Herman C.D.G. de Regt

Peirce's Pragmatism, Scientific Realism, and the Problem of Underdetermination

1. Introduction

Although it is common opinion that science penetrates into the unobservable structures of the world by representing these faithfully in scientific theories, philosophers of science have always wondered what the proper status of these representations is. Do the postulated entities of our successful science, ranging from quarks to mental structures, *really* exist? Do we have epistemic reason to believe in their existence? Are scientific representations merely instrumental devices which *Homo sapiens* uses in coping with the complex phenomena of a risky world, or can we confidently assume that the instrumental success of these representations warrants the stronger claim that the unobservable entities postulated really do exist?

If we think that we have epistemic reasons to believe that our best current scientific theories tell us about the underlying, unobservable structures of the world, we commit ourselves to the doctrine of *scientific realism*.[1] Edward MacKinnon offered a historical development of scientific realism in the introduction to *The Problem of Scientific Realism* (MacKinnon 1972a).[2] Although I will not discuss this historical reconstruction of the problem, it is interesting to note that scientific realism is an old philosophical doctrine and that the argument for scientific realism has not changed that much through time. There is a striking passage in Lucretius' *De Rerum Natura* (I, 265-328) which may illustrate how old the problem of scientific realism is. In his laudatory poem to Epicures' representation of the world in which he postulates "atoms," he wonders:

> Perhaps, however, you are becoming mistrustful of my words because these atoms of mine are not visible to the eye. Consider therefore, this further evidence of bodies whose existence you must acknowledge though they cannot be seen. First, wind [whips] up waves, [and] founders tall ships [therefore] there must be invisible particles of wind which sweep the sea and

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> land and the clouds in the sky ... Then again, we smell various scents of things though we never see them approaching our nostrils. Similarly, heat and cold cannot be detected by our eyes, and we do not see sounds. Yet all these must be composed of bodies, since they are able to impinge on our senses. For nothing can touch or be touched except body. [It] follows that nature works through the agency of invisible bodies.

Here we are in fact presented with an argument for "scientific" realism! If we accept the fact that 'nothing can touch or be touched except body' only the hypothesis of atoms can explain the various observable phenomena. Therefore, we are allowed to believe the hypothesis. Surprisingly, this argument, ***an inference to the only explanation***, is also the main argument in 20th century defenses of scientific realism.[3] Let us consider once again the ***locus classicus*** for the contemporary defense of scientific realism.

In his ***Philosophy and Scientific Realism*** (1963) Smart objects to what he calls phenomenalism. Phenomenalism in philosophy of science is the doctrine that statements about electrons, protons etc., can be translated into statements about galvanometers, Wilson barrels etc. According to phenomenalism electrons and protons are merely logical constructs from macro-objects (1963: 27). Against this phenomenalism Smart wants to defend scientific realism. First of all he argues that phenomenalism is not compelling. Arguments that are supposed to show that phenomenalism follows logically from premises the realist accepts are flawed (specifically Craig's theorem). But, though one cannot be logically coerced to accept phenomenalism it seems nevertheless a very plausible philosophy of science. Why should we not be careful and accept the weaker hypothesis? Smart suggests that phenomenalism cannot explain what scientific realism explains. Let us quote in full length the crucial passage from his book:

> If the phenomenalist about theoretical entities is correct we must believe in a ***cosmic coincidence***. That is, if this is so, statements about electrons, etc. are of only instrumental value: they simply enable us to predict phenomena on the level of galvanometers and cloud chambers. They do nothing to remove the ***surprising character*** of these phenomena. Admittedly the physicist will not be surprised in the sense that he will find these phenomena arising in unexpected ways: his theory will have instrumental value in preventing this sort of surprise. But, if he is reflective, he ought still to find it surprising that the world should be such as to contain these odd and ontologically disconnected phenomena, *i.e.* the phenomena are connected only by means of a purely instrumental theory. Is it not odd that the phenomena of the world should be such as to make a purely

> instrumental theory true? On the other hand, if we interpret a theory in a realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there really are electrons, etc., this is just what we should expect. A lot of surprising facts no longer seem surprising. Marshall Spector has in correspondence drawn my attention to the importance for the problem of theoretical entities of C.S. Peirce's notion of 'abduction'. (We argue from *A* to *B* by abduction when we point out that the previously surprising fact *A* is no longer surprising on the assumption of the truth of *B*.). (Smart 1963, 39)

The argument is an inference to the best explanation. We may choose between the explanation that the instrumental success of a theory is a cosmic coincidence or that the theory depicts the unobservable structures of the world faithfully. The latter is classified as the best. The argument may even be considered to be an inference to the *only* plausible explanation if we reject the former explanation as unsatisfactory. The allusion to Peirce characterises the argument as an abductive argument.

In this article I will argue that Peirce's notion of abduction has nothing to do with this contemporary "inference to the best (or only) explanation" defense of scientific realism. Secondly, I will consider Peirce's case for scientific realism to see if and, if so, how he himself defended this theory of science. Thirdly, since we will identify underdetermination of theory by data as the main threat to scientific realism, I will extensively discuss the question whether Peirce was aware of this problem of scientific realism and whether we may find a solution to this problem in Peirce's impressive papers. My conclusion will be that there is no Peircean solution to the contemporary problem of scientific realism.

