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THE POSITIVISTS' APPROACH TO SCIENTIFIC DISCOVERY[1]

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ABSTRACT

In the early eighties, philosophers of science came to the conviction that discovery and creativity form an integral part of scientific rationality. Ever since, the "positivists" (logical positivists and their immediate forerunners) have been criticised for their (alleged) neglect of these topics. It is the aim of this paper to show that the positivists' approach to scientific discovery is not only much richer than is commonly recognized, but that they even defended an important thesis which some of the 'friends of discovery' seem to have forgotten. Contrary to what is generally accepted, I shall also show that there is no reason at all why the positivists should have ignored discovery.

1. Aim and survey

Over the last fifteen years, philosophers of science have paid considerable attention to problem solving, discovery, and creativity. Remarkably, this interest in discovery was accompanied by an attack on the traditional philosophy of science. Nearly every paper or book on the methodology of discovery, written during the last two decades, starts with the contention that traditional philosophers of science viewed discovery as a psychological phenomenon, not amenable to logical analysis, and completely irrelevant for the philosophy of science (see, for instance, Nickles, 1980, pp. 1-2 and elsewhere, Wartofsky, 1980, p. 2, Ruse,

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1980, p. 131, Darden, 1980, p. 151, Achinstein, 1980, p. 117, Monk, 1980, p. 337, Laudan, 1981, Giere, 1988, p. 24, Darden, 1991, p. 9, Shaffner, 1993, p. 8, Kleiner, 1993, p. 1). The term "traditional philosophers of science" is commonly used to refer to the logical positivists and their immediate forerunners, namely Mach, Duhem, and Poincaré. (Henceforth, I shall use "traditional philosophers of science" and "positivists" interchangeably to refer to the logical positivists as well as to Mach, Duhem, and Poincaré, and "early positivists" to refer to the latter three; "logical positivists" will be used as usual.)

At first sight, the attack on the positivists seems entirely fair. Their writings undeniably contain passages in which the generation of novel 'hypotheses' is explicitly considered as an irrational affair. On closer inspection, however, it turns out that precisely those which made the most vehement attack on the methodological study of hypothesis generation, paid attention to the phenomenon themselves. Some of them even designed procedures which amount to 'logics of discovery'.

That positivists paid attention to scientific discovery has been noticed before. But, one has simply concluded that their approach to discovery was somewhat confused (Laudan, 1981, p. 181, Nickles, 1990, p. 158). I shall argue that it was not only much more developed than commonly recognized but also entirely coherent.

The situation is even worse. Present-day philosophers of science seem to believe that the positivists' view on science makes the methodological study of discovery impossible. I shall show that this belief rests upon some serious misconceptions. Contrary to what is commonly acknowledged, there is no reason at all why positivists should have ignored discovery. I shall even show that they defended an important thesis which some of the 'friends of discovery' seem to have forgotten.

I shall proceed as follows. After a brief sketch of the common view on the positivists' approach to scientific discovery (section 2), I shall discuss in what way these philosophers paid attention to the phenomenon (section 3). In sections 4 and 5, I shall clarify their view on discovery and show that it is coherent. The question where and why present-day philosophers misunderstood the positivists' approach will be dealt with in section 6. In the final section, I shall draw some conclusions for the contemporary study of discovery.

2. The common view on the positivists' approach to discovery

It is a commonplace, nowadays, that positivists distinguished the context of discovery (in which new knowledge claims are generated) from the context of justification (in which available knowledge claims are evaluated), and claimed that the former is of no philosophical relevance. They insisted, so we are told, that philosophers of science should restrict their attention to the context of justification.

As is generally agreed upon, this position is well in line with the romantic view on creativity which holds that the discovery of new knowledge claims, unlike their justification, does not involve (analyzable) procedures but merely results from 'intuitive leaps of genius'. According to Achinstein, for instance, positivists viewed the context of discovery as one permeated with inspiration, hunch, and conjecture — phenomena which may have causes deep in the human psyche, but which are not logical inferences of the sort philosophers and logicians can study (1980, p. 177). In a similar vein, Darden contends that traditional philosophers of science treated theories

> as if they arose *all at once* by a creative leap of the imagination of a scientist, a process whose study was viewed as the province of the psychologist. Only after the creative leap, they agreed, were the philosopher's logical tools useful to evaluate the theory so produced. (1980, p. 151, my italics)

Also Nickles claims that, from the positivists' point of view, systematic inquiry becomes possible only *after* a definite theory is available, for only here directives exist (1980, pp. 34-36). Similar ideas are expressed by Wartofsky (1980, p. 7), Monk (1980, p. 337), Giere (1988, p. 24), and Shaffner (1993, p. 8).

There is something more. As is commonly accepted, the positivists' approach to scientific discovery is also related to their conception of methodology — they adopted the hypothetico-deductive method. According to this method, the generation of new knowledge claims is irrelevant for their justification: new knowledge claims are justified by deriving testable consequences from them and by comparing these with observation. It is commonly acknowledged, and most clearly expressed by Nickles (1980, pp. 28-29), that adherents of this method located evaluation of all kinds in the context of justification, pretending that *nothing at all* of a normative or advisory sort can be said about discovery. Nickles contends that in such a conception of science a methodology of discovery is infeasible, for the latter presupposes precisely the possibility of evaluative and normative judgments. Nickles argues:

> [S]o far as these philosophers can determine, all 'methods' (or madnesses) by which people seek to solve problems are equally good [...]. So if you are struggling with a problem, these philosophers *should* (on their view) tell you [...] that as good a way as any to solve it is to doze off before a fire, board one tram after another, start pecking randomly at the typewriter, sit under an apple tree ... . (1980, p. 29)

