SCIENTIFIC DISCOVERY: A PHILOSOPHICAL SURVEY

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1. The Revival of the Study of Discovery

It is only in recent years, after many years of neglect, that the philosophers of science have rediscovered the phenomenon of scientific discovery. Logical empiricism, the dominant school of twentieth-century philosophy of science, regarded the study of the process of discovery as an empirical inquiry to be dealt with by such scientific disciplines as psychology or sociology. The only respectable engagement of the philosopher of science was considered to be the logical analysis of the products of scientific discovery. However, formal logic proved to be a not very effective tool for dealing with the phenomenon of science, and the discipline known as the philosophy of science became almost irrelevant for the understanding of the peculiarities and the significance of science. It is only in the last two decades or so, with the decline of logical empiricism and the emergence of new approaches to the study of science, that the study of scientific discovery has regained its respectability.

This survey is not intended to be exhaustive. I will mention four of the main trends in the study of discovery which are currently discussed in the literature. I will describe in some detail only representative works of each approach. The first approach is a continuation of the traditional logicist movement in a revised form, which treats discovery as inference. The second concentrates on case studies. The proponents of this approach moved from one extreme to another: from the search for a universal method to "particularism". They tell us about methods and strategies of discovery which they extract from specific domains and particular contexts. The third approach, which is very trendy, is mechanized discovery. Like the second approach, it deals with domain-specific processes. The fourth approach, which is the most radical, treats discovery as a natural phenomenon – cognitive, social or evolutionary.

For the purpose of this survey it will be helpful to use the distinction which I propose to make between two main concepts of discovery: discovery by *exposure* and discovery by *generation* (1993). In science, the meaning of the word discovery has long transcended its original etymological origin which refers to exposure. Newton's theoriy of universal gravitation, and quantum mechanics and Darwin's theory of natural selection, for example, were not literally disficovered or un-covered; they were generated. Discovery by exposure may be guided by reason or method. Creativity is required for generational discovery processes. The more creative the process or the act of discovery, the weaker the method for generating it.

Discovery is a multi-dimensional phenomenon which might be comprehended through a combination of all the above-mentioned approaches; the first might contribute to the analysis of discovery by exposure, the fourth will enhance our understanding of creative processes, the third will serve as an aid for dealing with both processes and the second will provide us with information about the human and historical dimensions of discovery.

2. *Discovery as Inference*

Discovery may be a straightforward inference. Deductive inference is ths act of exposing information hidden in a set of premises. In an example given by Elie Zahar (1983), it is shown how it is possible to deduce Newton's inverse square law of gravity from Kepler's third law of planetary motion.

Discovery of new ideas, concepts or theories is a generational process. The attempts to convert ampliative or generational processes into deductive inference stem from the exposure view of discovery. When generational discovery, such as theory construction, is viewed as deductive inference, it becomes a discovery by exposure. Some attempts to represent generational processes of discovery as method-governed deduction or exposure can be categorized as "postmortem" procedures. Typical examples, such as Peirce's and Hanson's logic of abduction or retroduction, Musgrave's "inventive arguments" and Simon's discovery machine, reconstruct the discovery process from the vantage point of one who benefits from the knowledge of the final result. They can help us, therefore, only in justifying or reproducing something which has already been generated. Another postmortem procedure is the logic of pursuit, which deals with the plausibility of a hypothesis and with the question of whether or not a hypothesis is worth pursuing. This logic of discovery is a method of initial evaluation of the product of discovery, rather than a method of generating the product. However, evaluation is an integral part of the process of discovery, so that the logic of pursuit should not be dismissed as a partial method of discovery.

At this point I shall describe in some detail Musgrave's logic of discovery as a recent representative of the postmortem inferential procedures.

Musgrave's Inventive Arguments

Alan Musgrave (1988) maintains that there is a logic of discovery and that it is no less than deductive logic. He represents a variety of ampliative arguments as deductive arguments. His method is to find the suppressed content-specific assumptions in the discovery arguments expounded by scientists. Indeed, there are always common presuppositions and beliefs shared by the members of the relevant community which do not, therefore, need to be explicitly stated in scientific discourse. Musgrave adopts this strategy in order to convince us that deductive logic is the logic of scientific discovery, and that it is applied to all kinds of discoveries. Let us follow some of his examples.

The first example is from everyday experience. When we want to put forward a hypothesis about the colour of emeralds, we do not guess blindly and test our guesses one by one. We start with an assumption or a premise such as p1: "all emeralds have some common colour". This assumption is not world-embracing; it is, rather, domain specific, reflecting our experience in our everyday environment. In this manner, Musgrave avoids the problem of finding or inventing the natural kinds in the domain; he considers a situation where the natural kinds are already available. The premise p1 can be reduced to a deductive conclusion of the following, more general, domain specific premise p'1: "emeralds belong to a family of kinds of precious stones whose members have a common colour". Of course, there is no justification for p1 or p'1. But the point is that an argument starts with some assumptions and its rationality resides in its validity rather than in the justification of the premises. Another premise, p2, which is drawn from observation, says that some particular emeralds are green. We, thus, have the following argument:

p1: All emeralds have some common colour.
p2: A particular emerald is green.
Therefore, C: All emeralds are green.

