novel entity, attempting to resolve particular (empirical or conceptual) problems. If the emerging representation provides an adequate solution to those problems, then this is taken as an indication that the corresponding entity exists. Thus, the "discovery" of an unobservable entity and the early phase of the construction of its representation are two aspects of a single process and cannot be sharply distinguished.[21] The "discovery" ends when the "discoverers" persuade the rest of the community that the entity in question is real. Only in this qualified sense can one claim that an unobservable entity was discovered. Again, I defer a detailed discussion of this case for later.

Further, the widespread view that a discovery is an isolated event that can be credited to a single individual is misconceived, at least vis-à-vis those cases that mostly concern me here. Both the discovery of phenomena and the discovery of unobservable entities involve many complex tasks and, thus, cannot take place at a single moment. Furthermore, the discovery of unobservable entities is rarely the accomplishment of a single individual. These are Kuhnian insights and they are reinforced by the realization that the context of discovery comprises both the context of generation and the context of justification (Kuhn 1970, pp. 52–65; Kuhn 1977, pp. 165–177). I think, however, that Kuhn's claim that discoveries of phenomena, which could not be predicted from accepted theory, cannot be attributed to particular individuals is not, in general, true. The criteria that enable us to claim that such a discovery has been accomplished are the criteria that are involved in judging the reliability of the experiment that exhibits the new phenomenon. Regardless of whether the phenomenon can be given a theoretical explanation, the experimental result, along with the demonstration of its validity (usually based on experimental background knowledge) constitute the occurrence of a discovery. Both of these achievements might be the product of a single scientist.

One might not want to use the term "discovery" to characterize the products of scientific activity, but undoubtedly discovery occupies a central place in the scientists' own image of their enterprise. Discoveries are seen as the units of scientific progress and are accordingly valued. Usually they are constructed in the light of knowledge that was not available to the historical actors at the time when the presumed discovery took place, and are intimately tied to the reward structure of science. They tend to be *post-hoc* reconstructions of specific episodes, whose aim is to propagate and reward certain practices and beliefs that are deemed significant for contemporary scientific activity (Schaffer 1986). The "discovery" of the electron provides a good example of what I have in mind. It is a discovery that supposedly took place in 1897 and was the exclusive achievement of J. J. Thomson. As I will argue below, neither of these claims can stand the test of historical scrutiny. This poses an interesting historiographical problem vis-à-vis the aims and function of the retrospective construction of that discovery. This problem, in turn, suggests that the construction and continuous reconstruction of scientific discoveries can be fruitfully studied from a sociological perspective (Brannigan 1981; Schaffer 1986; Caneva 2001). The study of discovery transcends both the psychological exploration of scientific creativity and the philosophical analysis of scientific justification, since in many cases discoveries serve specific purposes within the scientific community. The psychological, philosophical, and sociological approaches to the study of discovery are complementary and should not be undertaken at the expense of each other (Nersessian 1993; Stachel 1994, p. 142).

In what follows I will provide concrete illustrations of these historiographical and philosophical issues, by examining the discovery of the Zeeman effect and the discovery of the electron.[22]

3. ON THE DISCOVERY OF PHENOMENA: THE CASE OF THE ZEEMAN EFFECT

It was known since the middle of the nineteenth century that there was a close connection between magnetism and light. In the early 1890s a Dutch physicist, Pieter Zeeman (1856–1943), attempted to detect the influence of a magnetic field on the spectrum of a sodium flame. After several unsuccessful attempts he managed to demonstrate the effect in question (Zeeman 1896). He placed the flame of a Bunsen burner between the poles of an electromagnet and held a piece of asbestos impregnated with common salt in the flame. After turning on the electromagnet, the two D-lines of the sodium spectrum, which had been previously narrow and sharply defined, were clearly widened. In shutting off the current the lines returned to their former condition.