2. The Peircean Notion of Abduction in Contemporary Defenses of Scientific Realism.

Peirce offers the following well-known paradigmatic case of abduction:

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis — which is just what abduction is — was subject to certain conditions, namely the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts of them. The form of inference, therefore, is this:

(1) The surprising fact, C, is observed;
(2) But if A were true, C would be a matter of course,
(3) Hence there is reason to suspect that A is true.
(*CP* 5.189, cf. 2.511n, 2.624)

As Haack notices, if we transform the form of inference in a deductive manner (B, if A then B, therefore A) abduction surely qualifies as a fallacy. However, Peirce starts from the other end and argues that logic is the classification of arguments, that abduction is clearly an argument, and that, therefore, not all arguments are deductive. Next to deductive inferences there are also inductive and even abductive inferences (Haack 1977, Peirce CP 8.384). If we accept Peirce's opinion here, we could argue that, if 'inference to the best explanation' is a clear example of abductive reasoning, it is saved from being a logical fallacy. Although one might recast inference to the best explanation in a deductive mould by which it becomes a deductive fallacy it is nevertheless a logically valid abductive argument. One should notice that the term 'surprising' is of the utmost importance for the abductive validity of the argument.

So, the original and abductive defense of scientific realism could analogously be constructed along these Peircean lines:

(1) We are ***surprised*** by the fact that science is instrumentally successful.
(2) If scientific realism is true we understand best why science is instrumentally successful, for if realism is true the instrumental success of science would be a matter of course.
(3) Hence, there is reason to suspect that scientific realism is the true theory of science.

In my opinion, however, it would be a great historical distortion to reconstruct the abductive defense of scientific realism along these so-called 'Peircean' lines. In fact, Peirce had nothing to do with this wrong-headed way of defending the claim that we have good reasons to believe in the existence of unobservables. In this section I will try to bear out this assertion.

Whenever contemporary philosophers of science point to the autonomous abductive character of their argument for scientific realism and refer to the work of Peirce for an 'abductive logic,' the crucial question should be how the notion of abduction actually developed through the work of Peirce and what kind of notion of abduction Peirce himself accepted as satisfactory. Luckily K. T. Fann (1970) took great pains to map the different notions of abduction in Peirce's oeuvre and it is to this study I now turn.

The overall conclusion drawn by Fann is that there are roughly (and chronologically) *two* theories of abduction present in Peirce's papers (1970: 9-10).[4] The first theory of abduction (1859-1890) is presented in Peirce's

"Deduction, Induction, and Hypothesis" (1878). In this paper Peirce takes abduction as an inference and he considers all inference as evidencing processes, so that abduction (as an autonomous mode of reasoning, *i.e.* not reducible to deduction or induction) must be considered as an argumentative, that is evidencing, autonomous mode of reasoning (EP: 189, cf. Kapitan 1992 for criticism).

In a later, ***second***, theory of abduction (1891-1914) Peirce employs a different notion of 'inference', viz. inference as a ***methodological*** process.[5] Now abduction becomes 'the First Stage of Inquiry,' that is, abduction is the methodological process by which one introduces or suggests a hypothesis as a candidate for a true explanation of certain phenomena, and the act of suggesting a hypothesis cannot be any evidence for the probability of the hypothesis.[6] In 1902 Peirce states:

> When after repeated attempts, I finally succeeded in clearing the matter up, the fact shone out that probability proper had nothing to do with validity of Abduction. (*CP* 2.101)

Fann refers in his study to CP 2.755 and 6.526 and concludes:

> What Peirce succeeded in clearing up is the notion of abduction as the reasoning that leads to the adoption of a hypothesis on probation and of induction as the testing of the hypothesis ... The distinctions between induction and hypothesis [abduction] made in the early period [1859-1890], however, are kept, in a modified form, as that between Quantitative and Qualitative induction in the later period. (1970: 26-7)

The consequences of this interpretation of abduction in the work of Peirce for the abductive defense of scientific realism in the form of either an inference to the only explanation or an inference to the best explanation (in the work of Boyd, Smart, McMullin and others) may now be clear. The notion of abduction as suggested by Peirce in his later work has no place in contemporary defenses of scientific realism, since scientific realism is simply an already formulated philosophical theory! If the process of abduction is 'the First Stage of Inquiry,' as Peirce ultimately thought, then the conclusion follows that the 'Inquiry into Scientific Realism' has long since passed this First Inquiry. The contemporary abductive defenses of scientific realism have the form of an abductive evidencing reasoning as distinguished by Peirce in his first, and afterwards revised, theory of abduction. The conclusion, based on the scholarly work of Fann and others,[7] as regards the contemporary abductive defenses of scientific realism must be that ***these are based upon a notion of abduction that was ultimately rejected by Peirce***: abductive processes have nothing to do with

the probability of the hypothesis, they cannot be evidencing processes.[8] If we try to defend scientific realism by an evidencing abductive argument, and if we present this line of argument as stemming from the work of Peirce we are distorting the history of philosophy.

3. Peirce's Scientific Realism

The next stage in the present inquiry is to see whether Peirce himself tried to defend some form of scientific realism and, if so, what kind of argument he used to accomplish this defense. Is there a problem of scientific realism in the work of Peirce, and if there is such a problem, how does he solve this problem if the possibility of an abductive defense of realism is impossible?

In his aforementioned article "Deduction, Induction, and Hypothesis" (1878)[9] we find, or so I think, a first clue to the way Peirce wants to defend some form of scientific realism. In this paper he says:

> When we adopt a certain hypothesis, it is not alone because it will explain the observed facts, but also because the contrary hypothesis would probably lead to results contrary to those observed. So, when we make an induction, it is drawn not only because it explains the distribution of characters in the sample, but also because a different rule would probably have led to the sample being other than it is. (*EP* 191)

The interpretation of this passage seems to be straightforward: Peirce thinks we may adopt a certain hypothesis as an explanation for some phenomena after *eliminative inductions.* To illustrate this claim consider the following examples given by Peirce in connection with the early (or 'wrong') notion of abduction (or 'hypothesis') .

