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The topic of discovery and the concept of nature

The topic of discovery dominates the imagination of scientists in their working lives as well as that of students of science in their studies; as N. R. Hanson notes, 'discovery is what science is all about'.[1] It would appear that the primacy of discovery is derived from the ambitions of the field from which it arises. Science, that peculiar culture which is the hallmark of Western civilization,[2] makes the discovery or uncovering of nature its central focus. This conception of scientific activity as the dispelling of the unknown presumes a distinctive mystery or intrigue in the notion of nature which pervades the work of science. For example, Galileo's conception of the *mathesis universalis*, the 'underlying' relationships which govern nature, overshadowed the world as it was known directly to the senses, and challenged the common sense world of the Ptolemaists.[3] Man in Protagorean terms was no longer the 'measure of existence'. In the *Theatatus*, Socrates had spoken of Protagorus' theorum that 'man is the measure of all things: alike of the being of things that are, and the not-being of things that are not'.[4] Socrates spoke of the Protagorean concept of truth as the 'aisthesis', the directly knowable world – we have translated this as 'sense perception'. Centuries later, Galileo again raised the spectre of 'sense perception'; he claimed that the senses were 'subjective', and hence were untrustworthy and unreliable. The true field of nature was something out of the ordinary, and consequently the scientists explored a quasi-physical or metaphysical realm to determine the 'structure' or 'form' of the laws which underlay the directly perceived world. Common sense was no longer valid, and traditional thought about the world became the subject of studied scepticism.

Similarly, Descartes' theological efforts to provide a solid basis for scientific knowledge in his *Discourse on Method* were far from our demystified modern materialism. In Descartes' epochal dualism, the guarantee of the correspondence between what was impossible to doubt 'internally' and what actually existed 'externally' in nature was provided by God.[5] The godhead, that mystery of mysteries, simultaneously became the foundation for the self on the one hand, and for science on the other. One finds an uncanny reverberation of this thinking in Newton's picture of space as 'God's mind'. All these images lent a mysterious aura to nature and reified the process of discovery. This direction in early scientific culture had certain discrete consequences. The world as it was known in traditional and common sense ways became distrusted. Science directed its attention to a quasi-physical 'nature' or underlying order of things which had a characteristic intrigue associated with it. And because of the mystery associated with nature, the procedure of its becoming known came to exhibit a dramatic social significance. Consequently we find a curious feature in accounts of scientific discoveries; they are recurrently characterized as being bizarre achievements made by eccentric personalities under curious circumstances, often having horrible consequences. For example, in the traditional myth, Daedalus escapes his captivity by affixing feathers to his arms with wax, but falls to his death as the wax melts when he approaches too close to the sun. Midas faces an equally unhappy fate when his power to change things to gold makes life impossible. A whole series of dystopian novels like Aldous Huxley's *Brave New World*, Zamiatin's *We*, and Samuel Butler's *Erewhon* depict in various ways the ironic turns for the worse that follow the advance of science.[6]

As for the bizarre depictions of the process itself, the iconic image of discovery is provided in classical accounts of Archimedes who, naked and distracted, is said to have run from the gymnasium baths proclaiming 'eureka' after having discovered the laws of hydrostatic displacement. Presumably this was not what Hans Reichenbach had in mind when he spoke of justification as a method of 'presenting [a discovery] before a public'.[7] The history of science is filled with accounts similar to that of Archimedes. Alfred Russell Wallace, recovering from fever during his research in the dense jungles of the Malay Archipelago, reported that he was struck by the idea of speciation *in a delirium* and proceeded to write it out in a single sitting.[8] Einstein reported to Michael Polanyi that the idea of general relativity became vivid to him as a result of a youthful dream in which he tried to follow a beam of light.[9] Kekulé relates that the hexagonal structure of benzene molecules became apparent to him whilst staring half asleep into his fireplace; imagining the flames were snakes, he saw one bite its own tail, forming the hexagonal ring – the very form he was looking for.[10] And then there is the familiar tale of Newton and the apple.

So in the popular images of science, the kernel of scientific thinking is often shrouded in a shell of mystery and/or irrationality, in a dream, or in a fit of distraction, or in the eccentricity of a historical personality.