And there are not only general claims. Several authors refer to Mach for having defended the view that new discoveries simply reveal themselves to the properly prepared mind (Simonton, 1989, Wartofsky, 1980). On this view, Wartofsky contends, discovery remains

> an inscrutable act of grace (like being chosen by God for salvation, or by the World Spirit for an historical role.) In this approach, the scientist is not the creator but rather the passive instrument of a force which lies beyond his or her consciousness. (1980, p. 7)

Also Poincaré is often referred to for his (alleged) denial that discovery processes can be analyzed. Allegedly, Poincaré viewed one of his discoveries in mathematics (concerning Fuchsian functions) as the outcome of a sudden flash of insight: "As I put my foot on the step of the bus, I suddenly realized ..." (see, for instance, Hadamard, 1954). In section 3.3, we shall see that this is a very distorted picture of Poincaré's own account.

3. Positivists did pay attention to scientific discovery

Whether or not positivists maintained that the study of discovery is irrelevant for philosophers of science, one thing is clear: they themselves did not exclude the phenomenon from their domain of professional interest. As a matter of fact, some of their writings contain more on the methodology of discovery than the work of Hanson, who is commonly recognized as one of the forerunners of the friends of discovery. Let me provide some illustrations. (The interested reader can find many more examples in the relevant texts.)

3.1 Mach

The standard account of Mach's approach to discovery is highly misleading. It holds true that Mach insisted on the role unconscious processes play in the discovery of new knowledge. But, it does not hold true that he viewed discovery as completely nonrational.

In fact, Mach paid considerable attention to the phenomenon of scientific problems (1917, pp. 220-231).[3] According to his account, scientific problem solving does not differ qualitatively from ordinary puzzle solving (*vulgäre Rätsellösung*): both are best conceived as *search processes*; the fact that in the former case the domain is usually larger and less well known only makes it harder to find a solution. In line with this, Mach dealt with various problem solving methods which are, according to himself, not only useful for the justification of given results but also for the *generation* of new ones. The *synthetic* and the *analytic* method are viewed by him as examples. The former implies that one tries to derive the solution from what is already known. An application of this method, Mach argued, results in a highly 'constrained' search process: one only takes into account those findings which already satisfy the individual conditions of the problem. If the available knowledge turns out to be inadequate for a successful application of the synthetic method, one may resort to the *analytic method*. Here, one starts from a representation of the solution and reasons 'backward' (wanting to cross a river, one first imagines that a trunk connects both banks — this would solve the problem — and next examines the conditions for this particular solution). According to Mach, solving a problem by means of the analytic method is a quite precarious endeavour: the way in which one starts to work on

[3] On Mach's view, there are two sources of scientific problems: one may find a conflict between a theory and some empirical finding, or one may find a conflict between two (or more) theories. In modern terms, one might think of the distinction between empirical problems and conceptual problems.

the problem may be influenced by accidental circumstances, and it is only gradually that one comes to know the 'correct' conditions of the problem.[4] It is important to note, however, that Mach does not conclude from this that the process is blind or uncontrolled. Although he realizes that the hypothesized solution as well as the hypothesized conditions may have to be modified while working on the problem, he recognizes that they provide heuristic guidance.

In addition to this, Mach discussed at length the use of analogies, which he illustrated with several examples of historical discoveries (1917, pp. 220-231). He seemed especially impressed by Maxwell's combination of analogies and strict mathematical reasoning, which he considered as a close approximation of the ideal method of scientific inquiry. Mach also worked out an interesting study of thought experiments in the sciences (1917, pp. 183-200). The underlying method (which consists in the systematic variation of relevant findings) was conceived by him as a *method of discovery*. One of the examples discussed concerns the discovery of Newton's gravitation theory (pp. 189-191). According to Mach's account, which I merely summarize, Newton may have arrived at the idea of universal gravitation by conducting a thought experiment in which the size of a falling stone is varied to the size of the moon, while at the same time its distance to the earth is increased. According to Mach, such a thought experiment quite naturally suggests that the same force which is operative on the stone, must also be operative on the moon.

3.2 Duhem

Like Mach, Duhem paid attention to the use of analogies which he considered as the

> surest and most fruitful *method* of all the procedures put in play in the construction of physical theories. (1954, p. 96, my italics)

As an example, Duhem discussed the analogy between the propagation of heat and that of electricity which permitted Ohm to transfer the

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[4] This comes very close to what Nickles, much later, will describe in terms of changing constraints (see, Nickles 1980, and especially, his 1981).

equations Fourier had written for the former to the second category of phenomena. Other examples concern the analogy between light phenomena and sound phenomena, and the analogy between magnets and bodies which insulate electricity.

According to Duhem, the use of physical analogy often takes a more precise form.

> Two categories of very distinct and very dissimilar phenomena having been reduced by abstract theories, it may happen that the equations in which one of the theories is formulated are algebraically identical to the equations expressing the other. Then, although these two theories are essentially heterogenous by the nature of the laws which they coordinate, algebra establishes an exact correspondence between them. (1954, p. 96)

In that case, Duhem argues,

> every proposition of one of the theories has its homologue in the other; every problem solved in the first poses and resolves a similar problem in the second. (1954, p. 96)

This sort of algebraic correspondence, is viewed by Duhem as

> an infinitely valuable thing: not only does it bring a notable *intellectual economy* since it permits one to transfer immediately to one of the theories all the algebraic apparatus constructed for the other, but it also constitutes a *method of discovery*. (1954, p. 97, my italics)

It may happen, Duhem contends, that in one of these two domains which are covered by the same algebraic scheme, problems are easily formulated and solved which are not evident at all in the other domain.