If we find fossils of fish "far in the interior of the country" and try to seek an explanation of this (surprising) fact we infer to the hypothesis that once upon a time in the past the land was covered by sea. Other hypotheses cannot be ruled out beforehand and soon we are faced with a cluster of alternative hypotheses. Now we may apply (the rule of) eliminative induction and we conclude that successful predictions support the sea-hypothesis while they simultaneously refute alternative hypotheses. In the case of our belief in the existence of Napoleon we witness the same form of argument. How do we explain the presence of many documents and monuments concerning Napoleon? Peirce:

> Though we have not seen the man, yet we *cannot* explain what we have seen, namely, all these documents and monuments, *without* supposing that he really existed. (*EP* 189, my italics)

Although he presents these examples as examples of abduction the considerations Peirce discusses go beyond the mere suggestion or invention of the hypothesis, and I conclude that the well-known idea of eliminative induction obviously plays an important role in Peircean theory evaluation. The hypothesis on the existence of Napoleon is the *only* explanation for certain (surprising) phenomena, but the conclusion that it actually *is* the only explanation can only be reached or warranted after many eliminative inductions. So, relatively early in the work of Peirce we find, although confused with the notion of hypothesis or abduction, the concept of eliminative induction as a form of induction that leads one to infer the *only* explanation (cf. Bacon 1620, Bk. II). The more general conjecture is that many scientific reasonings have the form of an eliminative inductive inference to the only explanation.

However, up to this point we have not come far in characterizing Peirce as a scientific realist, given our working definition of scientific realism:

> we (sometimes) do have good (epistemic) reasons to believe that (some of) our best scientific theories tell us something about the unobservable structures of the world.

The examples given by Peirce concern observables only so that his conclusion does not have any bearing on the issue of scientific realism. Yet scientific realism has been an important theme in the writings of Peirce, I think, under the guise of his more encompassing *extreme scholastic realism* (Boler 1963), a term as specific as his term pragmaticism.

However, before elaborating on this extreme scholastic realism, and by that on Peirce's scientific realism, I will first, but only briefly, discuss two other characterisations of Peirce's scientific realism by two Peirce scholars, to wit Peter Skagestad and Robert Almeder, in order to illustrate how a reflection on Peirce's problem of scientific realism leads straight to the heart of his pragmatism.

3.1 Skagestad's Interpretation of Peirce's Scientific Realism

On the supposition that the instrumentalist is one of the major opponents to scientific realism it is interesting to discuss in some detail Peirce's attack on Karl Pearson's *The Grammar of Science* (1900). This attack appeared in the form of a book review in 1901 (CP 8.132-155). It is to this review that Skagestad (1983) turns (cf. Skagestad 1981).

Pearson can be characterized as a typical instrumentalist. Skagestad summarizes Pearson's instrumentalism as follows:

> on Pearson's view [science] can discover only empirical generalizations; theories which explain these generalizations are es-

> sentially beyond the scope of science, ***unless*** they are fully reducible to the empirical generalizations, thus serving merely as conceptual shorthand for the latter. (1983, 271-2)

Skagestad then offers an illuminating exposé of Peirce's critique of Pearson's instrumentalism and argues convincingly that Peirce's pragmatism is "actually opposed" to Pearson's instrumentalism (263). However, I think he misses an interesting point in Peirce's critique on Pearson that could connect Peirce's problem of scientific realism to the contemporary problem of scientific realism. In a long paragraph (CP 8.153), to which Skagestad also refers, the true objection to Pearson's instrumentalism is formulated. To Peirce, the most compromising claim in Pearson's admirable book is of course the suggestion that our scientific theories, and therefore the laws of nature, are nothing but ***figments.*** Peirce's argument against this instrumentalist suggestion reads as follows:

> the question is whether [law] is among those intellectual objects that are destined ultimately to be exploded from the spectacle of the universe, or whether, as far as we can judge, it has the force to stand its ground indefinitely [...] I, for my part, do not believe that any law is perfectly satisfied. If I am right in this, the reality of law is diminished; but it is not thereby abolished. But my argument to show that law is reality and not figment, — is in the nature independently of any connivance of ours, — is that predictions are verified. ***Nobody will maintain that these verifications are chance coincidences.*** (*CP* 8.153, my italics).

This is obviously an anticipation of the way Smart coined the argument for realism in his ***Philosophy and Scientific Realism.*** Since Peirce's argument is directed against the instrumentalist who thinks that one only needs empirical generalizations, that is, relations between observables, we are justified in saying that Peirce presumably wanted to defend a brand of scientific realism which may be categorized under our working definition of realism: on the basis of some implicit probability argument Peirce thought we had (epistemic) reason to believe in the existence of the postulated unobservables in instrumentally successful scientific hypotheses.[10] We are, therefore, confirmed in our claim that contemporary, so-called abductive, defenses of scientific realism take the discussion on realism away from the real issue, namely whether there is an inductive argument for scientific realism.

But more importantly, we notice that the notion of abduction Smart alluded to, by referring to a remark by Spector, ***plays no role at all*** in the Peircean argument for realism. Neither is realism suggested as a new hypothesis, nor is it a hypothesis that is defended by some abductive argument. It is rather an al-

ready existing hypothesis that seems to be defended by some *implicit* probability argument: although one *could* maintain that these verifications are chance coincidences, no one *does* maintain that these verifications *are* chance coincidences, for we do not doubt in our hearts that these verifications actually point toward reality, and "we should not doubt in philosophy what we do not doubt in our hearts." That is to say, we *feel* that the chance that these verifications are indeed chance coincidences is non-existent (or perhaps negligible), and this feeling is substantiated by the continuous accumulation of successful eliminative inductions.

Skagestad will of course object to this interpretation of Peirce's critique. He argues that according to Peirce no one maintains that scientific verifications are chance coincidences since to assume otherwise will block the road of inquiry, "the one mortal sin in Peirce's book" (275). And I agree. However, with the recent revival of empiricism, mainly because of the great effort by van Fraassen, it is now questioned whether nominalism *will* block the road of scientific inquiry.[11] Therefore, one is forced to dig deeper for the true motive in Peirce's realist philosophy of science. And this deeper motive must be the simple intuition that the problem of underdetermination, which is the main reason why one should be an empiricist, is not a real threat to realism. I turn to the problem of underdetermination in section 4.

