HOW TO STUDY SCIENTIFIC EXPLANATION?[1]

Erik Weber, *Leen De Vreese & Jeroen Van Bouwel*

Centre for Logic and Philosophy of Science
Ghent University (UGent)
Blandijnberg 2
B-9000 Gent
Belgium
Erik.Weber@UGent.be
Leen.DeVreese@UGent.be
Jeroen.VanBouwel@UGent.be

Abstract
This paper investigates the working-method of three important philosophers of explanation: Carl Hempel, Philip Kitcher and Wesley Salmon. We argue that they do three things: (i) construct an explication in the sense of Carnap, which then is used as a tool to make (ii) descriptive and (iii) normative claims about the explanatory practice of scientists. We also show that they did well with respect to (i), but that they failed to give arguments for their descriptive and normative claims. We think it is the responsibility of current philosophers of explanation to go on where Hempel, Kitcher and Salmon failed. However, we should go on in a clever way. We call this clever way the "pragmatic approach to scientific explanation." We clarify what this approach consists in and defend it.

Keywords
Carnap, explanation, explication, Hempel, Kitcher, Salmon.

---

1 The authors thank Raoul Gervais, Merel Lefevere and the audience of the 3rd SPSP conference in Exeter for their comments on this paper. The research for this paper was supported by the Research Foundation Flanders (FWO) through research project G.0031.09.

HOW TO STUDY SCIENTIFIC EXPLANATION?

1. Introduction

This paper investigates the working-method of three important philosophers of explanation: Carl Hempel, Philip Kitcher and Wesley Salmon. We argue that they do three things: (i) construct an explication in the sense of Carnap, which then is used as a tool to make (ii) descriptive and (iii) normative claims about the explanatory practice of scientists. In Section 2 – which has a preliminary character – we present Carnap's view on what the task of explication is, on the requirements it has to satisfy and on its function. In Section 3 we show that Carl Hempel develops an explication of the concept of explanation and makes descriptive and normative claims with it. We also show that he fails to provide convincing arguments for these claims. In Section 4 and 5 we show that Philip Kitcher and Wesley Salmon had a similar working-method in their philosophical analysis of scientific explanation and failed at the same stage as Hempel: the arguments for the descriptive and normative claims are missing.

We think it is the responsibility of current philosophers of explanation to go on where Hempel, Kitcher and Salmon failed. However, we should go on in a clever way. We call this clever way the "pragmatic approach to scientific explanation." In Section 6 we clarify what this approach consists in and defend it.

For readers who are not very well acquainted with the philosophical literature on scientific explanation, we have added an appendix containing a brief overview of Hempel's DN-model and the problems that other philosophers saw in it.

2. Carnap on explication

Carnap devotes Chapter 1 of his *Logical Foundations of Probability* to the notion of explication. The following quote summarises his view:

> According to these considerations, the task of explication may be characterized as follows. If a concept is given as explicandum, the task consists in finding another concept as its explicatum which fulfils the following requirements to a sufficient degree.
> 1. the explicatum is to be *similar to the explicandum* in such a way that, in most cases in which the explicandum has so far been used, the explicatum can be used; however, close similarity is not required, and considerable differences are permitted.
> 2. The characterization of the explicatum, that is, the rules of its use (for instance, in the form of a definition), is to be given in an *exact* form, so as to introduce the explicatum into a well-connected system of scientific concepts.
> 3. The explicatum is to be a *fruitful* concept, that is, useful for the formulation of many universal statements (empirical laws in the case of a nonlogical concept, logical theorems in the case of a logical concept).
> 4. The explicatum should be as *simple* as possible; this means as simple as the more important requirements (1), (2), (3) permit. (1950, p. 7)

According to Carnap, scientists often make explications. One of his examples is the replacement of the concept of Fish by the concept of Piscis:

> That the explicandum Fish has been replaced by the explicatum Piscis does not mean that the

> former term can always be replaced by the latter; because of the difference in meaning just mentioned, this is obviously not the case. The former concept has been succeeded by the latter in this sense: the former is no longer necessary in scientific talk; most of what previously was said with the former can now be said with the help of the latter (though often in a different form, not by simple replacement).
> ...
> ... [T]he concept Piscis promised to be much more fruitful than any concept more similar to Fish. A scientific concept is the more fruitful the more it can be brought into connection with other concepts on the basis of observed facts; in other words, the more it can be used for the formulation of laws. The zoölogists found that the animals to which the concept Fish applies, that is, those living in water, have by far not as many other properties in common as the animals which live in water, are cold-blooded vertebrates, and have gills throughout life. Hence, the concept Piscis defined by these latter properties allows more general statements than any concept defined so as to be more similar to Fish; and this is what makes the concept Piscis more fruitful. (1950, p. 6)