It is interesting to note that Duhem rigorously distinguished the use of analogies from the use of (mechanical) models. The latter are viewed by him as unscientific, because they substitute the use of imagination for the use of reason. According to his account, scientists who use models instead of analogies reject the logically conducted understanding of abstract notions in order to replace it with a vision of concrete entities (1954, p. 97). However, notwithstanding his strong disapproval of mechanical models (he assigned their use to a certain weakness of mind, allegedly occurring mainly among the English), he recognizes that the use of models

> has been able to guide certain physicists on the road to discovery and that it is still able to lead to other findings. (1954, p. 99)

In line with this, he insists that the use of mechanical models should not be suppressed. His motivation is quite amusing, but nicely reflects his concern for the most productive organisation of science:

> Strong minds, those that do not need to embody an idea in a concrete image in order to conceive it, cannot reasonably deny to ample but weak minds, which cannot easily conceive of things devoid of shape and colour, the right to sketch and paint the objects of physical theories in their visual imagination. The best means of promoting the development of science is to permit each form of intellect to develop itself by following its own laws and realizing fully its type; that is, to allow strong minds to feed on abstract notions and general principles, and ample minds to consume visible and tangible things. In a word, do not compel the English to think in the French manner, or the French in the English style. (1954, p. 99)

Note that Duhem is not pleading for a form of methodological anarchism. Allowing that different people develop and follow different procedures does not amount to the claim that 'anything goes'. On Duhem's view, discovery methods are chosen on the basis of their success in formulating *interesting* theories.

3.3 Poincaré

Contrary to what is commonly accepted, Poincaré did not reduce his discovery of Fuchsian functions to a single stroke of insight. As Gruber nicely analyzes (1989, and especially 1995), he describes it as consisting of seven episodes, taking place over a period of several months. This should not surprise us. Poincaré attached great importance to the so-called illumination moment, which he viewed as an essential component of creative discoveries. But, he also recognized that every discovery process involves a considerable amount of systematic inquiry.

Like Mach and Duhem then, Poincaré dealt with discovery methods (which he viewed as largely inductive). More especially, Poincaré drew attention to the important role mathematical techniques can play in the generalization of empirical findings. (Empirical generalizations were conceived by him as the hallmark of science.) According to his account, mathematical techniques can be very helpful in the discovery of new knowledge claims, and thus, may considerably contribute to the efficiency of scientific inquiry (see, for instance, 1906, p. 167, and p. 172).

3.4 The logical positivists

Interest in the methodological aspects of discovery is also found among the logical positivists. Reichenbach, for instance, advocated the *straight rule*, a technique for the discovery of natural regularities. And, as Nickles observed, several members of the Vienna Circle contributed to the development of data reduction methods, such as factor analysis, which actually amount to a special kind of 'discovery logics' (Nickles, 1990, p. 158). Even Hempel paid attention to discovery. (Hempel is usually portrayed as one of the strongest opponents of the methodological study of discovery — see, for instance, Curd, 1980, pp. 205-207). More especially, he discussed the method of thought experiment and the method of analogy (1965, pp. 164-165, pp. 433-447). As this aspect of his work has been largely ignored, I shall consider it a bit further.

According to Hempel, thought experiments are aimed at anticipating the outcome of experimental procedures which are just imagined, and may be divided into two categories. First, there are *intuitive thought experiments*. These are characterised by the fact that the assumptions and data which guide the prediction are not made (entirely) explicit — past experience and general principles function as suggestive guides for imaginative anticipation rather than as a theoretical basis for systematic inquiry. Next, there are *theoretical thought experiments*. These presuppose a set of explicitly stated principles (laws of nature, for instance) and anticipate the experimental outcome by *deductive or probabilistic inference* from those principles together with suitable boundary conditions representing the relevant aspects of the imagined experimental situation.

Hempel insisted that theoretical thought experiments are characterised by rigorous *deduction* from available theoretical principles.

> Imagination does not enter here; the experiment is imaginary only in the sense that the situation it refers to is not actually realized and may indeed be technically incapable of realization. (1965, p. 165)

Hempel illustrated the nature of theoretical thought experiments with the following example. Confronted with the question what would happen if the thread of a pendulum were infinitely thin and perfectly rigid, and if the mass of the pendulum were concentrated in the free end point of the thread one may proceed in two ways. One may try to 'think away' the aspects of a physical pendulum which are at variance with the assumption and thus to envisage the outcome, or one may derive the outcome from available principles. In the latter case, but not in the former, one engages in a theoretical thought experiment.

According to Hempel, the two types are rarely realized in their pure form:

> in many cases, the empirical assumptions and the *reasoning* underlying an imaginary experiment are made highly, but not fully, explicit. Galileo's dialogues contain excellent examples of this procedure, which show how fruitful the *method* can be in suggesting general theoretical insights. (1965, p. 165, my italics)

Two remarks are in order here. First, Hempel clearly recognized the systematic and methodical character of thought experiments. As should be clear from the quotations, Hempel acknowledged that one may *reason* to the result of a thought experiment, even if not all the assumptions are made fully explicit. Next, Hempel considered the use of thought experiments as a *method of discovery*. According to his account, thought experiments play an important heuristic role in suggesting new hypotheses.