Peirce's scientific realism, then, is primarily a realism about laws. Yet, contemporary scientific realism is an existential claim about unobservables. So the difficult question remains how Peirce thought about the existence of unobservables. Did he think some of these laws postulate unobservables? In section 3.3 I will address this question, but first we'll have a look at Almeder's interpretation of Peirce's scientific realism.

3.2 Almeder's Interpretation of Peirce's Scientific Realism.

Robert Almeder also investigates the question whether Peirce may be characterized as a scientific realist (Almeder 1989, cf his 1983). His point of departure is not Peirce's opposition to instrumentalism but his theory of truth and Peirce's optimism regarding progress in science and scientific thought.

As is well-known, according to Peirce true scientific theories are theories on which ultimately, *in the long run*, scientists will reach consensus (cf. Peirce's review of Fraser's *The Works of George Berkeley* 1861, CP 8.7-38). Some authors interpret Peirce's theory of truth in such a way that science will never be in this state and that the phrase 'in the long run' indicates some ideal limit that will never be reached. Others however, and Almeder is amongst them, suggest that Peirce thought that this final stage will actually be reached some day, though only after billions of years and not necessarily here on earth nor necessarily by *Homo sapiens*. This Peircean opinion on the convergence of science expresses, so Almeder argues, a scientific realism (cf. Laudan 1981b).

Subsequently, Almeder suggests that Peirce obviously must have thought that there is a finite number of non-trivial empirical questions. For, if there is an infinite number of such questions, we could never answer these questions in a finite time so that the scientific realism of Peirce, the actually accomplished convergence, is in jeopardy. The result of Almeder's study is a bit disappointing because his strategy of defense of scientific realism consists in essence in arguing that the ball is in the anti-realist's court and that the burden of proof is on him to show that there is an infinite number of non-trivial empirical questions (1989, 362). Almeder concludes that, since such proof is still lacking, Peirce's realism, at least for the time being, is saved.

In my opinion, however, Almeder ignores a crucial element of scientific realism, indeed, the element that makes this realism a scientific realism, namely the claim that the realist typically and on *epistemic* grounds believes in the *unobservable* entities postulated by our best *scientific* theories. The discussion of Peircean scientific realism as presented and discussed by Almeder is completely neutral with regard to this claim. Does the convergence Peirce refers to (if Almeder's interpretation is correct) involve theories that postulate only observable entities or does it also involve theories that postulate unobservables? Almeder's problem of scientific realism, the problem of the convergence of theories which are answers to *empirical* questions, that is questions concerning *observables*, is, however interesting, not a problem that is specifically a realist problem. For Peirce to be a scientific realist he should think that he, and others of his time, had good reasons to (tentatively) believe that the unobservable entities postulated by the best scientific theories of their time really existed.

3.3. Peirce's Extreme Scholastic Realism.

Looking for an acceptable description of some Peircean form of scientific realism we are faced with his complex and idiosyncratic extreme scholastic realism (cf. his "Lessons from the History of Philosophy" CP 1.15-42). Whenever Peirce calls himself a realist he does so to contrast his position with a position he himself once held for only a brief moment, namely the nominalist one (CP 5.470). In an illuminating study, Susan Haack discusses the importance of Peirce's extreme scholastic realism for contemporary philosophy of science and I will accept her reconstruction of this realism (Haack 1992).

According to Peirce there are undeniable facts. These undeniable facts can be explained or can be made intelligible if we accept the truth of extreme scholastic realism. However, nominalism asserts that it can explain these facts as well and *more economically*.[12] The very old issue that originated with these different philosophical claims, the realism-nominalism debate, is lucidly summarized by Peirce:

> The question ... is whether *man*, *horse*, and other names of

> natural classes, correspond with anything which all men, or all horses, really have in common, independent of our thoughts, or whether these classes are constituted simply by a likeness in the way in which our minds are affected by individual objects which have in themselves no resemblance. (*CP* 8.12)

The first question one raises is: what are these undeniable facts that, according to Peirce, can be explained by realism but not by nominalism?[13] Haack reconstructs Peirce's argument for his extreme scholastic realism along the following lines:

> His view ... was something like this: the science of his day, though thoroughly fallible and incomplete, had some *success*, success partly explicable in evolutionary terms; that this constituted grounds for supposing that *genuine scientific explanation*, etc. is possible; that the possibility of genuine science requires the truth of scholastic realism; and that the real generals are those that would figure in the laws — some of them already known — which would be accepted in a hypothetical completed science. (Haack 1992, 29, my italics)[14]

Now, we are not concerned with the details of this argument for extreme scholastic realism as opposed to nominalism. We are only interested in the question whether Peirce was a scientific realist in our sense. Peirce was unmistakably a realist with regard to generals: there are real generals, although these generals have a mode of being which is different from existing (CP 6.349). Peirce then was indeed a scientific realist in the sense that he thought that it is science that determines which of the postulated generals are *real* generals.

To what extent does the Peircean formulation of scientific realism differ from the one suggested as the working definition throughout this paper? Compare

> *Contemporary scientific realism*: We have good reasons to suppose that our best scientific theories tell us something about the unobservable structures of reality, to

> *Peircean scientific realism*: We have good reasons to suppose that our best scientific theories tell us which of the postulated generals are real generals.