These conceptions of science are vividly confirmed in the literary images of Dr Faustus, Dr Frankenstein and Dr Jekyll. The scientist and the act of discovery are repeatedly represented as sources of intrigue and mystery, associated with the bizarre and the irrational. It is my conjecture that this characterization of discovery may derive from the shift of attention from the scholastic 'world' of the middle ages to the unknown 'nature' of the Renaissance, from the world of common sense knowledge and traditional belief, to the mathematical nature of existence. This shift is nicely reflected in the change of attitude regarding the formal representation of the world. In Cusanus, the arithmetical models of nature are referred to as 'De Conjectura' – conjectures. With Galileo, the shoe is on the other foot: the real world is the 'mathesis universalis', and the world of everyday life is elusive and 'conjectural'.[11]

REICHENBACH'S DISTINCTION: DISCOVERY VERSUS JUSTIFICATION

The philosophical study of science in the twentieth century appears to have avoided the popular images of scientific discovery and the ostensively irrational aspects of it under a directive from the 'positivist' movement. Hans Reichenbach suggested that the *actual thought processes* and historical conditions whereby a new law or a new mathematical demonstration is arrived at are different from the *rational reconstructions* which occur when the scientist or mathematician communicates the new theory to others. This supposition has become elevated to the status of a *doctrine* of the separation of the context of discovery from the context of justification. However, having made such a principled distinction, the philosophers have generally assumed that *only* justification could be amenable to logical analysis; hence the context of discovery had no status as a philosophical problem. Thus Karl Popper, in a book whose title was mistranslated as *The Logic of Scientific Discovery*, legislated the problem out of philosophy by relegating the matter to 'empirical psychology':

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man – whether it is a musical theme, a dramatic conflict, or a scientific theory – may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.
>
> There is no such thing as a logical method of having new ideas, or a logical reconstruction of this process.[12]

Popper's entire work, which was originally published in 1934 as *Logik der Forschung* (i.e. Logic of Research), is concerned with the formalization and falsifiability of hypotheses. Popper's view re-articulates the position originally formulated by Hans Reichenbach in 1930, in *Erkenntnis*, the journal for the movement of the unity of science. Though Reichenbach reiterates the distinction in several of his other works which are available in English, the original article, to my knowledge, has never been translated. The grip which Reichenbach's doctrine has had on modern authors is due to the influence of Popper. However, it is by no means limited just to his writing. Richard Braithwaite in his *Scientific Explanation* reiterates this opinion: 'The solution of these historical problems involves the individual psychology of thinking and the sociology of thought. None of these questions are our business here.'[13]

Hence the philosophy of science was seen to concern itself entirely with the function of objective arguments in the 'justification of change in science',[14] that is, with the construction of precise hypotheses which could be presented in favour of an idea, the observations which these hypotheses illuminate, and the results of the attempts to falsify them.

Specifically, for Karl Popper the topic of the historical processes of discovery was displaced by the conception of scientific theorizing as the formulation of conjectures about nature. Hence the model of *Conjectures and Refutations* was not necessarily a description of how scientific thinking was actually done, but was a normative idealization, or an injunction about how it ought to be conducted. R. G. A. Dolby touches on this point:[15]

> Nineteenth-century writings on scientific method claimed to be describing the process which successful scientists actually used, and which all scientists *ought* to use. There was no divergence between history and philosophy of science. But with the rise of Logical Empiricism, the descriptive claims of the nineteenth century seemed to be abandoned. With philosophers like R. Carnap, no attempt was made to reflect the activities of practising scientists. The explicit motive of the methodologist was to set out an ideal that scientists should *aspire* to follow . . . It is not difficult to demonstrate the separation between the logic of scientific method . . . and the actual scientific procedures revealed by historical study.

This movement in the study of science had two prominent consequences. First of all, though writers like Popper and Reichenbach may have had good reasons for separating the actual behaviour which resulted in the discovery of new laws from the subsequent presentation and/or demonstration of their validity, the confinement of attention to justification had an unhappy consequence: it inadvertently contributed to the popular notion that the act of discovery was some mysterious process. As Richard Blackwell later noted, Popper's action obscured the problem by laying it to rest in psychology, a 'convenient dustbin' for philosophical problems. One imagines that Reichenbach and Popper reeled back from the image of Archimedes soaking wet and distracted, and directed their attention to the reasons which demonstrate the hydrostatic laws. Only later did philosophers give serious attention to a theory of the actual *in situ* behaviour.