Thus, the "inductive" argument, whose premises are statements referring to observed green emeralds and whose conclusion is C, is, in fact, a deductive argument with a suppressed premise p1, for example.

The conclusion C does not constitute a novelty with respect to the premises p1 and p2. Consequently, C is certain relative to the premises. The uncertain element, however, did not disappear; it was shunted on to the premise p1. Thus, we have here a method of discovery based on deductive logic. This method presupposes that the discoverer starts with some working hypotheses which are plausible and established in his mind.

Not much novelty is generated by the above inference, since the novelty-generating premise p1 or p'1 is a very common kind of assumption which has proved successful in ordinary experience. Creativity would be needed if we were to turn to an unfamiliar environment, occupied with unfamiliar objects and phenomena. Creativity is needed in order to find what natural kinds exist, on which inductive generalizations, such as p1, can be made. No wonder that inductive inference, as in the above example, which presupposes a stable set of natural kinds, can be "dressed up" as deductive inference. This is a process of discovery by exposure. We have p1 in mind and then a discovery of a single green emerald amounts to the discovery of the generalization that "all emeralds are green".

Musgrave's second example is the generation of a hypothesis about the relationship between two measurable quantities, L and M. We make the general hypothesis that the relationship is a linear one. We then make two pairs of measurements and find out the exact relationship. The deductive argument might, for example, be as follows:

q1: L and M are lineary related, i.e., $L=a M+b$ for some a and b.
q2: When $M=0$ then $L=3$.
q3: When $M=1$ then $L=5$.
Therefore, $L=2M+3$.

The product of discovery is a specific relationship. Again, this is not a novelty-generating discovery, since the major working hypothesis is included in the premises (perhaps as a tacit assumption).

Encouraged by his success in "dressing up" inductive arguments as deductive ones in cases where no novelty is generated and no creativity is needed, Musgrave turns to another example which is a typical generational discovery. The example is Ernest Rutherford's discovery of the structure of the atom. Unlike the first two examples, this discovery resulted in a great novelty. The whole process is encapsulated by Musgrave in the following argument:

A1: The same (similar) effects have the same (similar) causes.
A2: Atoms and the solar system behave in the same "dense and diffuse" way with respect to bodies entering them [i.e., most bodies entering them pass straight through them, but a few collide violently with them].
A3: The solar system's "dense and diffuse" behavior is explained by its structure, a relatively small but massive body orbited by much lighter bodies.
Therefore C: Atoms are structurally similar to the solar system...

Here, Musgrave "dresses up" an argument by an analogy which is usually considered to be an inductive argument, using it as a deductive argument. This is a typical example of how a process of discovery can be reconstructed without giving us a clue as to the method which might have led the discoverer to his discovery prior to his actually making the discovery. A2 is the crucial premise in the so-called "inventive" argument above. However, it is a premise which can be stated only *after* the main step has been taken in the process of arriving at the hypothesis. The fact from which the discoverer started out, and which he wanted to explain, was the "dense and diffuse" behavior of atoms with respect to bodies entering them. So whence sprung the idea about the solar system into the argument?

Everything is similar to everything else in some respect. The problem is to find a fruitful similarity. Finding the fruitful similarity between the atomic structure and the structure of the solar system was the creative step in Rutherford's discovery. The reconstrucutor already knew this. Had we not already known about Rutherford's discovery, we would not know how to reproduce it here, since we are not presented with any method to directing us how to arrive at this particular model. In all probability, there is no method for arriving at such a creative association. It took Rutherford more than a year to free himself from his entrenched belief that the atom's structure is something like a billiard ball, and to hit upon the idea.

Musgrave's logic of discovery is applied to a marginal step in the process of discovery. The general method here is to convert the discovery to deductive inference, where the premises contain certain hypotheses which bridge the inferential gap. But the creative step in the discovery is the generaiton of these hypotheses. Once these have been discovered, the process does not, in fact, generate any novelty, it merely exposes information implied by the premises.

Dynamic Theory Construction, Research Programmes and Discovery as Correction

James Blachowicz (1987, 1989) offers a logic of discovery which is a "logic of correction". The process of correction involves error-controlled feedback. Correction covers all those cases in which an inference to a new explanans T' is guided by the discrepancy or difference between the observational value (e) and the prediction (p) from an antecedent explanans T. This discovery pattern may be represented by the following formula: CO: (e - p)&T--->T'.

The discovery pattern CO is a special case of heuristic-guided theory generation, where the theory is constructed according to a general model, or a general heuristic principle available in the field.