Now, generals are obviously unobservables, even when they concern classes of observables. In this sense, then, Peirce counts as a scientific realist. However, from the perspective of the current scientific realism/empiricism debate one

would rather like to concentrate on the question whether Peirce would recognize, besides natural classes of *observable* objects, natural classes of *unobservable* objects. In other words, one would like to know whether the generals Peirce thought indispensable for scientific inquiry concern observable objects only; whether we have epistemic reason to believe in the existence of unobservable objects.[15]

Evidently, as the famous example of the diamond shows, Peirce was obviously of the opinion that scientists do have good (epistemic) reason to suppose that there *are* unobservables like molecules to account for the hardness of the diamond by postulating the "high polymerization of the molecule" (CP 5.457). So, according to Peirce, it is to be expected that there are laws of nature of which the real generals postulated are concerned with natural classes of individual *unobservable* objects. It is the history of science that offers an inductive argument to believe that the postulation of unobservables is indispensable to real science. Peirce, then, was certainly a scientific realist in the contemporary sense of the word.

4. Peirce's Pragmatism and Underdetermination

The identification of Peircean extreme scholastic realism with contemporary scientific realism is admittedly problematic. Yet within this philosophically broad doctrine of scholastic realism Peirce evidently held on to scientific realism. According to him we have epistemic reason to believe in the existence of unobservables.

Against the instrumentalist Peirce used the same form of argument as his successors Smart, Putnam, Boyd and McMullin do. In each case it has the form of an "inference to the *only* plausible or satisfactory explanation." This inference to the only explanation is *not* an *abductive* argument but rather an *inductive* argument, and more specifically it consists in the eliminative inductive arguments scientists *themselves* give for their hypotheses (postulating unobservables). On the level of scientific theories Peirce offers the strength of these scientific eliminative inductions to argue that if scientists hold theories that postulate unobservables their tentative belief in the truth of the theories is warranted.[16] Unfortunately, the strength of (the rule of) scientific eliminative induction is seriously undermined by the phenomenon of underdetermination of theory by data.

Obviously contemporary scientific realism would be maximally defended if we could offer some probability argument that would take us from knowledge concerning observables to knowledge concerning unobservables. Eliminative induction in science could be such a probability argument at work. But if the list of alternative scientific hypotheses is incomplete the strength of an eliminative inductive argument is subsequently diminished since we might have overlooked some possible scientific explanations of the same facts that may be as

good as our "sole" survivor. Now, were the number of these neglected alternatives to be only very small and, more importantly, were these alternatives to have an ontology that differs only in some very limited number of aspects from our main candidate for truth, the strength of eliminative induction might in the long run be saved. If, subsequently, the only scientific explanation of some phenomena postulates unobservables, scientific realism seems maximally defended.

However, it is the fundamental underdetermination of scientific theories by data that ***completely*** destroys the philosophical plausibility of scientific realism. The problem of underdetermination is that, in the case of a scientific hypothesis which postulates unobservable entities, it is ***always*** possible to construct an alternative scientific hypothesis that is logically incompatible with, but nevertheless empirical equivalent to, that hypothesis. Moreover, not only is it always possible to construct at least ***one*** such alternative, it is always possible — so it is said — to construct an ***infinite*** number of such alternatives. If this is true, the strength of scientific eliminative inductive arguments is completely destroyed: not only do we have to face the possibility that, in the end, not one but two hypotheses survive the eliminative method, we must also reckon with the ***infinitely*** many alternative hypotheses, so that the probability of the initial hypothesis decreases to zero! If for any hypothesis H which postulates some ontology of unobservables the probability of the existential claims of H is zero, the hope of ever defending scientific realism (that says that we do have epistemic reasons to believe that our best scientific theories tell us about the unobservable structures of the world), is idle.[17]

The general problem of eliminative induction as an adjudicatory argument in theory evaluation and acceptance is simply the fact that we can never be sure that we have considered ***all*** alternative explanations for some phenomena. The danger of the incompleteness of the list of alternatives undermines the argument for scientific realism.

I presented Peircean scientific realism to argue that the true defense of scientific realism cannot be abductive. Instead, as the work of Peirce indicates, scientific realism gets its plausibility from the eliminative inductive arguments in science, where theories which postulate unobservables are the sole survivors of this rule of eliminative induction.[18] However, the problem of underdetermination now becomes the main threat to scientific realism.

In the immense secondary literature on the philosophy of science of C.S. Peirce there are at least (and to the best of my knowledge at most) three authors who claim that Peirce identified the problem of underdetermination of theory by data, where we take the problem of underdetermination to be the always existing possibility to construct infinitely many logically incompatible but empirically equivalent alternatives to instrumentally successful scientific theories postulating unobservables. According to these authors (Hookway, Skagestad and Almeder), Peirce was well aware of the threat of underdetermi-

nation to his scientific and scholastic realism. However, if we consider the arguments and references to the work of Peirce offered by these philosophers we must conclude that it is not clear at all whether Peirce was aware of the problem of underdetermination.

(1) In his well documented study Christopher Hookway discusses the problem of theory equivalence within Peirce's pragmaticism (Hookway 1985). Within this pragmaticism it is possible that there are two theories on the basis of which the same predictions are deduced which are subsequently verified while a common vocabulary in which these predictions can be described is lacking. We could, for the sake of argument, interpret this situation as a situation in which there are logically incompatible but empirically equivalent theories, that is to say, it is an example of a case of underdetermination. The conclusion would follow that Peirce was aware of the problem of underdetermination and we might look for a Peircean answer to this problem. However, Hookway's reference to the paragraph which is supposed to illustrate this awareness of Peirce, reads as follows (Hookway 1985: 249):

> I have already remarked that a definition of science in general which shall express a really intelligent conception of it as a living historic entity must regard it as the occupation of that peculiar class of men, the scientific men. The same remark may be extended to definitions of the different branches of science. The men who pursue a given branch herd together. They understand one another; they live in the same world, while those who pursue another branch are for them foreigners. (*CP* 1.99)

Here Peirce speaks of different branches of science. Although the classifications of the sciences is indeed a very important theme in the work of Peirce it is not to be confused with the discussion of the possibility of constructing logically incompatible yet empirically equivalent alternatives to some hypothesis for the same domain of phenomena. Clearly, the paragraph does not support the claim that Peirce was aware of the always existing possibility of underdetermination. Since Hookway does not offer us any other clues that might substantiate this claim we are pessimistic about the possibility that Peirce might have in store some solution to the problem of underdetermination.