His second scientific example is Temperature as explicatum of Warmer:

> The concept Temperature may be regarded as an explicatum for the comparative concept Warmer. The first of the requirements for explicata discussed in §3, that of similarity or correspondence to the explicandum, means in the present case the following: The concept Temperature is to be such that, in most cases, if x is warmer than y (in the prescientific sense, based on the heat sensations of the skin), then the temperature of x is higher than that of y. (1950, p. 12).

In both cases, the explicatum allows us to formulate more empirical laws than the explicandum, and that is why they are fruitful.

According to Carnap, mathematicians also provide explications. For instance, the definitions of natural number as given by Russell and Whitehead in their *Principia Mathematica* are – in his view – explicata for prescientific arithmetical terms (numerals and operations). The fruitfulness of these explicata lies in the fact that "Peano's axioms become provable theorems in logic" (1950, p.17). Logic is to be understood here as first order predicate calculus plus set theory. Here the fruitfulness lies in more theorems rather than more empirical laws (as already indicated in condition 3 in the first quote).

Finally, Carnap claims that also philosophers often make explications. His aim in his 1950 book is to develop a good explication of the concepts of probability and (degree of) confirmation. Before we turn to the use of the concept of explication by philosophers of explanation, it is important to note that the fruitfulness of philosophical explications may be of a different nature than the fruitfulness of scientific and mathematical ones. It is possible that the fruitfulness of philosophical explications lies in the formulation of empirical generalisations (like scientific explications) and/or logical theorems (like mathematical explications). But a serious option to consider is that their fruitfulness lies in that they allow us to offer clear guidelines for scientists (i.e. we formulate norms with them, not empirical generalisations or logical theorems).

3. Hempel's working-method

Hempel's first stage: explication

It is clear that Hempel sees his covering law model as an explication in the sense of

Carnap. At the end of the long essay 'Aspects of Scientific Explanation' He writes:

> This construal, which has been set forth in detail in the preceding sections, does not claim simply to be descriptive of the explanations actually offered in empirical science; for - to mention but one reason - there is no sufficiently clear generally accepted understanding as to what counts as a scientific explanation. The construal here set forth is, rather, in the nature of an *explication*, which is intended to replace a familiar but vague and ambiguous notion by a more precisely characterized and systematically fruitful and illuminating one. Actually, our explicatory analysis has not even led to a full definition of a precise "explicatum"-concept of scientific explanation; it purports only to make explicit some especially important aspects of such a concept.
> Like any other explication, the construal here put forward has to be justified by appropriate arguments. In our case, these have to show that the proposed construal does justice to such accounts as are generally agreed to be instances of scientific explanation, and that it affords a basis for a systematically fruitful logical and methodological analysis of the explanatory procedures used in empirical science. (1965, pp. 488-489)

If we compare this with Carnap's criteria, we notice the following:
(1) Two of Carnap's criteria are explicitly mentioned: *fruitfulness* and *exactness*.
(2) Hempel does not mention *similarity to the explicandum* explicitly. However, proposals should do justice to what are generally agreed as instances of explanation (cfr. the last sentence of the quote). "Doing justice to" is vague, but we think it can safely be assumed to correspond to Carnap's similarity: no exact match, but not too much dissimilarity.
(3) *Simplicity* is not mentioned. Is it self-evident for Hempel that philosophers should be as simple as possible, or does Hempel think that simplicity is not a cognitive value for philosophers? Given how he proceeds, we bet on the first answer.
Based on this comparison, it is reasonable to assume that Hempel's idea of explication is the same as Carnap's.

Descriptive and normative claims
Hempel has a clear view on the fruitfulness of philosophical models of explanation. This view is expressed in the following quote:

> As is made clear by our previous discussions, these models are not meant to describe how working scientists actually formulate their explanatory accounts. Their purpose is rather to indicate in reasonably precise terms the logical structure and the rationale of various ways in which empirical science answers explanation-seeking why-questions. (p. 412)
> The construal here broadly summarized is not, of course, susceptible to strict "proof"; its soundness has to be judged by the light it can shed on the rationale and force of explanatory accounts offered in different branches of empirical science. (p. 425)

Hempel suggests here that his models give us insight in the logical structure of explanation (What do they look like?) and the rationale of explanations (Why do scientists construct them? Why are they valuable?). How this second aspect works can be seen in the following quote:

> Thus a D-N Explanation answers the question '*Why* did the explananandum-phenomenon occur?' by showing that the phenomenon resulted from certain particular circumstances, specified in C_1, C_2,...,C_k, in accordance with the laws L_1, L_2,...,L_r. By pointing this out, the argument shows that, given the particular circumstances and the laws in question, the occurrence of the phenomenon *was to be expected*; and it is in this sense that the explanation enables us to *understand why* the

phenomenon occurred. (p. 337).