Now, what about Hempel's approach to the use of analogies? Central to Hempel's treatment of analogies is the notion of *syntactical isomorphism*. Two sets of laws, governing different sets of phenomena, are considered as syntactically isomorphic if they have the same syntactical structure (the empirical terms occurring in the first set of laws can be matched, one by one, with those of the second set; replacing each term in a law of one set by its counterpart results in a law of the other set). A physical analogy then is viewed as a *nomic isomorphism* — a syntactic isomorphism between two corresponding sets of laws. It is important to note that according to Hempel all references to analogies can be dispensed with in the systematic statement of a finished theory. Nevertheless, he emphasizes that the discovery of an isomorphism between different sets of laws or theoretical principles may prove useful in other respects. More especially, he stresses that well-chosen analogies play an important heuristic role in the context of discovery.

All this accords well with Duhem's account of analogies. There is, however, one important difference. As I mentioned above, Duhem severely criticised the use of mechanical models. Hempel, however, treats mechanical models in much the same way as structural analogies — he even provides an analysis of some historical discoveries which were arrived at by means of a mechanical model. From this analysis, he concludes that

> the mechanical models scorned by Duhem exhibit nomic isomorphisms of basically the same kind as those scientific analogies in Duhem's sense which are not specifically formulated in the parlance of models. (p. 438)

Hempel adds to this that Duhem's distinction between models and analogies does not reflect a difference in logical status, but rather a difference in the precision and scope of the isomorphic sets of laws — the number of laws that can be carried over in the case of a mechanical model is quite small compared to the number that can be transferred in the case of a structural analogy between two theories.

4. Toward a better understanding of the early positivists' approach to discovery

In the following sections, I shall discuss some theses which, taken together, may significantly contribute to our understanding of the early positivists' view on discovery. Parallels with the view of the logical positivists will be dealt with in section 5.

4.1 Scientific discovery consists in the generation of (unified sets of) quantitative hypotheses

In order to understand the early positivists' approach to discovery, it is important that we first consider their specific view on the objects of scientific inquiry. According to the early positivists, the sciences aim at the production of knowledge claims that are couched in precise mathematical terms and that organise a set of phenomena in an economic way. On their view, knowledge claims are mere tools to organise and predict phenomena — they are not supposed to offer an explanation.

Note especially that early positivists view scientific knowledge as free from interpretations. According to their account, it consists of abstract representations of the facts, or rather, of *quantitative* expressions representing certain *relations* between them. These 'facts' are conceived by them as direct, uninterpreted sense data. Moreover, they believe that terms and expressions have pure verificationistic meanings (which may refer to techniques of measurement but which are themselves free of interpretative elements) and that everything can be expressed in an exact language — hence their conviction that not only the 'facts' but also the quantitative representations of relations between them are free from interpretations.

In line with this, early positivists accept that scientific theories can be divided into two entirely separate 'parts': a representative part (a unified set of quantitative expressions) and an interpretative or explanatory part. The former, early positivists maintain, is all that matters from an epistemological point of view; this part alone determines the scientific value of a theory. The interpretative part, on the other hand, is considered as superfluous. Duhem is most explicit: interpretations do not contribute to the body of scientific knowledge but are mere expressions of a (metaphysical) desire to get hold of reality.

> Everything good in the theory, by virtue of which it appears as a natural classification and confers on it the power to anticipate on experience, is found in the representative part; all that was discovered by the physicist while he forgot about the search for explanation. On the other hand, whatever is false in the theory and contradicted by the facts is found above all in the explanatory part; the physicist has brought error into it, led by his desire to take hold of realities. (Duhem, 1954, p. 32)

In view of this, early positivists maintained that the representative part is much more stable than the explanatory part, and moreover, that the same set of quantitative expressions may be combined, quite arbitrarily, with a variety of interpretations.

> When the progress of experimental physics goes counter to a theory and compels it to be modified or transformed, the purely representative part enters nearly whole into the new theory, bringing to it the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another explanation. (Duhem, 1954, p. 32)

Similar ideas are expressed by Mach (1896a), who argued that the entire history of thermodynamics is compatible not only with the contemporary view on the nature of heat but also with the view that heat is a substance (*calorique*), and also by Poincaré who maintained that an eventual rejection of the (then current) ether hypothesis would not have the slightest influence on the laws and equations of optics (1906, p. 246).

In a sense — but as we shall soon see, only in a sense — early positivists viewed the interpretative elements of a theory as reprehensible: 'real' knowledge is composed of pure sense data and should be kept as free as possible from interpretations. The many rational reconstructions they made of episodes of the history of science, have to be understood against this background. These reconstructions were meant to free the body of scientific knowledge of all interpretations. For example, in his reconstruction of the history of mechanics (1933), Mach attempted to eliminate all those concepts (among which the concept of absolute space) which cannot be supported in a direct manner by empirical findings.

In view of all this, it should be clear in what way early positivists use the term "scientific discovery". Within their conception of science, the term refers to the generation of (unified sets of) quantitative hypotheses;[5] it never refers to the formation of interpretations.

[5] Roughly speaking, early positivists use the term "hypothesis" to refer to a tentative idea which is suggested as a possible way to understand the facts but which is not yet empirically tested (see, for instance, Mach, 1917, p. 235).

4.2 New knowledge claims cannot be derived directly from the facts

This is central to comprehend the early positivists' approach to scientific discovery. According to their account, *any* procedure (algorithmic or not) to generate new knowledge claims (new quantitative hypotheses), presupposes the availability of an antecedent, less specific 'guiding hypothesis', which is not obtainable by the same procedure. This entails that there can be no procedure which would suffice, *by itself*, to derive new quantitative hypotheses from the phenomena.