(2) The already mentioned Peirce scholar Peter Skagestad is also of the opinion that Peirce was aware of the problem of underdetermination. Skagestad argues that Peirce saw the problem after studying the logic of his preceptor Augustus de Morgan. Skagestad mentions the crucial passage in the papers of Peirce by which this claim is illustrated and supported. The reference, however, is disappointingly unclear. I here offer the full citation (Skagestad 1981: 182):

> [There] are general laws connected with [dyads, one of whose subjects is a monad and the other a possible dyad, that is, a unit]. The first of these is that any unit (or units) whatsoever contemplated in itself without conscious regard to its parts would, were our sense to respond to it, be seen to embody a monad. De Morgan propounded this law, so far as it is pertinent to formal logic, affirming that any collection of objects whatsoever possess universally some character which belongs to no other object at all. For, said he, they at least possess the character of being units of that collection. Considered as a *proof*, this begs the question; but considered as another way of formulating the same phenomenon, and as a way which throws some light upon it, it has its value. This coincides with the principle of the excluded middle. Those objects of the universe which do not possess a given character possess another character which, in reference to that universe, is in relation of negation to the first. Hence, it is impossible to form a single class of dyads; two classes of dyads must be formed at once. Hence, considering all the monads which can appear on the contemplation of sets of units of the universe in their monadic aspect, every single unit is determined to be one subject of a dyad which has any one of those monads as its second object, namely it is either such a dyad as determines it to have the character of being one of the units which made up the object of contemplation in which that monad appeared, or it is such a dyad as determines the unit to have the character belonging to all the other units of the universe. (*CP* 1.449-50)

It may be the idiosyncratic idiom of Peirce that is the cause for my possible misunderstanding, but to my mind, the paragraph offers all but a clear formulation of the problem of underdetermination. If Peirce really identified the problem of underdetermination, and if he was really aware of the possibly devastating effect on the plausibility of the realism he wanted to defend, he should have referred to this problem openly and more than once, for instance in his "Lessons from the History of Science" (CP 1.43-125).

(3) Finally, Robert Almeder also claims that Peirce was aware of the problem of underdetermination. It is safe to accept the assertion, so Almeder claims, "that Peirce was well aware that physical theory is underdetermined by observational data" (1980: 44). Again one waits for the references to the papers of Peirce. But the only reference offered by Almeder in order to back up his claim is the following:

> The other variety of the argument from the fulfillment of pre-

> dictions is where truths ascertained subsequently to the provisional adoption of the hypothesis or, at least, not at all seen to have any bearing upon it, lead to new predictions being based upon the hypothesis of an entirely different kind from those originally contemplated and these new predictions are equally found to be verified. (*CP* 7.117)

Even though this paragraph is more convincing than those referred to by Hookway and Skagestad it is still a far cry from establishing the fact that Peirce was "well aware" of the problem of underdetermination. I think we should rather conclude, for the moment, that although Peirce defended some form of scientific realism, evidence for the claim that he was conscious of the threat from underdetermination is lacking. It is therefore not to be expected that we will find some Peircean argument that saves contemporary scientific realism from the threat of underdetermination.

There are at least two possibilities to escape the conclusion that Peirce was not aware of the problem of underdetermination. The *first* possibility concentrates on the notions of reality and truth. For instance, we could argue that the Peircean idea of reality, as that to which the scientific community will converge and conclude "in the long run", is precisely meant as an answer to the possibility of underdetermination and a rebuttal of scientific anti-realism. This idea of Peirce, which he already formulated in the closing paragraphs of his "Some Consequences of Four Incapacities" (1868), could indeed be interpreted as an answer to the problem of underdetermination if we emphasize the implication to the *very* far future. The idea is that, given the continuation of science through time, there will surely be cases of underdetermination in science, but these will be of a *temporary* nature only; they will ultimately disappear and *in the end* there will be no cases of underdetermination left. In a letter to Cantor Peirce formulates his theory of truth very concisely:

> By a *reality* [or truth] I mean anything represented in a true proposition. [By] a *true* proposition (if there is any such thing) I mean a proposition which at some time, past or future, emerges into thought, and has the following characters: First, no direct effort of yours, mine or anybody's, can reverse it permanently, or even permanently prevent its asserting itself; Second, no reasoning or discussion can permanently prevent its asserting itself; Third, any prediction based on the proposition, as to what ought to present itself in experience under certain conditions, will be fulfilled when those conditions are satisfied. (*NEM* III, 2: 772-79)

From Peirce's philosophy of science there clearly emerges the expectation that

when science has come to an (ideal) end underdetermination will have disappeared. Indeed, in the letter to Cantor we find the further claim that if, even in the long run, there are empirically equivalent theories, we must decide, on the basis of the pragmatic principle, that these theories are merely different formulations of the same theory. But are there arguments to be found in Peirce's papers that rule out the *logical* possibility of underdetermination?

The logical possibility of underdetermination follows from Putnam's well-known and extensively discussed paper "Models and Reality" (1977). There Putnam argues that it is *always* possible to construct infinitely many logically incompatible yet empirically equivalent theories. It is not my intention to discuss Putnam's paper in detail and I will only offer Putnam's conclusion of his philosophical *tour de force*. Putnam states:

> What Skolem really pointed out was this: no interesting theory (in the sense of first-order theory) can, in and of itself, determine its own objects up to isomorphism. And Skolem's argument can be extended, as we saw, to show that if theoretical constraints don't determine reference, then the addition of operational constraints won't do it either. (1983: 23)

In a very loose translation we could reformulate this conclusion as follows: if a first-order theory has a model, it has an infinite number of models; in other words, if we offer an interpretation of a scientific theory as a first-order theory, by constructing a model, we must be aware of the fact that if such a model *can* be constructed an infinite number of alternative models can be constructed. Or, in yet another wording, in that case many empirically equivalent yet logically incompatible ontologies are possible. In this way the Löwenheim-Skolem theorem is applied by Putnam as a *logical* proof for the claim that there *must always be* massive underdetermination involving empirically equivalent yet logically incompatible theories, *even in the longest run*.