In other words: explanations provide understanding and the covering law models – according to Hempel – provide insight into what understanding really is.

In claiming that his model gives insight in the logical structure of explanations and in connecting DN-explanation with expectability and understanding, Hempel puts forward the following descriptive hypothesis:

> All scientists who have understanding as an aim really seek DN-explanations, so that the phenomenon they want to understand becomes expectable.

This hypothesis cannot be formulated without the explicatum. In this way, the explicatum is fruitful. In order to avoid misunderstanding of the descriptive claim, it is important to stress that is *not* a claim about what scientists or laymen call explanations. It is a claim about the intentions of scientists, about the kind of understanding they are after.

In the quote above Hempel not only mentions “rationale” but also “force”. One of the insights we get from his models – in his view – is that non-causal explanations can be as good as causal explanations: they can have the same explanatory force. Therefore, there is no reason to demand that a good scientific explanation should be causal. Hempel states that it is unclear “what reason there would be denying the status of explanation to all accounts invoking occurrences that temporally succeed the event to be explained” (pp. 353-354). This view can be summarised in the following guideline:

> All scientists that are engaged in trying to understand the world should construct DN-explanations (and not necessarily something more specific, such as D-N explanations citing causes).

This guideline cannot be formulated without the concept of DN-explanation. Again, this explicatum is fruitful: we can formulate a guideline that we cannot formulate without it.

Hempel’s failures

Let us now have a look at the problems. A first problem is that Hempel does not provide arguments for his descriptive claim. He does not provide a database with records of relevant opinions of a large representative sample of scientists from all disciplines all over the world (their opinions on what kind of understanding is important). However, that is what he should have done if he wanted to build a convincing argument for his empirical claim. If we compare what he does with what Carnap has in mind with “Piscis” and other scientific explicata, Hempel resembles a scientist who develops a nice explicatum concept, formulates an interesting hypothesis with it and then stops instead of trying to collect empirical evidence. Hempel stops where the real challenge starts: gathering evidence for his hypothesis by systematically investigating the opinions of a large representative sample of scientists (by interviewing them or analysing their writings).

Our criticism of Hempel is similar to the complaints of experimental philosophers about appeals to intuition in analytic philosophy: claims about what layman would say about a particular case are part of the argument for a philosophical theory, but traditional philosophers don't actually ask people what they think. More generally traditional philosophy (which experimental philosophers call armchair philosophy) does not use the standard empirical methods of the behavioural and social sciences while they do make descriptive claims about human attitudes, opinions, behaviour, etc. (see Knobe 2004 for a concise statement of what experimental philosophy is).

The second problem is that Hempel does not give arguments for his normative claim. Why, for instance, are causal explanations not interesting in a special sense (i.e. in a sense different from the general one in which the set of DN-explanations, of which they are a subset, are interesting)? Again, he stops where the real challenge begins.

4. Philip Kitcher's working-method

Kitcher versus Hempel

In his paper 'Explanatory Unification' Philip Kitcher writes:

> Why should we want an account of explanation? Two reasons present themselves. Firstly, we would like to understand and to evaluate the popular claim that the natural sciences do not merely pile up unrelated items of knowledge of more or less practical significance, but that they increase our understanding of the world. A theory of explanation should show us *how* scientific explanation advances our understanding. ... Secondly, an account of explanation ought to enable us to comprehend and to arbitrate disputes in past and present science. Embryonic theories are often defended by appeal to their explanatory power. A theory of explanation should enable us to judge the adequacy of the defense. (1981, p. 508)

The first reason Kitcher mentions is similar to what Hempel calls "rationale". However, Kitcher claims that Hempel has it all wrong: understanding consists in unification, not in expectability. They have a common aim, but Kitcher thinks that Hempel has failed. Kitcher does not mention the concept of explication explicitly, but his model of explanation satisfies Carnap's criteria: precise definition, similarity and fruitfulness (Kitcher formulates descriptive and normative hypotheses with it, see below).