The second major consequence was that the Reichenbach distinction created a rift between the normative picture of scientific theory construction which was offered by logical empiricists on the basis of finished theories, and the descriptions of scientific practice by historians and behavioural scientists based on studies of how research had actually been conducted. An illustration of this rift is offered by the reaction to Thomas Kuhn's accounts of the way in which historical changes in science had been brought about. Many students of the 'reconstruction' school were quite shocked by Kuhn's model of world-outlook shifts, for they appeared to portray the adoption of new theories as a type of 'mob psychology'. One writer characterized Kuhn's work as a 'Frankenstein, which no amount of reformulation can call back'.[16] Though I believe these reactions to Kuhn are indefensible, they underline one of the great costs paid by the Reichenbach distinction: it produced a tension between scientific *practice* as a topic and finished scientific *theories* as a topic. Furthermore, it appears to have created the *impression* that an account of the 'logic' of scientific discovery, i.e. a demonstration of a theory's validity, was simultaneously an account of scientific discovery, i.e. *how* the idea had occurred to an individual, so that for many people, Popper's *Logic of Scientific Discovery* was read as an account both of the process of discovery and of the validation of theories.

Not only behavioural scientists but their objects – the natural and physical scientists – were aware of the rift between the actual practices of inquiry and the normative idealizations of the philosophers. For example, in reply to Reichenbach's logical axiomatization of relativity theory, Einstein responded that 'he did not find it convincing even on its own grounds'.[17] Reichenbach further attempted to reconstruct Einstein's actual inferences as though they were the result of a 'radical empiricism in a field which had always been regarded as a reservation for the discoveries of pure reason';[18] in other words, Reichenbach appeared to equate the actual process of inference with his formalized reconstruction. This too met with Einstein's objections.

Holton notes, in reply to Reichenbach's essay: 'Einstein devoted most of his attention to a denial of this claim.'[19] Clearly, the representation of the logic and/or rational reconstruction of relativity theory created a picture which was unfamiliar even to its originator. However, given Reichenbach's doctrine, this infidelity was not unjustified; strictly speaking, philosophy is only interested in the completed theory and the grounds of its validity, not in its fidelity to the historical process or its familiarity to the originator.

There were two reactions to these consequences of the Reichenbach distinction. The first took the form of an attempt to describe various 'logics of discovery' based on the *in situ* reasoning of scientists in their actual research. These logics constitute theories of discovery, that is, accounts of the processes which result in discoveries. This book is directed largely to an evaluation of such efforts. However, there has been a second reaction: many authors have rejected the relevance and validity of the distinction. As we shall see, these latter efforts are not theories of how discoveries occur, but theories of how discoveries do *not* occur. We shall deal briefly with this latter reaction first.

REICHENBACH'S DISTINCTION AND THE HISTORICAL RECORD

Logics of discovery are not intended to undermine the distinction between the context of discovery and justification but to supplement what is known of the latter with a description of the logic of the former. Such efforts, from the point of view of philosophy, reclaim ground that was hastily abandoned by the early positive philosophers. The work of Feyerabend, Holton and Kuhn, on the contrary, challenges the integrity of the distinction itself.

The best illustration of this position is the work of Paul K. Feyerabend. Feyerabend's analysis of Galileo's theories indicates that Galileo brought about allegiance to the Copernican system through the deceptive use of new natural interpretations of motion which, unknown to his Aristotelian opponents, concealed a highly abstract observational language. This was subsequently 'justified' by Galileo's self-serving interpretations of telescopic images which, though contradicted by what could be seen with the naked eye, supported the Copernican view. Consequently, *his justification and his discoveries could hardly be said to be independent.* His justification also had other dimensions: Feyerabend argues that

> Galileo prevails because of his style and his clever techniques of persuasion, because he writes in Italian rather than Latin, and because he appeals to peo-

ple who are temperamentally opposed to the old ideas and the standards of learning connected with them.[20]

It is clear that allegiance to the new ideas will . . . be brought about by means other than arguments. It will . . . be brought about *by irrational means* such as propaganda, emotion, *ad hoc* hypotheses, and appeal to prejudices of all kinds. We need these 'irrational means' in order to uphold what is nothing but a blind faith,[21] . . . an unfinished and absurd hypothesis . . .[22]

Far from deploring this state of affairs, Feyerabend recommends it.