Zeeman was not convinced that the observed widening was due to the action of the magnetic field directly upon the emitted light. The effect could be caused by an increase of the radiating substance's density and temperature. Since the magnet caused an alteration of the flame's shape, a subsequent change of the flame's

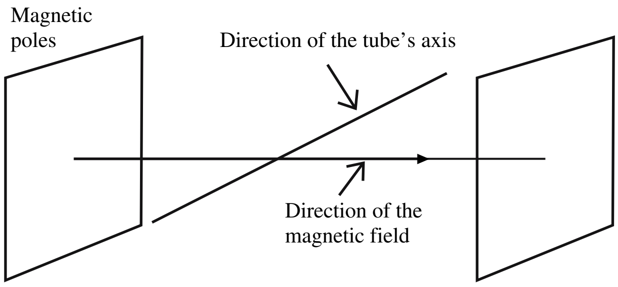

Figure 1.

temperature and density was also possible. Therefore, Zeeman tried another more complicated experiment. He put a porcelain tube horizontally between the poles of the electromagnet, with the tube's axis perpendicular to the direction of the magnetic field (see figure 1). A piece of sodium was introduced into the tube and simultaneously the tube's temperature was raised by the Bunsen burner. At the same time the light of an electric lamp was guided by a metallic mirror to traverse the entire tube.

In the next stage of the experiment the sodium, under the action of the Bunsen flame, began to gasify. The absorption spectrum was obtained by means of a Rowland grating. Finally the two sharp D-lines of sodium were observed. By activating the electromagnet the lines became broader and darker. When it was turned off the lines recovered their initial form.

Zeeman's experimental scruples were, nonetheless, not satisfied. Remember that the experiment's purpose was to demonstrate the direct effect of magnetism on light. Zeeman was still skeptical about whether this aim had been accomplished. The different temperature in the upper and lower parts of the tube resulted in a heterogeneity of the vapor's density. The vapor was denser at the top of the tube and, since their width at a certain height depended on the number of incandescent particles at that height, the spectral lines were therefore thicker at the top. It was conceivable that the presence of a magnetic field could give rise to differences of pressure in the tube of the same order of magnitude and in the opposite direction to those produced by the differences of temperature. If this were the case, the action of magnetism would move the denser layers of vapor toward the bottom of the tube and would alter in this way the width of spectral lines, without interacting directly with the light that generated the spectrum.

To eliminate this possibility, Zeeman performed an even more refined experiment. He used a smaller tube and heated it with a blowpipe in order to eliminate disturbing temperature differences. Moreover, he rotated the tube around its axis and thus achieved equal densities of sodium vapor at all heights. The D-lines were now uniformly wide along their whole length. The subsequent activation of the electromagnet resulted in their uniform broadening. Zeeman was by then nearly convinced that the outcome of his experiments was due to the influence of magnetism directly upon the light emitted or absorbed by sodium.

The originality and ingenuity of Zeeman's experiments consisted in the elaborate and sophisticated methods that he used in order to eliminate background "noise" and thus establish the direct relationship between the observed effect and the action of magnetism. The change of the width of the spectral lines after the activation of the electromagnet was not by itself an indisputable demonstration of a direct interaction between magnetism and light. As we have seen, certain intermediate links, interposed between the generation of a magnetic field and its effect on the spectrum of a substance, could have explained the experiment's result and thus prevented Zeeman from postulating a direct causal connection between magnetism and light. Zeeman's significant achievement was in the elimination of all these potentially existing links.[23]

Zeeman's experimental work indicates that the role of background knowledge in experimental practice, along with the all-pervasive "noise" render experimentation a very complex process. Part of the experimenter's task is to employ background knowledge in order to eliminate the all-pervasive "noise", a task that requires a very subtle form of "experimental" reasoning. The display of this reasoning in the narration of experimental discoveries amounts to the construction of an argument for the validity and significance of the reported experimental results. In Zeeman's case, his strategy in eliminating potentially distorting features of his experimental situation depended on already established experimental knowledge. The reasoning behind this strategy was displayed in the initial report of his discovery to persuade his audience that his experimental results revealed the direct influence of magnetism on light.

I have already suggested that discoveries of new phenomena involve the observation of a novel situation and the construction of an argument to the effect that the observations obtained are not artifacts of the apparatus employed and that all perturbing factors ("noise") have been eliminated. A further requirement was that the validity of the argument in question must not be affected by subsequent theoretical and experimental developments. All of those criteria are met in the episode that I have sketched. Zeeman's novel observations were supported by considerable argumentation that aimed at establishing their validity. Furthermore, his "experimental" reasoning was not based on the high-level electromagnetic theory (Lorentz's theory of "ions") that was employed to explain the results he had obtained. Thus, the subsequent abandonment of that theory was not detrimental for the validity of his arguments. It was the robustness of those arguments that rendered the Zeeman effect a stable part of the experimental setting for several subsequent developments in the theory of atomic structure.