On the descriptive side, Kitcher puts forward two claims. The first is a negative one, a claim against Hempel:

> Scientists who have understanding as an aim often seek something more specific than DN-explanations. So Hempel's descriptive hypothesis is false.

This claim can be found at several places in Kitcher's work. For instance, in commenting on Hempel's covering law model he writes:

> Many derivations which are intuitively nonexplanatory meet the conditions of the model. (1981, p. 508)

The derivations which Kitcher has in mind are the whole bunch of counterexamples which several philosophers have launched against Hempel (cf. our Appendix). In order to understand Kitcher's critique of Hempel properly, it is important to remind that they are not interested in what scientists and other people call explanations. They are interested in what understanding is. According to Kitcher, if you would present a scientist with the two derivations of the flagpole example (see Appendix) then this scientist would say that the derivation of the length of the shadow from the height of the flagpole provides understanding, while the other derivation does not. So the first derivation would be judged as scientifically more interesting. Kitcher is convinced that scientists would judge similarly in other cases (e.g. the pendulum) and concludes that expectability is not the kind of understanding scientists are after. According to Hempel, scientists would judge that both derivations are equally interesting (the derivation of the height from the flagpole provides as much understanding as the other derivation). Similarly for the other cases such as the pendulum. Hempel concludes that expectability is the kind of understanding that scientists are after.

The problem with Kitcher's line of reasoning is the same as with Hempel's: he speculates about what scientists would answer when presented with a set of derivations and a corresponding question. He did not actually interview a representative sample of scientists. The result is that the dispute cannot be settled: both Hempel and Kitcher claim that if one would consult scientists, that would result in evidence supporting their hypotheses. They cannot both be right.

Kitcher's positive descriptive claim

Kitcher also puts forward a positive descriptive claim:

> All scientists who have understanding as an aim really seek K-explanations, so that the phenomena they want to understand become more unified.

"K-explanation" stands for "Kitcher-explanation".[2] An underlying idea of Kitcher is that, while all explanations are arguments, the converse is not true. He uses argument patterns to distinguish explanations from non-explanatory arguments. For an individual with knowledge K, an argument A can only be an explanation if it is acceptable relative to K (i.e. if the premises of A are members of K). But not all acceptable arguments are explanations: an acceptable argument is an explanation if and only if it instantiates an argument pattern that belongs to a privileged set of argument patterns. This set of argument patterns is privileged because it has a higher unifying power with respect to K than any other conceivable set of argument patterns. The unifying power of a set of argument patterns is determined by four factors: (i) it varies directly with the number of accepted sentences (i.e. the number of elements of K) that can be derived by means of acceptable arguments that instantiate a pattern in the set; (ii) it varies conversely with the number of patterns in the set; (iii) it varies directly with the stringency of its members; and (iv) it varies directly with the degree of similarity of its members.

Here is a quote from Kitcher in which the positive descriptive claim is clearly present:

> *Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute)*. (1989, p. 432; italics in original)

In Kitcher's work on explanation no evidence for this second, positive claim can be

---

[2] There is no standard label, comparable to "DN" or "covering law" for Hempel's views to denote explanations in the sense of Kitcher. So we introduce our own label here.

found. He has not surveyed a large sample of scientists of various disciplines, in order to investigate what their views on understanding are. What he does is clarify the meaning of the claim (by developing his account of unification) but – like Hempel – he stops where the real challenge starts: gathering empirical evidence for the descriptive hypothesis.

Kitcher's normative claim
Kitcher also puts forward a normative claim:

> All scientists that are engaged in trying to understand the world should construct K-explanations (nothing more, nothing less).

This is a strong claim, because it excludes contrastive explanations (contrastive explanations are no arguments, so they are "less" than K-explanations) and implies that causal information is useless (causal explanations are "more" than K-explanations). Here is a quote from Kitcher in which he clearly expresses a normative aim:

> The most general problem of scientific explanation is to determine the conditions which *must* be met if science is to be used in answering an explanation-seeking question *Q*. I shall restrict my attention to explanation-seeking why-questions, and I shall attempt to determine the conditions under which an argument whose conclusion is *S can be used* to answer the question "Why is it the case that *S*?" (1981, p. 510; emphasis added)

Kitcher does not argue for this normative claim either. Again, he stops where the real challenge starts.