What our historical examples seem to show is this: there are situations when our most liberal judgments . . . would have eliminated an idea or a point of view which we regard today as essential for science . . . The ideas survived and they can *now* be said to be in agreement with reason. They survived because prejudice, passion, conceit, errors, sheer pigheadedness, in short all the errors which characterize the context of discovery, *opposed* the dictates of reason . . . *Copernicanism and other 'rational' views exist today only because reason was overruled at some time in their past* . . . Hence it is advisable to let one's inclinations go against reason in any circumstances, for science may profit from it.[23]

Feyerabend concludes that

The results contained so far suggest abolishing the distinction between a context of discovery and a context of justification and disregarding the related distinction between observational terms and theoretical terms. Neither distinction plays an important role in scientific practice. Attempts to enforce them would have disastrous consequences.[24]

A determined application of the methods of criticism and proof which are said to belong to the context of justification, would wipe out science as we know it – and would never have permitted it to arise.[25]

Reichenbach's distinction is unfounded, according to Feyerabend, because real conceptual advances in science transform the very criteria of justification, i.e. observations and proof. For example, Galileo's belief in the reliability of the telescope was co-emergent with its 'confirmation' of his Copernicanism. Ludovico Geymonat writes, 'In Galileo's mind, faith in the reliability of the telescope and recognition of its importance . . . were two aspects of the same process.'[26] This problem which has been called the theory-loaded character[27] of data also informs Kuhn's rejection of the Reichenbach doctrine. Notes Kuhn, 'My attempts to apply [these distinctions] even *grosso modo* to the actual situations in which knowledge is gathered, accepted and assimilated have made them seem extraordinarily problematic.'[28] They have been problematic because the radically new theories transform observational terms and objects simultaneously with their theoretical counterparts! In other words, justification and discovery occur simultaneously.

Gerald Holton's view is quite similar. Holton argues that *in addition* to the logical and empirical aspects of explanations there is a third element: the themata.[29] Themata are pre-theoretical suppositions about nature: for example, that it is mathematically harmonious, that it is composed of fundamental units, or atoms, that it is mechanically integrated like a clock, that natural forms are symmetrical, inherently aesthetic, etc. Holton notes that the major consideration for Einstein's famous paper was not the work of Lorentz or Michelson, but the aesthetically disturbing *asymmetry* of Maxwell's equations. Einstein's alternative account had an inner consistency or symmetry which Maxwell's did not. It was this thematum which recommended the theory, in spite of the fact that the first response to the paper to appear in the scientific community was 'a categorical experimental disproof of the theory'. Nevertheless, and before the confirming experimental data appeared, physicists endorsed the theory because of its *thematic* element. Thus Wilhelm Wien wrote: 'What speaks for it most of all . . . is the inner-consistency which makes it possible to lay a foundation having no self-contradictions, one that applies to the totality of the physical appearances . . .'[30] Nor was Einstein prepared to be discouraged by later empirical disconfirmation. Eddington's observations in 1919 clearly confirmed Einstein's predictions about the deflection of starlight by solar gravity. However, Einstein suggested that even if the results had been negative, 'then I would have been sorry for the dear Lord – the theory is correct'.[31]

This only reinforces the position articulated by Feyerabend: that certain of the greatest historical discoveries were not in accord with Reichenbach's distinction, nor could they be, given the theory-ladenness of observations, nor *should* they have been, had that been possible. For example, if Einstein had followed Reichenbach's protocol, he would have recanted his views in 1905 following the experimental disproof, and would never have tolerated the post hoc justification of relativity by Michelson's unrelated experiments of 1887. Indeed, Einstein himself did not become aware of the ether experiments until *after* 1905. Apparently, compliance with canons of logical empiricism would have retarded the whole revolution of twentieth-century physics which Einstein initiated. The same applies to Galileo, according to Feyerabend; a strict experimentalist would have stayed with a Ptolemaic cosmos.