I hope it has become evident that, with respect to the discovery of phenomena, justification is an essential part of the discovery process. In discoveries of this kind, the context of discovery and the context of justification are inextricably linked. In the rest of this paper, I will try to show that the same is true of the discovery of unobservable entities. I will illustrate my arguments by reference to the discovery of the electron.

4. ON THE DISCOVERY OF UNOBSERVABLE ENTITIES: THE CASE OF THE ELECTRON[24]

In order to identify an event or a process as the discovery of an unobservable entity, one needs a criterion (or a set of criteria) that would enable one to say that such a discovery has taken place. Several possible stances to the problem of what constitutes a discovery of this kind can be adopted. Two of those possibilities, that I will examine here, depend on one's position on the debate on scientific realism, a salient aspect of which concerns the grounds that we have for believing in the reality of the unobservable entities postulated by science (electrons, protons, fields, etc.). First, one might favor an anti-realist stance, i.e., maintain that one has to be at least agnostic with respect to the existence of unobservable entities. From such a point of view discoveries of unobservables never take place. To quote from an eminent contemporary representative of this approach, "scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable" (van Fraassen 1980, p. 5). On this stance, "discovery" has nothing to do with truth. Rather, it is a process of constructing empirically adequate models. The unobservable entity is a convenient fiction. To put it in terms of the discovery / justification distinction, existence claims concerning the unobservable can never be sufficiently justified.

On the second (realist) stance, one might propose certain epistemological criteria whose satisfaction would provide adequate grounds for believing in the existence of a particular entity. From this point of view a discovery takes place when an individual or a group has managed to meet the required criteria. As an example consider Ian Hacking's proposal that a belief in the reality of an, in principle, unobservable entity is justified to the extent that the entity in question can be manipulated (Hacking 1983, pp. 262–266). It follows then that an unobservable entity has been discovered only if a scientist has found a way to manipulate this entity. Justification is considered an essential aspect of the discovery process and is identified with manipulability.

It is evident that the adequacy of the proposed way for deciding when something qualifies as a genuine discovery depends on the adequacy of the epistemological criteria for what constitutes unobservable reality. Any difficulties that might plague the latter would cast doubt on the adequacy of the former. Although this approach to the issue of scientific discovery can be, in principle, realized, no adequate proposal of the kind outlined has been made so far. That is, no epistemological criteria have been formulated whose satisfaction would amount to an existence-proof of an unobservable entity. In particular, Hacking's proposal that manipulability provides such a proof leaves much to be desired. Let us examine the merits and limitations of Hacking's view vis-à-vis the discovery of the electron.

The historian Isobel Falconer has employed Hacking's criterion of what constitutes unobservable reality in an attempt to justify the attribution of the electron's discovery to J.J. Thomson (Falconer 1987). She challenged traditional interpretations of Thomson's discovery that portrayed

> this discovery . . . [as] the outcome of a concern with the nature of cathode rays which had occupied Thomson since 1881 and had shaped the course of his experiments during the period 1881–1897 (*Ibid*., p. 241).

Instead she argued that "[a]n examination of his work shows that he paid scant attention to cathode rays until late 1896" (*Ibid*.). Furthermore,

> [t]he cathode ray experiments in 1897 were not the origin of the corpuscle [which has been re-named electron] hypothesis; instead they acted as a focus around which Thomson synthesized ideas he had previously developed (*Ibid*., p. 255).

However, she did not deny a central presupposition of the traditional view, namely that the discovery of the electron was a temporally non-extended event which can be credited to a single individual. Even though the "corpuscle *hypothesis*" did not originate with Thomson's experiments with cathode rays, the *discovery* of corpuscles (i.e., the experimental demonstration of their existence) was the outcome of these experiments.

> Arriving at the theoretical concept of the electron was not much of a problem in 1897. Numerous such ideas were "in the air". What Thomson achieved was to demonstrate their validity experimentally. Regardless of his own commitments and intentions, it was Thomson who began to make the electron "real" in Hacking's sense of the word . . . . He pinpointed an experimental phenomenon in which electrons could be identified and methods by which they could be isolated, measured and manipulated. This was immensely significant for the development of the electron theory which hitherto has been an abstract mathematical hypothesis but now became an empirical reality (*Ibid*., p. 276).