5. Wesley Salmon's working method

In Chapter 1 of *Scientific Explanation and the Causal Structure of the World*, Wesley Salmon explicitly puts himself in the tradition of explication:

> Many philosophical studies, including the one to which this book is devoted, aim at providing reasonably precise explications of fundamental concepts [.] (1984, p. 4)

Like Hempel and Kitcher, he claims that his explication of the concept of scientific explanation provides insight into what understanding is:

> Our aim is to understand scientific understanding. We secure scientific understanding by providing scientific explanations; thus our main concern will be with the nature of scientific explanation. (p. ix)
> Scientific explanations can be given for such particular occurrences as the appearance of Halley's comet in 1759 or the crash of a DC-10 jet airliner in Chicago in 1979, as well as such general features of the world as the nearly elliptical orbits of planets or the electrical conductivity of copper. The chief aim of this book is to try to discover just *what scientific understanding of this sort consists in*. (p. 3, emphasis added)

His ambitions are quite high. He hopes to have achieved this aim for all sciences except quantum mechanics:

> It is my hope that the causal theory of scientific explanation outlined in this book is reasonably adequate for the characterization of explanation in most scientific contexts – in the physical, biological, and social sciences – as long as we do not become involved in quantum mechanics. (p. 278)

According to Salmon, explaining ...

> ... involves the placing of the explanandum in a causal network consisting of relevant causal interactions that occurred previously and suitable causal processes that connect them to the fact-to-be-explained. (1984, p. 269)

The usual label for explanations of this type is "causal-mechanical explanation" (abbreviated here as CM explanation).

These quotes clearly reveal a descriptive aim, which is captured in the following hypothesis:

> All scientists (except maybe in QM) who have understanding as an aim really seek CM-explanations.

The aim of Salmon's book is to clarify what CM-explanations are and to argue for this claim. Salmon only succeeds in the first task: he defines the crucial concepts, viz. causal interaction and causal process. Arguments for the descriptive claim are missing: there is only the hope to have succeeded (cf. the quote above, which comes from the very end of the book).

Of course we could treat Salmon charitably and claim that he should not have set himself descriptive aims that require large scale empirical research for which philosophers do not have the (financial) resources and often not the methodological skills. He should have stayed where philosophers belong, in the critical and normative realm. In other words, he should have put forward the following claim:

> All scientists (except maybe in QM) who have understanding as an aim should seek CM-explanations (nothing more, nothing less).

Salmon's book does not contain arguments for this claim, and so he does not do better *in this respect* than Hempel and Kitcher. The three philosophers put a lot of effort in clarifying what their (descriptive or normative) claims mean, but the arguments are missing.

6. A pragmatic approach to studying scientific explanations

We think it is the responsibility of current philosophers of explanation to go on where Hempel, Kitcher and Salmon stopped. However, we should go on in a clever way. We call this clever way the "pragmatic approach to scientific explanation." Let us clarify what it consists in and defend it. A pragmatic approach to explanation has three guiding principles, which we discuss below.

Context-dependent normative claims

The first principle is: make context-dependent normative claims and argue for them. This means that you look at how scientists explain in certain disciplines and within certain research traditions in a discipline. Philosophers of explanation then can defend certain explanatory practices (e.g when other scientists in the discipline deny the validity or usefulness of that practice) or criticise explanatory practices. They can also try to provide guidelines for improving an explanatory practice. Examples of this can be found in Van Bouwel & Weber (2008a) and (2008b) and in De Vreese, Weber & Van Bouwel (2010). These papers engage in debates on explanatory practices in the social sciences and in the biomedical sciences. Models of scientific explanation as developed by philosophers of science are used as tools to make context-dependent normative claims.

The fact that we use traditional models of explanation as toolbox is one of the reasons why we call our approach pragmatic (there are more reasons, see below). “Pragmatic” thus refers to an “instrumentalist” attitude towards models of explanation: we see them as tools in a toolbox, rather than as describing the “essence” of explanation or understanding. In doing this, we differ in one important respect from the normative endeavours of Hempel, Kitcher and Salmon: the claims we make are context-dependent, i.e. they relate to specific types of explanation problems within specific disciplines. There are two reasons for this piecemeal approach. First, this approach makes the results accessible for the scientists in the relevant disciplines (which increases the chance that philosophical reflection has an impact on scientific practice). Second, the only way to defend general context-independent normative claims is bottom-up (i.e. by generalising from context-dependent claims, if they turn out to be similar). Third (and related to the second reason), the context-dependent claims are more modest and therefore normally easier to argue for.