According to Poincaré, for instance, mathematical techniques can be used to generate new quantitative hypotheses from the data. On his view, however, this generalization process necessarily presupposes the availability of (what he calls) 'necessary hypotheses': general principles which are used in making judgments of relevance. A similar view is defended by the others. All of them agree that one needs preliminary ideas of possible relations between the facts (henceforth, "elementary hypotheses"), *before* one is able to collect the relevant data and to construct a general formula for them. Note, however, that elementary hypotheses are viewed by early positivists as *means* to obtain quantitative hypotheses. They are not considered themselves as parts of (finished) theories.

4.3 Interpretations play an indispensable part in many discovery processes

The early positivists' approach to science exhibits a remarkable characteristic (which has been largely overlooked in this connection). Notwithstanding their extremely severe standards for scientific knowledge, they were far more tolerant with respect to the domain of scientific *inquiry*. As far as the latter is concerned, they recognize that hypotheses which provide an *interpretation* of the phenomena (henceforth, "explanatory hypotheses") play a crucial role. Mach (1917, p. 270), for instance, discusses the development of theories of light in the seventeenth and eighteenth century, and insists that their formulation was highly dependent on the explanatory hypothesis adopted: Newton, Hooke, and Huygens arrived at different results because they were guided by different interpretations of light.

Also Poincaré recognized the heuristic importance of interpretations as is apparent from his defence of 'indifferent hypotheses' — hypotheses that cannot be verified. (Poincaré considers the hypothesis that matter has atomic structure as an example.) Because of their resistance against verification, Poincaré argues, indifferent hypotheses do not belong to the body of scientific knowledge. He stresses, however, that this kind of hypotheses should *not* be excluded from the domain of scientific *inquiry*, for they have an important *heuristic* value: they are not only useful devices for calculation and pictorial aids to understanding, they also *guide* the process of discovery (1906, pp. 180-181).

Remarkably, Mach and Poincaré went even a step further. Not only did they admit that explanatory hypotheses may provide heuristic guidance, they even advocated the belief that, unless the discipline has reached a sufficient degree of maturity, explanatory hypotheses form a *necessary* condition for the generation of new knowledge claims. Thus, in a reaction to Hillebrand, Mach (1917, p. 247) defends the view that explanatory hypotheses played an indispensable part in the discovery of Newton's gravitation theory. Also Mach's reaction against Mill's methods should be seen in this light: not allowing that explanatory hypotheses enter the analysis, Mill's methods make it impossible to arrive at truly novel discoveries (1917, p. 240, for instance).

In line with this, early positivists explicitly rejected the idea that explanatory hypotheses should be banned from the domain of scientific inquiry. According to Mach (1917, p. 248), a developing science is full of interpretations. It is only when a science is nearing completion, that a more direct representation of the phenomena becomes possible.

4.4 Every discovery process involves elements which are not the outcome of reasoned inquiry

This idea, which is central to the early positivists' approach to discovery, has to be understood in the light of the following (see also section 4.2 and 4.3): (i) early positivists realized that the generation of new knowledge claims presupposes the availability of guiding hypotheses (explanatory hypotheses and/or elementary hypotheses), and (ii) they did not consider this type of hypothesis as the result of reasoned inquiry. On their view, the way in which new guiding hypotheses are generated is not amenable to logical analysis.

According to Poincaré, for instance, novel 'ideas' originate as *random* combinations of unconscious elements (during the so-called period of incubation). This is why he attaches so great an importance to so-called 'aha-experiences' (see, for instance, 1912, pp. 43-63). A similar view is defended by Mach who devotes two essays to the role fantasy plays in the acquisition of new knowledge (1917, pp. 88-108, pp. 144-164). According to his account, the discovery of novel ideas requires that the 'representative elements in the mind', which are formed and interconnected through experience, combine *randomly* and *unconsciously* in ways not (yet) given by sensorial experience. Also the point of view of Duhem is illuminating. According to him,

> the physicist does not choose the hypothesis on which he will base a theory [...] any more than a flower chooses the grain of pollen which will fertilize it; the flower contents with keeping its corolla wide open to the breeze or the insect carrying the generative dust of the fruit; in like manner, the physicist is limited to opening his thought through attention and reflection to the idea which is to take seed in him without him. (1954, p. 256)

So, early positivists did not consider the generation of novel guiding hypotheses as a methodical matter. Note, however, that they did not view it as an 'inscrutable act of grace' either. The idea that novel hypotheses result from some mysterious 'mechanism' like 'divine inspiration' or 'intuition' is completely foreign to them. From their point of view, the generation of novel ideas is a natural phenomenon that can be explained without any recourse to obscure capacities. Mach (1896b), for instance, explicitly rejects the idea that creative scientists have some special faculty for generating correct hypotheses. On his view, creative scientists distinguish themselves by their extremely rich experience. As a result of this experience, Mach argues, every representative element in their mind is connected with a bunch of others; hence, they easily arrive, by means of associative processes, at a great number of interesting hypotheses some of which may turn out to be correct.