In terms of the methodological issues discussed above, Falconer attempts to reduce the discovery process to the precise moment when experimental verification took place, thus equating discovery with the ability to isolate, measure, and manipulate. From my perspective this amounts to equating discovery with justification.

If, however, as I have argued, the context of discovery comprises both the context of generation and the context of justification, then the physicists who had formulated all those "ideas in the air" should also be considered as having taken part in the discovery of the electron. Furthermore, Thomson was not the only one who could manipulate electrons. All those who experimented with cathode rays were able to manipulate them in various ways. For example, they could deflect them by means of magnetic fields. That is, from our perspective, given that they manipulated cathode rays and that cathode rays are streams of electrons, it follows that they manipulated electrons. And this brings me back to Hacking's criterion of what constitutes unobservable reality.

To see the limitations of this criterion consider Thomson's experiments with cathode rays. Since one could describe these experiments in terms of cathode rays as opposed to electrons, the act of manipulation could be described without even mentioning the entities that, according to present-day physics, were manipulated. Moreover, an anti-realist could give an even less theory-laden description, by avoiding the term "cathode rays" and using instead the phenomenological expression "spot on a phosphorescent screen." The only thing that we know, the anti-realist would argue, is that by activating an electromagnet Thomson could move a spot on a phosphorescent screen. Since an act of manipulation can be described without mentioning the unobservable entity that is (supposedly) manipulated, this act does not, by itself, imply the existence of the entity in question. Thus, given that experiments can be (re)described in phenomenological terms,[25] manipulability cannot be employed, to the satisfaction of an anti-realist, for existential inferences. Whereas for Hacking manipulability justifies existence claims, for the anti-realist it is the other way around: It is the belief in the existence of, e.g., electrons, prior to the act of manipulation, that allows us to interpret that act as a manipulation of electrons (as opposed to something else).[26]

Since Hacking's criterion does not provide adequate grounds for a realist position towards unobservable entities, it cannot be employed to justify discovery claims. Thus, Falconer's claim that the discovery of the electron was Thomson's exclusive experimental achievement is undermined.[27]

Besides offering a satisfactory account of the justification of existence claims concerning the unobservable, the "friends of discovery" (Nickles' expression) should tackle two related problems, what I will call the problem of knowledge and the problem of identification.

The problem of knowledge: Kuhn formulated the problem very succinctly, in relation to the discovery of oxygen: "Apparently to discover something one must also be aware of the discovery and know as well what it is that one has discovered. But, that being the case, how much must one know?" (Kuhn 1977, p. 170). Any entity that forms part of the accepted ontology of contemporary science is endowed with several properties. The electron, for instance, has a given mass, a certain charge, an intrinsic magnetic disposition (spin), a dual nature (particle versus wave), and many other features. The question then arises, how many properties must one have discovered in order to be granted the status of the discoverer of the entity in question? To give an example, Laszlo Tisza, a very well-known physicist, suggested to me that the electron was discovered in the late 1920s when C. J. Davisson & L. H. Germer and G.P. Thomson detected its wave properties. From the measurement of the electron's wavelength it became possible to calculate its momentum. That, according to Tisza, rendered electrons directly detectable.[28]

Another aspect of the problem of knowledge concerns mistaken beliefs. If knowing what one has discovered is a prerequisite for being credited with the discovery, then can one be considered the discoverer of, e.g., the electron even though he entertained wrong beliefs about it? For instance, in 1897 J. J. Thomson thought of his corpuscle, an entity later identified with the electron, as a structure in the ether. Leading physicists at the time (e.g., Joseph Larmor and H. A. Lorentz) entertained similar "wrong" conceptions of the electron. To put the problem in terms of the discovery—justification distinction, how many beliefs about an entity should be justified for the entity in question to be (considered) discovered?

The problem of identification: If most, or even some, of the beliefs that the putative "discoverer" had about the "discovered" entity are wrong, it is not at all evident that the entity in question is the same with its contemporary counterpart. It has to be shown, for instance, that Thomson's "corpuscles", which were conceived as classical particles and structures in the ether, can be identified with contemporary "electrons", which are endowed with quantum numbers, wave-particle duality, indeterminate position-momentum, etc. The "friends of discovery" should propose some criteria that enable us to identify the original entity with its present counterpart. I have attempted elsewhere to come to terms with the problem of identification, for reasons not directly related to scientific discovery (Arabatzis 2006). Nevertheless, I will sketch below a more neutral approach to the discovery of unobservable entities, which has the advantage of avoiding that problem altogether.