So we are now confronted with two alternative general philosophies. Peirce, motivated by his pragmatic principle, would say that if (in the ideal limit) all empirical scientific results are in and if (say) electrons are postulated in one of our final rivalling hypotheses, the question whether electrons *really* exist is simply meaningless. For in that case there is no empirically detectable difference between the electron hypothesis and those hypotheses that deny the existence of electrons. An empiricist however would argue that *even then* there is reason to be at least agnostic (not to say radically sceptic) about the existence of electrons because of the logical problem of underdetermination. Were Peirce confronted with Putnam's argument for the logical possibility of underdetermination there would seem to be no easy way out for the realist-minded pragmatist, since the infinitely many alternative models (the intended and the unintended) are ontologically incompatible, that is to say, they postulate *different*

relations in the world; the models are *not* just different formulations of the same hypothesis. Now, if this is the case, it seems that Peirce must admit that, although there is (in the ideal limit) no empirically detectable difference between the electron hypothesis and its rivals, the difference between the hypotheses is significant. In other words, *if there is a decisive argument for logical underdetermination*,[19] it is the very principle of Peircean pragmatism that is under pressure! Was Peirce aware of this? I think not, but who could blame Peirce for not considering an argument that wasn't available to him? However, it does have implications for the present discussion of contemporary scientific realism: there seems to be no specific Peircean argument that may come to our rescue whenever we feel that scientific realism is in jeopardy!

The *second* possibility to answer the critique that Peirce was not aware of the problem of underdetermination, stems from his semeiotics and consists in emphasizing the Peircean notion of "real vagueness": a sign which is objectively indeterminate is objectively *vague* to the extent that it must be determined by other signs; however, the complete determinacy of a sign is simply impossible for it can always be logically divided into further analyzable elements (CP 5.447, 1.434, Engel-Tiercelin 1992: 66-7). Here we embark upon Peirce's opaque theory of continuity. Perhaps this notion of vagueness can be identified with the Peircean version of the thesis of underdetermination. I cannot exclude this hypothesis since I am unable, given the many difficulties surrounding Peirce's theory of continuity, to assess the plausibility of this interpretation. If I understand Engel-Tiercelin's reference to manuscript #283 correctly I must conclude that the problem of underdetermination of theory by data is ultimately something different from the problem of the indeterminacy of signs. Drawing from this manuscript Engel-Tiercelin suggests that:

> a sign is indefinite if its interpretation remains doubtful, but not if there is a choice between possible interpretations (which is ambiguity). (1992: 68)

My suggestion here would be that the problem of underdetermination in the philosophy of science is indeed a case of ambiguity, having epistemological consequences, rather than indeterminacy.[20]

5. Conclusion

I think Peirce ultimately saw the problem of underdetermination as the result of a wrong-headed philosophy. Given the principle of pragmaticism, Peirce's own grand empirical hypothesis, the problem of underdetermination simply disappears, as we witnessed in Peirce's letter to Cantor. However, in our time the problem of logical underdetermination is a serious threat to scientific realism, as I understand it here.

Perhaps Peirce wasn't fully aware of the problem of underdetermination because of the simple fact that ***he did not have a theory of the structure of scientific theories.*** It may be enlightening to notice that Pierre Duhem was one of the first to discuss the structure of physical theories in 1906, and he was very much aware of the problem of underdetermination. Although Peirce, who died in 1914, was very interested in European philosophies of science (especially in those of Mach and Poincaré) it seems unlikely that he was in a position to account for this Duhemian analysis of physical science and to incorporate the problem of Duhemian underdetermination and the denial of crucial experiments, and subsequently the more general formulation of the problem of underdetermination, in his philosophical system. Nowhere, as far as my knowledge goes, do we find any reference to the work of Duhem in the papers of Peirce.[21] If we accept that with Duhem's discussion of the nature and structure of scientific theories the thesis of underdetermination becomes (one way or another) the main threat to scientific realism, it becomes intelligible why Peirce was probably not fully aware of this threat to his scientific and realistic pragmaticism.

To my mind we are not justified in concluding that Peirce was not aware of the threat of underdetermination of theory by data to the plausibility of his scientific realism. We can however conclude that he was not fully aware of the logical possibility of underdetermination, which might have something to do with the fact that he did not have a theory of the structure of scientific theories. Of course, Peirce cannot be blamed for not knowing the Löwenheim-Skolem theorem and its application to scientific hypothesis by Putnam in 1977.

The sad overall-conclusion to be drawn is that the difficulties of contemporary scientific realism, stemming from the logical problem of underdetermination, cannot be solved by an appeal to the ingenuity of Peirce. Sometimes even a genius can raise expectations that cannot be met.[22]

Tilburg University, The Netherlands

NOTES

1. I take van Fraassen to be the main opponent to current scientific realism (van Fraassen 1980, 1985, 1989), and that is why I use the term 'epistemic reasons' here. See my (1996) for a further elaboration on van Fraassen's notion of rationality.

2. See also his systematic analysis of the problem of scientific realism in that introduction (1972b: 39-71) and his "Scientific Realism: the New Debates" (1979).

3. See for instance Sellars (1963), Putnam (1975), McMullin (1984), W. Salmon (1984), Boyd (1984), Musgrave (1988), Thagard (1988), Hooker (1995), and also Harré (1972), (1994), Hacking (1983), (1985), Cartwright (1983), P.M.