4.5 Most (if not all) discovery processes involve (an amount of) systematic inquiry

Contrary to what is commonly acknowledged, early positivists explicitly rejected the idea that scientific discoveries (merely) result from some flash of insight. Mach (1917, p. 161), for instance, admits that new perspectives on a problem may open in a sudden and unexpected way. He emphasises, however, that sudden insights *always* occur in the context of a long and hard search process. Also Duhem dissociates himself from the romantic 'blow-of-insight-view' on discovery which, according to his account, occurs mainly among nonprofessionals:

> The ordinary layman judges the birth of physical theories as the child the appearance of the chick. He believes that this fairy whom he calls by the name of science has touched with his magic wand the forehead of a man of genius and that the theory immediately appeared alive and complete, like Pallas Athena emerging fully armed from the forehead of Zeus. He thinks it was enough for Newton to see an apple fall in an orchard in order that the effects of falling bodies, the motions of the earth, the moon, and the planets and their satellites, the trips of the comets, the ebb and flow of the ocean, should all suddenly come to be summarized and classified in that one proposition: Any two bodies attract each other proportionally to the product of their masses and inversely to the square of their mutual distance. (1954, p. 221-222)

Those who have a deeper insight into the history of science, Duhem argues, realize that

> no physical theory has ever been created out of whole cloth. The formation of any physical theory has always proceeded by *a series of retouchings* which from almost formless first sketches have *gradually* led the system to more finished states. (1954, p. 221, my italics)

As these examples indicate, early positivists accepted that the generation of new knowledge claims involves systematic inquiry. They simply did not believe that (interesting) theories appear out of the blue. Neither did they believe that they can be generated by some kind of random process. In line with this, early positivists considered it possible to analyze specific discovery processes and to make comprehensible the steps involved in them. They even believed that it is possible to describe (more or less general) methods of discovery — see section 3 for some evidence.

Someone might object that early positivists viewed the generation of new knowledge claims as a psychological phenomenon. And, indeed, they insisted on the necessity of guiding hypotheses, and viewed the generation of the latter as a nonrational phenomenon. But this does not entail that discovery processes were conceived by them as devoid of any methodical dimension. In their view, guiding hypotheses provide a *frame* within which a systematic search process can take place. That the generation of this frame itself cannot be viewed as a methodical matter, is not to the point here.

Note especially that there is no conflict with the previous section. The view that novel 'ideas' originate from random processes is perfectly compatible with the view that discovery involves systematic inquiry. All passages which suggest that early positivists regard discovery as a methodical matter, are dealing with the generation of knowledge claims. On the other hand, whenever they claim that the generation of novel hypotheses is nonrational, they are considering the generation of *guiding* hypotheses. Mach's and Poincaré's random combinations of 'representative elements in the mind', are not supposed to result in full-fledged scientific theories but in new guiding hypotheses (such as the idea that there is a relation between the pressure, volume and temperature of a gas, that heat is a substance, that light is a wave, ...) Also, when Duhem claims that scientists should 'keep their minds open', he is not dealing with the generation of new quantitative hypotheses (remember his view that the generation of new scientific theories is a gradual process), but with the 'generation' of guiding hypotheses on which new scientific theories may be built.

It is remarkable that early positivists did not consider the absence of procedures for the generation of new guiding hypotheses as a drawback for the efficiency of scientific inquiry. On their view, the generation of guiding hypotheses, although far from a methodical matter, is quite unproblematic. Mach, for instance, describes how the hypothesis that heat is a substance (which, according to his own account, played an important role in the history of early thermodynamics) originated in a completely natural way (1917, p. 241). Where it comes to, so it seems, is that scientists have a sufficiently rich experience. In that case, they will arrive with great ease at fruitful guiding hypotheses.[6]

Remember also that early positivists viewed interpretations as largely *arbitrary* (see section 4.1). Also this helps to explain why the generation of new guiding hypotheses was considered by them as a rather straightforward process. They took the line that, for any domain of inquiry, there is a huge amount of interpretations which may prove useful for the discovery of new knowledge claims. Thus, almost any idea an experienced scientist may come up with may prove good enough to arrive at interesting results.

4.6 Discovery procedures provide neither reasons for acceptance nor for rejection

As far as discovery is concerned, early positivists seem to consider all methods as 'equally good'. Duhem is most explicit:

> [t]here is no doctrine so foolish that it may not some day be able to give birth to a new and happy idea. (1954, p. 98)

But also Mach (1917) stresses that precisely the *same* generation processes may lead to knowledge of the phenomena as well as to error with respect to them, and that it is *only* by testing consequences that the former may be distinguished from the latter. In other words, it is impossible to differentiate beforehand (before the attempted solution is put to the test), between successful problem solving methods and unsuccessful ones.

These examples seem well in line with the view discussed in section 2, for they strongly suggest that early positivists considered the context of discovery as completely nonevaluative. One should take care, however, not to jump to conclusions. What do these examples show? They reveal that, for early positivists, the way in which new knowledge claims are generated does neither provide reasons for their acceptance nor for their rejection. The mere fact that a knowledge claim was arrived at

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[6] Feyerabend (1987, pp. 136-138) correctly observes that Mach's insistence on the 'instinctive' elements of a scientist's world view (general principles which were formed through a long process of adaptation) allows him to explain discovery without making an appeal to some mysterious faculty.

by the careful application of accepted rules does, from their point of view, not entail that it will be accepted. Where it comes to is that the knowledge claim 'saves the phenomena'; whether or not this is the case can only be decided by prolonged empirical tests. Conversely, the fact that a knowledge claim was generated by a foolish procedure does, according to the early positivists, not necessitate its rejection. According to their account, it is possible (at least in principle) that a knowledge claim which was arrived at by a completely blind process passes the empirical tests better than one which was generated by an accepted 'method of discovery'. A simple analogy may help to clarify all this.