Because of these problems I would be extremely reluctant to base a historical narrative about the electron (or any other unobservable entity, for that matter) on the traditional, realist notion of scientific discovery. Moreover, this notion is often an obstacle to historical understanding. Consider, for example, the case of energy conservation. Several parallel developments, from the study of steam engines to theoretical mechanics to physiology, contributed to the formulation of that principle. Until recently, historians portrayed those developments as "simultaneous discoveries". This view, however, has been plausibly challenged, because all those putative discoverers of energy conservation were concerned with different problems and came up with different theoretical hypotheses. It was only in the 1850s that these different approaches were reinterpreted as aspects of the same discovery (Smith 1999).

Besides, there is a straightforward alternative that avoids these problems. One should simply try to historicize the notion of scientific discovery, by adopting the perspective of the relevant historical actors, without worrying whether that perspective can be justified philosophically.[29] On this approach, one would first examine the context of generation, that is, show how a novel concept denoting an unobservable entity was introduced into the scientific literature. Then one would reconstruct the original context of justification, consisting of all the experimental and theoretical arguments that were given in favor of the existence of the entity in question. The next step would be to trace the developmental process that followed that initial stage and gradually transformed the corresponding concept. The evolution of any such concept resembles a process of gradual construction, which takes place in several stages. A realist might want to label the first stage of that process "the stage of discovery". In that case discovery should be construed as a gradual process of consensus formation within the scientific community whose outcome is the acceptance of an existence claim (e.g., "the electron exists") (Caneva 2001, p. 19; Stachel 1994, p. 143).

This appeal to the consensus of the scientific community should not be interpreted as a social constructivist position. My approach is constructivist, in the sense that the representation of, say, the electron was gradually constructed. However, it is not a (social) constructivist one in the usual sense, which implies that consensus within the scientific community is the outcome of professional interests, the distribution of power within the scientific community, etc.

5. CONCLUDING REMARKS

I have argued that the context of discovery, if we want to retain this expression, concerns an extended process, which involves both generation and justification. In view of the inextricability of discovery and justification, what can we conclude about the

DJ distinction? I think that my analysis undermines versions 1–4 of that distinction, as distinguished by Hoyningen-Huene. Take, for instance, version 4: the DJ distinction as an expression of the division of labour between history of science (and related "empirical" disciplines) and ("logical") philosophy of science. I hope to have shown that an adequate understanding of the process of scientific discovery (including justification) requires an integrated historical *and* philosophical approach. Understanding scientific discovery is not just an empirical task. The, apparently innocent, question "when and by whom was something discovered?" is not merely a request for factual information, but requires conceptual analysis. And conceptual (not merely logical) analysis is the hallmark of philosophy.

What about version 5 and the related "lean" version, advocated by Hoyningen-Huene? There seems to be a difference between a factual and an evaluative perspective towards scientific knowledge. It is one thing to understand how a scientific claim was generated and accepted and another to ask whether it is justified, in light of the available evidence. I wouldn't object to this version, provided "that facts have normative presuppositions" (Hoyningen-Huene, this volume). In particular, the descriptive statement "X discovered Y" embodies an evaluative judgment, namely that the evidence presented by X demonstrated Y's existence. It is this confluence of the descriptive and the evaluative that makes fruitful, or even indispensable, a joint HPS approach to scientific discovery.

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NOTES

1. This paper draws on material I have presented elsewhere. See my *Representing Electrons: A Biographical Approach to Theoretical Entities* (Arabatzis 2006).
2. Reichenbach 1938 is usually cited as the primary site of the DJ distinction. However, as Nickles has pointed out, the distinction found there "is merely one between scientific activity itself and that activity as logically reconstructed" (see Nickles 1980a, p. 12). The original context in which Reichenbach put forward the DJ distinction and his evolving take on it are explored in Howard, this volume, Richardson, this volume, and Schiemann, this volume.
3. As Hoyningen-Huene points out (this volume), the DJ distinction, as presented above, has various aspects ("versions"). What I say below undermines all of those versions, with the possible exception of version five and the "lean" one advocated by Hoyningen-Huene; but more on this at the concluding section of this essay.
4. Here I am paraphrasing Reichenbach.
5. See, for instance, Nersessian 1992; Nickles 1980b; Hoyningen-Huene, this volume.
6. See, for instance, Langley et al. 1987.
7. Kuhn 1977, p. 328. For a detailed analysis of Kuhn's criticism of the DJ distinction see Hoyningen-Huene 1987, pp. 508–509; Hoyningen-Huene, this volume; Sturm & Gigerenzer, this volume.
8. I borrow the term from Nickles 1980a.
9. See, for instance, Burian 1980, pp. 322–323; Curd 1980, pp. 201–202; Laudan 1980, pp. 174–175.