These accounts of scientific discoveries do not constitute a theory of how discoveries occur. They suggest on the contrary how they have *not* occurred: by following the method of conjecture and refutation, and carefully discriminating between elements which suggest a hypothesis and those which justify it. What is recommended by Feyerabend *et al.* is a new epistemological position regarding research and discovery: Feyerabend's conclusion is simply and literally 'anything goes'.[32] Holton's work has a somewhat related intention: 'to prompt the educator to re-examine conventional concepts of education in science',[33] by paying full heed to those thematic aspects of explanations in science which have guided important discoveries, but which have been obliterated by the inaccurate reconstructions found in the science textbooks. In other words, Holton and Feyerabend's conclusions are *prescriptive;* on the basis of how science has actually operated, they advise how texts should be written and how research should or, more correctly, should *not* be conducted. Though of interest to the discussion of Reichenbach's doctrine, that conclusion is not of immediate importance in this work, nor are any epistemological studies which argue which theory of knowledge the scientist should adopt. These are more clearly the subject matter for the philosophy of science. Our task is different: we are concerned with models of how discoveries have actually occurred. Consequently, our domain is behavioural, not philosophical. Our major concern with discovery is: how has it occurred, and can we describe such occurrences with an adequate theory? This is the second notable reaction to Reichenbach's distinction.

The logic of discovery and the action of scientists

Students of science have frequently investigated the context of discovery under the rubric of 'the logic' of discovery. Presumably this provides a pleasing symmetry with 'the logic' of justification. However, these terms refer to two qualitatively different realms. The logic of justification refers to the definitional coherence of, and relations between, specific variables in a constructed model. These are logical and empirical matters par excellence. However, when we speak of 'the logic' of discovery, this is hyperbolous. Only when an account of such action has been shown valid will we be able to speak unambiguously of the logic of *the theory* of discovery (i.e. 'the logic of discovery'), meaning the formal and/or empirical reliability and validity of the theory of this type of action. Consequently, this usage is ambiguous. The topic it refers to is the research activities associated with the production of discoveries, and the chief question it pursues is what are the conditions which produce or control the occurrence of these discoveries.

When we approach the problem in this way, we are doing a number of things. We are making the topic an examinable or 'researchable' phenomenon that begins with a mindfulness of the mystical notions found in folklore. Also, we are separating the problem of the theory of discovery from the question of whether the *in situ* logic, whatever it is, is *different* from its retrospective reconstruction; that was Reichenbach's phenomenon. Consequently, our interests lie neither in the question of how it *ought* to occur nor in the question of how it does *not* occur. How it *ought* to occur is a matter for normative-minded methodologists; how it does *not* occur has been a matter for a group disputing the claims of the former. There has been a certain futility in this latter debate. When we consider that the methodologist is recommending an idealized prescription for research outcomes, the claims by others that discoveries have not in fact occurred in this way is, strictly speaking, addressing a different set of concerns. The anti-Reichenbachian position that recommends 'anything goes' can only be seen to be answering the methodologist's prescriptive plans if the latter can be heard to confuse his *pre*scriptions of how science should be done (based on hindsight) with *de*scriptions of how it has actually happened. We have seen that Reichenbach does indeed confuse these issues when he represents Einstein's actual reasoning in terms of his own reconstructed axiomatization of relativity. Having recognized this confusion, we see in our own minds that some of Reichenbach's critics are not simply postulating an alternative model of how scientists have made discoveries – they are recommending an alternative epistemology or methodology. That is, by raising the question of how discoveries *have not* been discovered, they are not constructing a model of discovery, but are opening up an *alternative* model of how research *ought* to be done. This, however, is not our question. How things 'ought' to be done is a separate topic from how things have been done.

Having clarified the problem this way, we see how this work is capable of responding to the two consequences of Reichenbach's doctrine outlined earlier. The problem is no longer 'discarded' by relegating it to psychology; indeed we shall spend a good deal of time investigating psychological contributions to the question. Furthermore, having clarified the nature of Reichenbach's distinction, we see that there is no overlap between the philosophical problem of explicating the formal and empirical adequacy of reconstructed truth claims about nature, and the behavioural science problem of explaining the conditions under which certain social phenomena, namely discoveries, occur. These are problems for separate domains.