Another difference is that we not only put forward normative claims; we also argue for them in our papers (see the papers referred to above).

Context-dependent descriptive claims

The second principle is: try to make context-dependent descriptive claims and argue for them. On the one hand, this means that you try to describe the explanatory practice of scientists in a certain discipline or research tradition. For instance, Walmsley (2008) describes the practice of explanation in dynamical cognitive scientists and argues that they are constructing DN explanations (in Gervais & Weber (2011) it is nonetheless shown that this description is inaccurate). In these descriptions, traditional models of explanation are used as a toolbox (this is a second reason to call the approach pragmatic). The advantages of the piecemeal approach (as compared to the general claims made by Hempel, Kitcher and Salmon) are the same as explained above for the normative claims.

Besides focussing on specific scientific disciplines, there is a second way to formulate context-dependent descriptive claims: one can select a subtype of explanations (e.g. causal explanations of contrasts between facts) and try to get a grip on their structure and the factors that determine their explanatory power. An example of this can be found in Weber, Van Bouwel & Lefevere 2011: in this paper, the role (explanatory value) of unification is investigated for several subtypes of causal explanations of facts. The claims made in this approach are general in two respects (they are universally true for clearly identifiable subclasses of explanations and they are not limited to one scientific discipline) but at the same time they are context-dependent: we do not make claims about the nature of *all* scientific explanations. All claims have a limited area of validity.

Epistemic interests
The third principle is this: take into account the epistemic interests (i.e. the reason scientists have for asking specific explanation-seeking questions) when trying to make context-dependent normative or descriptive claims about explanations. These epistemic interests have to be taken into account because they influence the type of explanation that is appropriate in a given context and also influence which properties of an explanation (e.g. depth, deductivity) are important and which not in the given context. The relation between epistemic interests, types of explanation and the value of properties of explanations is investigated in Weber, Van Bouwel & Vanderbeeken (2005) and Weber & Van Bouwel (2007). The use of epistemic interests constitutes a third reason to call our approach pragmatic: the idea of epistemic interests originates in the work of the American pragmatists (Charles Peirce, John Dewey) and is also clearly present in the work of the German pragmatist Jürgen Habermas (who uses the term "knowledge-constitutive interest", see Habermas 1978).

Pragmatic approach versus pragmatic theory
Our pragmatic *approach to* explanations should not be confused with a pragmatic *theory of* explanation as developed by e.g. Bas van Fraassen (1980). According to a pragmatic (also known as erotetic) theory of explanations, an explanation is an answer to a why-question that uses the appropriate relevance relation. Which relevance relation is appropriate depends on the context. Though this theory is much vaguer than the model of e.g. Hempel, Kitcher and Salmon, it is still a *theory about what an explanation is.* Our proposal – the pragmatic *approach* – is situated at the meta-level: it is a proposal on *how to study* scientific explanation. One of the ingredients of the approach – as outlined above – is that we use models of explanations as instruments. So we stop making claims about the nature of explanations. We don't even make vague claims like in the pragmatic theory.

7. Conclusion

In this paper, we have presented the descriptive and normative claims on scientific explanation that Hempel, Kitcher and Salmon make by means of their explicata. We have identified a major shortcoming which they have in common: they don't gather empirical evidence for their descriptive hypotheses (this could be done by systematically investigating the writings of a large representative sample of scientists) and don't argue for their normative claims either.

Since it is – in our view – about time for philosophers of explanation to improve our ways of studying scientific explanation, we presented our pragmatic approach to studying scientific explanation as a better way to proceed. The main differences of what we propose – compared to Hempel, Kitcher and Salmon – are: context-dependence of the normative and descriptive claims, providing arguments instead of merely putting forward hypotheses, and the role of epistemic interests.

Let us conclude this paper by going back to Carnap's idea of explication. The pragmatic approach we defend still assumes that we develop explicata as a first step in the analysis of explanations: we propose to use them – in the same way as Hempel, Kitcher and Salmon did – to put forward descriptive and normative claims. The reason is that without explicata we have no precise claims. And if the meaning of the claims remains vague, we cannot develop decent arguments. So if we want to develop arguments, we need explicata. However, our approach requires that we give up an *implicit* assumption which Carnap makes. Carnap seems to assume that philosophers of science can get far by developing *one* explicatum for a given explicandum (e.g. probability or explanation). Hempel, Kitcher and Salmon certainly assume this: they are convinced that their explicatum will do all the work (maybe leaving out some "strange" fields like QM, cf. Salmon). Our pragmatic approach rejects this monism and adopts pluralism as a heuristic guideline: in principle, every explicatum that has been developed by a philosopher of explanation in the past can be used to formulate a context-dependent descriptive or normative hypothesis that is then further investigated. In the end – after analysing all explanatory practices of scientists – it may turn out that all explicata except one are superfluous. However, an a priori choice for monism is an overhasty conclusion which is unfounded at this moment and thus – from a methodological point of view -- a bad start for studying scientific explanation.