If your radio is broken and you want to fix it yourself, you may resort to different procedures: one possibility is to consult a manual and to follow the instructions for repairing defects, another one is to open the radio and to tinker with its components, still another one is to throw the radio against the wall. None of the three procedures guarantees that, in the end, the radio works. On the other hand, each of them *may* lead to success.

This nicely illustrates the early positivists' position. A scientific problem is conceived by them as a demand to find a quantitative hypothesis that organises a set of phenomena. Given such a problem, and given some guiding hypothesis, they recognize different procedures: one may use curve fitting procedures, look for analogies with established results, or consequently depart from these. From the early positivists' point of view, none of these procedures provides good reasons for acceptance nor for rejection: prior to testing, one is neither justified in believing that the employed procedure was successful (whatever it was, it may have been the wrong procedure), nor in believing that it was unsuccessful (whatever it was, one may have been lucky).

4.7 Some discovery procedures are more efficient than others

As should be clear already from section 3, early positivists acknowledged that discovery procedures can be evaluated with respect to their *efficiency* in generating new knowledge claims. Remember that the method of analogy was conceived by Duhem as the *surest* and *most fruitful* method of discovery — a view endorsed by the others.

There is something more. As I mentioned in section 4.3, early positivists were quite tolerant with respect to the context of scientific inquiry: within this context the exclusion of explanatory beliefs was regarded as undesirable (notwithstanding the fact that these beliefs were conceived by them as unscientific). Why is that so? I see only one explanation. Early positivists recognized that 'nonscientific' beliefs may provide heuristic guidance in domains were there is a lack of accepted knowledge, and thus may significantly contribute to the efficiency of problem solving. Put in other words: they realized that discovery processes which are *guided* by some kind of beliefs (however unscientifical these might be) are more efficient than completely blind processes.

Is this compatible with the claims discussed in the previous section? It certainly is. Even if one accepts that discovery procedures do not provide *reasons* to accept or reject the result, it still makes sense to believe that some discovery procedures are more likely to lead to success than others. Think again of the radio example. As I mentioned already, any radio fixing procedure may lead to success as well as to failure. Still, trying to understand what the defect may be (and calling in a competent other in the event of failure) is more likely to be successful than simply throwing the radio against the wall.

5. What about the logical positivists?

It cannot be denied that the logical positivists were primarily interested in the logical analysis of (finished) scientific theories and that they paid considerably less attention to scientific discovery. Still, as I mentioned in section 3.4, several of them described or even designed *methods* of discovery.

It is my claim that the logical positivists' approach to scientific discovery can be understood in much the same way as that of the early positivists. Like the latter, logical positivists recognized that the construction of new scientific theories involves all kinds of 'metaphysical' beliefs for which they did not see a generation procedure. Hence their conviction that the construction of new scientific theories, although it may involve a considerable amount of methodical search, will not ever be reduced to a purely 'logical' matter. Hempel, for instance, admits that in situations of a special (and relatively simple) kind, *mechanical* procedures (for instance, curve fitting procedures) can be specified for 'inferring' new (quantitative) hypotheses from data. He stresses, however, that these mechanical procedures can only do part of the job: the choice of associated data necessarily presupposes some *guiding hypothesis* (for instance, that the length of a copper rod is a function of its temperature alone) (1966, p. 14). Hempel also admits that the solution of more complex problems may require all kinds of interpretations. As an example, he refers to Kepler whose study of planetary motion was inspired by some mystical doctrine about numbers and a passion to demonstrate the music of the spheres (1966, p. 16). All this nicely explains why Hempel, but also the others, claim that *every* discovery process involves an amount of 'creative imagination'. On their view, every scientific problem presupposes the availability of appropriate guiding hypotheses, and precisely these are usually not obtained through a systematic search process.

There is something more. Like their forerunners, the logical positivists strongly opposed the idea that discovery procedures may provide reasons for acceptance. On their account, the acceptance of new knowledge claims is entirely dependent on the (post hoc) comparison with empirical findings. This is why they sometimes give the impression that it is impossible to distinguish 'good' discovery procedures from 'bad' ones. But, as I mentioned earlier, this is perfectly compatible with the idea that some discovery procedures are more efficient than others.

Someone might object that the question is not whether logical positivists recognized that discovery procedures can be evaluated with respect to their efficiency, but whether they considered this type of evaluation to be of philosophical concern. My reply to this objection can be brief. I did not find any indication that logical positivists considered the study of problem solving efficiency as unrespectable for philosophers of science. Of course, there is the notorious distinction between the context of discovery and the context of justification and Reichenbach's claim that the former is of no concern for the philosopher of science. But, as Curd (1980) and Nickles (1980) have convincingly argued, Reichenbach never interpreted this distinction in such a way as to exclude the (normative) study of discovery from the domain of the philosophy of science. The 'context of justification' was taken broadly enough by him to cover the normative and evaluative aspects of discovery.

6. Where and why present-day philosophers of science misunderstood the positivists

6.1 The attack on the possibility of a 'logic of discovery'

Why do present-day philosophers have such a truncated view on the positivists' approach to discovery? One of the main reasons seems to be that they misinterpreted their attack on the possibility of a logic of discovery.

Positivists strongly opposed the idea of a 'logic of discovery' (inductive or otherwise). They maintained that we do not have such a logic, and, moreover, that this is not ever to be expected. In order to make their point against adherents of the old inductive method, positivists strongly insisted that *every* search process in the sciences presupposes the availability of a (preliminary) 'hypothesis', and hence, that there can be no (neutral) logic which would take us from a set of data to a scientific theory.