10. The exceptions are important. See, for instance, Gutting 1980; Hoyningen-Huene 1987; Koertge 1982; Kordig 1978; McMullin 1980.
11. Reichenbach neglected the variety of scientific discoveries and used the term "discovery" only in connection with scientific theories (Reichenbach 1938, p. 7). Thus, it is not surprising that he conflated discovery with the process of developing new theories.
12. As Whewell recognized, a "happy guess" does not constitute a discovery (Schickore, this volume). Popper made a similar point, when he referred to the "*tests* whereby the inspiration may be discovered to be a discovery, or become known to be knowledge." (Popper 1968, p. 31)
13. This idea has also been put forward by Nickles 1980a, p. 9. Nickles, in turn, credits Ryle (Ryle 1949, pp. 303–304).
14. A possible exception is Langley et al. 1987.
15. Quoted in Conant 1957, p. 13.
16. Cf. Potthast, this volume. It is true that the mere use of a term (or statement) does not necessarily commit its user to its presuppositions. For instance, one may say that "the sun rose today at 6:00 am", without believing that the sun actually moved. Thus, it is possible to use the term "discovery" in a weaker, non-realist sense. In section four, below, I attempt to show how this can be done with respect to discoveries concerning unobservable entities.
17. Cf. Friedrich Steinle's distinction between the private and the public aspects of scientific activity (Steinle, this volume). It is interesting that this was also Reichenbach's original version of the DJ distinction. See Reichenbach 1938, p. 6. Cf. Richardson, this volume and Schiemann, this volume.
18. The function and importance of reconstruction in science have been emphasized by Nickles 1989b.
19. Note, however, that even this version of the distinction is not free of difficulties. See Hoyningen-Huene this volume.
20. The connections between the discovery issue and the realism debate will be explicitly drawn below.
21. This is not peculiar to discoveries of this kind. As Steinle argues (this volume), the discovery of regularities and the formation of new concepts are also inextricable.
22. Since I have discussed elsewhere those discoveries in considerable detail, my presentation will be sketchy. For a detailed analysis I refer the interested reader to Arabatzis 1992; Arabatzis 1996.
23. Zeeman's achievement exemplifies one of Allan Franklin's "epistemological strategies", which "entails the elimination of all plausible sources of error and all alternative explanations". This strategy is part of the "arguments designed to establish, or to help establish, the validity of an experimental result or observation". See Franklin 1989, pp. 466 and 438, respectively.
24. Even though I do not believe that unobservable entities are discovered, in the traditional sense of the term "discovery", I will continue to use this term in this section for two reasons. First, because it is used by the proponents of views that I will be arguing against. Only after having argued against those views, I might be justified in dropping it. Second, because, as I will indicate below, the term might still be used to capture a distinction between two stages. The first stage is usually characterized by ontological debates, where the existence of an entity is contended. After that stage is over, a realist might claim that the entity has been discovered (i.e., that we know that it exists).
25. See Caneva 2001, pp. 18–19 for further examples.
26. This is just one problematic aspect of Hacking's entity realism. For further criticism of his view see Arabatzis 2001.
27. In correspondence, Falconer has suggested to me that she agrees with my "objections to using Hacking's criteria for reality to justify 'discovery'". But she still "think[s] it can be used as an analytical tool, to ask what did this or that physicist contribute to our understanding of the electron; did he help to give it manipulative reality for the physicist?" I do not have a problem with using Hacking's manipulability criterion in this way, provided that it gives a good descriptive account of how scientists construct "existence proofs" for unobservable entities. As I have argued elsewhere, however, it also faces difficulties in this respect. See Arabatzis 2006.
28. I would like to thank Prof. Tisza for discussing with me some of the ideas I develop here.
29. Cf. Arthur Fine's Natural Ontological Attitude (Fine 1986).

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