References

Carnap Rudolf (1950), *Logical Foundation of Probability*. London: Routledge and Keegan Paul.

De Vreese Leen, Weber Erik & Van Bouwel Jeroen (2010), 'Explanatory Pluralism in the Medical Sciences: Theory and Practice', in *Theoretical Medicine & Bioethics* **31**, pp. 371-390.

Gervais Raoul & Weber Erik (2011), 'The Covering Law Model Applied to Dynamical Cognitive Science: a Comment on Joel Walmsley', in *Minds and Machines* **21**, pp 33-39.

Habermas Jürgen (1978), *Knowledge and Human Interests. Second Edition*. London: Heinemann Educational Books. [Translation of *Erkenntnis und Interesse*. Frankfurt: Suhrkamp Verlag, 1968].

Hempel Carl (1965), *Aspects of Scientific Explanation and other Essays in the Philosophy of Science*. New York: Free Press.

Hempel Carl & Oppenheim Paul (1948), 'Studies in the Logic of Explanation', in Hempel (1965), pp. 245-290. [originally in: *Philosophy of Science* **15**)

Kitcher Philip (1981), 'Explanatory Unification', in: *Philosophy of Science* **48**, pp. 507-531.

Kitcher Philip (1989), 'Explanatory Unification and the Causal Structure of the World', in: Kitcher & Salmon (1989), pp. 410-505.

Kitcher Philip & Salmon Wesley (eds.) (1989), *Scientific Explanation*. Minneapolis: University of Minnesota Press.

Knobe Joshua (2004), 'What Is Experimental Philosophy?', in *The Philosophers' Magazine* **28**.

Salmon Wesley (1984), *Scientific Explanation and the Causal Structure of the World*. Princeton, NY: Princeton University Press.

Salmon Wesley (1989), 'Four Decades of Scientific Explanation', in: Kitcher & Salmon (1989), pp. 3-219.

Van Bouwel Jeroen & Weber Erik (2008a), 'De-ontologizing the Debate on Social Explanations: a Pragmatic Approach Based on Epistemic Interests', in *Human Studies* **31**, pp. 423-442.

Van Bouwel Jeroen & Weber Erik (2008b), 'A pragmatic defence of non-relativistic explanatory pluralism in history and social science', in *History and Theory* **47**, pp. 168-182.

van Fraassen Bas (1980), *The Scientific Image*. Oxford: Oxford University Press.

Walmsley Joel (2008), 'Explanation in Dynamical Cognitive Science', in *Minds and Machines* **18**, pp. 331-348.

Weber Erik, Van Bouwel Jeroen & Vanderbeeken Robrecht (2005), 'Forms of Causal Explanation', in *Foundations of Science* **10**, pp. 437-454.

Weber Erik & Van Bouwel Jeroen (2007), 'Assessing the Explanatory Power of Causal Explanations', in Johannes Persson & Petri Ylikoski (eds.), *Rethinking Explanation*. Dordrecht: Kluwer Academic Publishers/Springer, pp. 109-118.

Weber Erik, Van Bouwel Jeroen & Lefevere Merel (2011), 'The Role of Unification in Explanations of Facts', in H. De Regt, S. Okasha & S. Hartmann (eds.) *EPSA Philosophy of Science: Amsterdam 2009*. Springer.

Appendix: Hempel's DN-model and its problems

The DN model

Because Hempel's deductive-nomological model of explanation is very well known, we will use this model as a starting-point. According to Hempel, we have to distinguish between true explanations and explanations that are well-confirmed (see e.g. Hempel 1965, p. 338). Both concepts are defined by means of the auxiliary concept of *potential explanans*, which is characterized as follows (cfr. Hempel & Oppenheim (1948), part 3; readers who are well acquainted with Hempel's DN model will notice that we make some simplifications):

(H)The ordered couple (L,C) constitutes an potential explanans for the singular sentence E if and only if
(1) L is a purely universal sentence and C is a singular sentence,
(2) E is deductively derivable from the conjunction L&C, and
(3) E is not deductively derivable from C alone.