Present-day philosophers seem to have missed the point. From the positivists' claim that scientific inquiry cannot begin without a (preliminary) hypothesis, and that the generation of this hypothesis may require 'great ingenuity', one has concluded that positivists viewed the generation of scientific *theories* as completely nonrational. Remember, for instance, that on Nickles's view, positivists believed that there can only be methodological directives for deriving testable consequences from a *finished* theory. Remember also that according to Darden's account, traditional philosophers of science viewed scientific theories as if they arise *all at once* by a creative leap of the imagination of an individual scientist.

All such claims are mistaken. Traditional philosophers of science never claimed that scientific theories suddenly arise in a fully developed form ready for testing. Contrary to what Darden and others seem to believe, this idea was even explicitly rejected by them — see, for instance, the quotations in section 4.5. Neither did they maintain that scientists need a finished, full-fledged theory before they can start reasoning. They asserted that scientists, before they can engage in a search process, need in any case a preliminary idea of possible relations between the phenomena, and in some cases even an interpretation of the domain they are approaching. This does not amount to the claim that all systematic inquiry falls in the context of *testing* finished scientific *theories*. (Remember that neither preliminary ideas nor interpretations were considered as 'scientific theories' — interpretations were even considered as highly suspect.)

It is my claim that present-day philosophers of science failed to make a distinction which is crucial to understand the positivists' approach to discovery: (i) hypotheses which enable and guide inquiry, but which themselves are not the outcome of a reasoning process, and (ii) hypotheses which form the object of scientific inquiry. The fact that, from the positivists' point of view, the former are needed to obtain the latter explains why they deny the possibility of a logic of discovery. But, as should be clear by now, it does not entail that they viewed theory generation as completely nonrational.

6.2 The defence of the hypothetico-deductive method

There seems to be a second reason why present-day philosophers of science failed to appreciate the positivists' approach to discovery: they misunderstood their defence of the hypothetico-deductive method.

As I mentioned already, positivists strongly defended the idea that the way in which new knowledge claims are discovered does not contribute to their justification. From their point of view, hypotheses which are not yet tested are mere 'guesses' — no matter how they were arrived at, it is quite possible that they do not pass the empirical tests.

Again, present-day philosophers of science seem to have missed the point. From the positivists' claim that (untested) hypotheses are 'guesses', they concluded that positivists viewed the generation of new knowledge claims as completely random. But this makes no sense. When positivists consider an untested hypothesis as a mere guess, they are referring to its unjustified character, *not* to the way in which it was arrived at.

There is something more. Seemingly dazzled by the positivists' repeated claim that the way in which new knowledge claims are arrived at is irrelevant for their justification, present-day philosophers concluded that the study of discovery was conceived by them as insignificant from a philosophical point of view. This conclusion was further promoted by the fact that present-day philosophers of science failed to distinguish between different types of evaluation. Thus, the (correct) finding that positivists considered problem solving procedures to be 'equally good' (in a particular sense), led some to believe erroneously that the 'context of discovery' was considered as completely nonevaluative.

But, as I have argued in section 4.7, there is no reason at all why an adherent of the hypothetico-deductive method should be blind for the fact that some problem solving procedures are more efficient (more likely to lead to a desired result) than others. Nor is there any reason why he or she should refrain from giving advice in problem solving matters. To use an example of Nickles (1980, p. 29), adherents of the hypothetico-deductive method have no difficulty in acknowledging that tinkering with electrical circuit models is a better (more efficient) way of tackling a problem in acoustics than pecking randomly at the typewriter. And, contrary to what Nickles as well as others seem to believe, there is no reason why they should.

7. Are we ahead of traditional philosophers of science?

In many respects, present-day philosophy of science did go beyond the positivists' approach to discovery. Especially under the impulse of Thomas Nickles, we arrived at a better understanding of scientific problems, and of the relation between discovery and justification (see, especially, Nickles 1981 and 1985). In addition to this, we have results concerning heuristics positivists could not even dream of — as an example one might think of the heuristics implemented in the program BACON (Langley et al., 1987).

There is, however, a clear message in the positivists' approach to science that some of the 'friends of discovery' seem to have forgotten: discovery processes are highly dependent upon interpretations.

That interpretations play a role in many discovery processes, is typically ignored by those who are trying to make computer simulations of important scientific discoveries. People like Simon still believe that concrete discovery processes in the sciences consist in the mere manipulation of abstract formulas. Qin and Simon (1990), for instance, explicitly deny that explanatory hypotheses played a part in Kepler's discovery of the third law — this law constitutes one of BACON's famous 'rediscoveries'. According to their account (pp. 305-307), Kepler discovered his law, in a data-driven way, and only *afterwards* added an interpretation to it — for convincing evidence that this does not hold true, see, for instance, Hallyn (1993), Field (1988), Gingerich (1993), Kozhamthadam (1994).

There can be no doubt about it that the results of Simon and his followers are impressive and extremely important for the study of heuristics. But, contrary to what these authors seem to believe, I can find nothing in their work that would not have been applauded by the positivists. Except for the underlying claim that programs like BACON inform us about the original discoveries. For all their shortcomings, positivists would at least have recognized that the reconstructions offered by BACON and similar programs do not shed light on the way in which creative scientists arrive at new theories. They realized, better than anyone else, that Kepler's idiosyncratic interpretation of the universe formed an integral part of his discoveries. They were right for that.

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