Churchland (1979), (1985), Derksen (1994b), and my *The Case for Scientific Realism* (1995) in which I try to substantiate the claim that not much has changed since Lucretius' defense of "scientific" realism.

4. The importance of the development of the notion of ***abduction*** in these papers of Peirce cannot be emphasized enough given Peirce's remark that "If you carefully consider the question of pragmatism you will see that it is nothing else than the question of the logic of abduction" (*CP.* 5.196).

5. Dewey: "As far as I am aware, [Peirce] was the first writer on logic to make inquiry and its methods the primary and ultimate source of logical subject matter" (*Logic*, 9n1). Cf. Peirce's remark that "each step in science has been a lesson in logic" (*CP.* 5.363).

6. Ian Hacking speaks of "the mature Peirce" (1989: 568).

7. For instance Burks (1946), Thagard (1988), (1981), Shanahan (1986), Hookway (1985, ch. 7), Skagestad (1981), Almeder (1980), Rescher (1979).

8. However, the abductive scheme can be very tempting as an evidencing argument especially in philosophy where the Second and Third Stages of Inquiry (deduction and induction respectively, cf. *CP.* 7.202-7.206) are (principally?) omitted. Abductive reasoning is very often conflated with (Kantian) transcendental reasoning which may be seen as a typical philosophical style of reasoning. As Peirce discovered in his studies on the syllogistic form, abduction appears under the name of ***apagoge*** in Aristotle's *Analytica Priora*. Here we find Aristotle struggling with the proper characterisation of abduction. To Aristotle ***apagoge*** is clearly a dialectic syllogism. But if this is true one expects abduction to have a place in the *Topica* since that is the place where Aristotle discusses this kind of syllogisms. The Aristotle scholar Heinrich Maier seems to have an explanation for this curious fact: abduction must be interpreted as a dialectic instrument for ***apodeictic*** science (1970, 453). Nevertheless, it seems that abduction does not have a natural place in the *Analytica Priora*, but we will leave this question to the scholars.

9. *EP* 186-199, *CP* 2.619-44.

10. Of course, a scientific realism that defends the reality of scientific *laws* is different from a scientific realism that defends the existence of the ***entities*** postulated in those laws. I will not discuss entity realism as a brand of scientific realism since here I am concerned with the ***nature*** of the defense of some form of scientific realism (but see my 1994, 1995, ch. 8).

11. See for instance van Fraassen's description of Robert Millikan's experiments in *The Scientific Image*. In his (1989) French also deals with this matter. His main thesis is that "the essence of Peirce's answer to scepticism is that sceptical doubts are not genuine doubts because they do not halt or prevent practical activity (in the broadest sense) and are therefore not resolvable" (1989, 298), and he argues that this Peircean insight might play a role in the ongoing debate between realists and empiricists. As will become more clear below I doubt whether this Peircean response to the current debate on scientific realism is effective.

12. Note the similarity between Peirce's characterisation of the nominalism-realism debate and the instrumentalism-scientific realism debate.

13. Cf. Peirce's *Harvard experiment*.

14. Haack speaks of "real generals that figure in the laws of a completed science". To Peirce however laws themselves are also generals.

15. However, see for instance the studies by Armstrong (1978a,b),

(1989).

16. Even on the level of his metaphysical scholastic realism, which Peirce offered as a testable hypothesis about the nature of the world, he argues that the verifications of predictions is an inductive argument *for* realism and simultaneously *against* nominalism.

17. The further hope of realists, of course, is that empirically indistinguishable hypotheses need not be evidentially indistinguishable. However, the Putnamian logical argument from underdetermination I'll refer to below seems also to hold for evidentially equivalent hypotheses.

18. In his *Science and Hypothesis* Larry Laudan (1981a) investigates the rise of the method of hypothesis. This method was historically offered as an alternative to the method of enumerative induction. Laudan states: "By the 1740s and 1750s [a] number of scientists — and philosophers — began developing theories which, in the nature of the case, could not conceivably have been arrived at by enumerative induction. Franklin's fluid theory of electricity, the vibratory theory of heat, the Buffonian theory of organic molecules, and phlogiston chemistry are but a small sample of the growing set of theories in the middle of the 18th century, which hypothesized *unobservable* entities in order to explain observable processes." (1981a: 12, my italics). The problem of hypothesis is identical to the problem of scientific realism: "if we have an hypothesis (or theory) all of whose thus far examined consequences are true, then what — if anything — can we warrantedly infer about the truth or likelihood or verisimilitude or well-testedness of the hypothesis?" (Laudan 1981a: 5) The realist claims that we can warrantedly infer the likelihood of the hypothesis.

19. See Laudan & Leplin (1991), (1993) and Kukla (1993) for a discussion on the question whether there is a decisive argument for the logical possibility of underdetermination.

20. To what extent there may be a parallel between the Peircean discussion on indeterminacy and ambiguity of signs and Quine's discussion of indeterminacy and underdetermination, remains to be seen.

21. More importantly, I know of only one Peirce commentator who acknowledges the fact that Peirce's system of philosophy indeed lacks any theory on the structure of scientific theories. In a footnote Riemer keenly observes that "[dieser] Mangel an zusätzlicher Differenzierung zum Gesetzts- und Theoriebegriff tritt besonders deutlich hervor, wenn man Peirces Schriften in dieser Hinsicht mit dem Werk seines Zeitgenossen Duhem, P. *Ziel und Struktur der physikalischen Theorie* (1908) vergleicht." (1988: 62-3).

22. I would like to thank Menno Hulswit and Guy Debrock at the Dutch *C.S. Peirce Study Centre* (Nijmegen University) for fruitful discussions on these Peircean topics, and Sam Mitchell for his helpful remarks on an earlier version of the paper.

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