A purely universal sentence consists of one or more universal quantifiers, followed by an expression which contains no quantifiers and no individual constants. (L,C) is a true explanans for E if and only if (L,C) is a potential explanans for E and both L and C are true. (L,C) is a well-confirmed explanans for E if and only if (L,C) is a potential explanans for E and both L and C are well-confirmed.

Let's consider an example (inspired by an example in Hempel & Oppenheim (1948), p. 246). A mercury thermometer is rapidly immersed in hot water. We observe a temporary drop of the mercury column, followed by a swift rise. According to definition (H), the following construction is a *potential* explanans for this phenomenon:

C_1: This thermometer consists of a glass tube which is partly filled with mercury.
C_2: This thermometer was rapidly immersed in hot water.
L: In glass tubes which are partly filled with mercury and are rapidly immersed in hot water, the mercury level first drops and then rises.

If all these claims are true, we have a *true* explanation.

Accidental generalisations

Hempel realised that he needed to distinguish between genuine laws and accidental generalizations. Consider the following two statements, apparently equivalent as far as their logical form goes (see Salmon (1989), p. 15):

(i) No gold sphere has a mass greater than 100,000 kg.
(ii) No enriched uranium sphere has a mass greater than 100,000 kg.

Whereas the second statement seems to be the expression of a lawful fact (since the critical mass for enriched uranium is just a few kilograms), the truth of the first statement seems to be a contingent matter of fact (it just happens to be the case that no one did produce such a sphere as yet).

Hempel's example is this: if we derive that Horace is bald from the premises (i) that Horace is a member of the Greenbury School Board, and (ii) that all members of the Greenbury School Board are bald, this is not an explanation because we have used an accidental generalisation. Hempel's problem is that he has no viable account of how to distinguish between laws and accidental generalisation (he admits this).

Irrelevant premises

Several people have offered counterexamples to Hempel's models. The first group of examples are inspired by the fact that the relation of logical deduction is monotonous: if you add premises to a deductive argument, the result is still a deductive argument. This is not the case for explanations: most people will not regard arguments with superfluous premises (that otherwise satisfy Hempel's conditions) as explanations. Well known examples are:

(1) This sample of tables salt dissolves in water because I have hexed it, and all samples of hexed salt dissolve in water.

The problem is that non-hexed salt also dissolves in water.

(2) John Jones did not get pregnant during the last year because he took birth control pills, and men who take birth control pills don't get pregnant.

Here the premise about the birth control pills is superfluous.

(3) It was almost certain that John Jones would recover from his cold in less than one week, because he took vitamin C and almost all colds disappear within one week after taking vitamin C.

Here the problem is that almost all colds disappear within one week, even without vitamin C.

This problem can be solved by adding a criterion (requirement that no premise is redundant) to Hempel's model.

Asymmetry

Several people have argued that explanation is asymmetrical. Because arguments can be reversed, this asymmetry is a problem for Hempel's models. Consider the following questions and answers:

Question 1

Why does this flagpole have a shadow of 10 metres long?

Answer 1

The flagpole is 10 metres high. The sun is at 45° above the horizon.

Because light moves in a straight line, we can derive (by means of the Pythagorean Theorem) that the flagpole has a shadow of 10 metres long.

Question 2

Why is this flagpole 10 metres long?

Answer 2

The flagpole has a shadow of 10 metres long. The sun is at 45° above the horizon.

Because light moves in a straight line, we can derive (by means of the Pythagorean Theorem) that the flagpole is 10 metres high.

The problem is that only the first argument is an intuitively acceptable explanation, while both answers are DN explanations in Hempel's sense.

Another famous example is the pendulum. The pendulum law ($P=2\pi.\sqrt{L/g}$) describes the relation between the length of a pendulum (L) ans its period (P). Consider the following questions and answers:

Question 1

Why does this pendulum have a period of 2.006 seconds?

Answer 1

The pendulum is 1 metre long. From this it can be derived by means of the pendulum law that its period is 2.006 seconds.

Question 2

Why does this pendulum have a length of 1 metre?

Answer 2

The pendulum has a period of 2.006 seconds. From this it can be derived by means of the pendulum law that it has a length of 1 metre.

Again we have two arguments satisfying Hempel's criteria, but only one intuitively acceptable explanation.

Hempel did not regard asymmetry as a problem: for him these examples show that we have bad intuitions about explanations. Once we realise that understanding equals expectability, we can get rid of the bad intuitions (because we realise that explanations are symmetrical).