This discovery pattern may be represented as a "semi-inference"; i.e., an inference in which the inference rules are replaced by the heuristic rules. The premises of such an argument include the new (anomalous) data. However, there may be more than one "conclusion" to the argument. The heuristic rules do not uniquely determine the product of discovery. When we are already equipped with a theory, T, and a new piece of data, e, brings about a new theory, T', which is a result of adjusting T to the new data, then T', the product of discovery, may be regarded as a modified version of T. The process can be symbolized according to the following formula:

RP: e&T--[HEU]-->T'.

The heuristic HEU guides us in modifying the theory, and the product T' is not uniquely determined. This discovery pattern can be viewed as dynamic theory construction, where we start with an initial hypothesis and modify it in order to adjust it to the data. The data-driven process of dynamically constructing a theory is very close to the notion of research programme, or to the notion of dynamic theory. The latter notion refers to a theory which is expanded and modified from time to time in order to adjust it to new observational data. In the process of adjustment and expansion, the theory retains its name and identity and its central claims. Within the Popperian tradition, the Lakatosian notion of research programme refers more or less to the same thing.

In the discovery pattern CO, the heuristic is replaced by the difference between p and e. The latter is supposed to guide the discoverer in correcting his hypothesis. This may be appropriate for describing a very limited class of research programmes which solve a "closed" problem described by a definite set of variables, or curve fitting, such as Kepler's problem. It is not appropriate, however, for describing research programmes such as Bohr's, where the discovery process involves the introduction of new variables or concepts, such as the concept of spin. And these scientific discoveries, which require creativity, are the most interesting.

Discovery as a Skill

There is another kind of treatment of discovery which can be represented as inference: discovery as a skill. Since skill cannot be taught by the provision of a list of instructions, in this case discovery would remain beyond the reach of method. No description or recipe can replace the expert. This is evident from the practice of *expert systems* in AI. Computer scientists try to translate the experience of the expert into a set of machine-oriented instructions. They try to watch or to interrogate the experts in order to draw up sets of heuristic principles which be translated into sets of instructions. However, at the present state of the art, the success of this method is very limited.

Terry Winograd and Fernando Flores (1987) show how Martin Heidegger's analysis may account for the limitation of expert systems. Heidegger distinguishes a domain of action from a domain of description. When bringing tacit knowledge into action, we do so without being aware of the knowledge we employ. In the translation of action into description by an external observer, something is lost. An expert system is a description of the expert's action provided by an external observer. Since the translation is incomplete, the expert system does not function properly in new situations. It is nonfiadaptable to new tasks in contrast to the human expert. Practical skills, such as riding a bicycle or baking a cake, are carried out by human beings almost "automatically", without paying attention to the details; furthermore, when one tries to pay attention to the way a task is corried out, the performance may be disturbed.

A skill involves making the right judgements and performing the proper acts in a given domain of practice. It is acquired by experience. Past experiences are not stored in the memory such that in performing a task one simply recalls them. Bo Goeranzorn and his colleagues studied the nature of human skill and how a skill may be affected by the use of different technologies (Vaux, 1990). The rules an expert follows in performing a task are not expressed by propositions; they are expressed directly in action. This view is in line with Heidegger's observations. It also agrees with Ludwig Wittgenstein's view on tacit knowledge, according to which following a rule does not mean following a set of instructions, it means doing something in a practical way. One acquires a skill through apprenticeship, by imitation and by non-verbal communication.

In the context of scientific discovery we may conclude that the skill and discerning power of the discoverer is restricted to the domain in which he has acquired experience. Thus, a scientist may be a great discoverer in one scientific field but not in another. This may be related to the different material logics employed by different scientific communities. A material logic is, in general, part of the tacit knowledge which governs the reasoning practice and action of a given community. Thus, part of the discerning power of a discoverer in science is drawn from the internalization of the tacit material rules of inference. There may be rules governing a skill, but they can only be correctly applied by an experienced expert. For example, in devising a mathematical theory in physics, one might be guided by a rule of simplicity. But only the experienced theoretical physicist would know how to apply the rule in the construction of a theory, or how to choose the rule or adjust it to new observational data.

The tacit knowledge internalized by the scientist includes the presuppositions and background theories which are taken for granted and shared by the members of the relevant research community. These presuppositions appear as suppressed premises in scientific discourse and argumentation. In this sense, they are "invisible" to the expert; from the expert's vantage point they are "transparent". The expert who employs these presuppositions or suppressed premises does not "see" them; he considers them to be "self-evident", and he is not fully aware of them. This is the reason he has difficulties in explaining to the non-expert what he is doing.

The notions of transparency and invisibility have been used by David Gooding, Trevor Pinch and Simon Schaffer in their book, The Uses of Experiment (1989), mainly in relation to the practice of using observational instruments. In this context, it refers to "the attribute an instrument possesses when it is treated as a reliable transmitter of nature's messages" (ibid., p.3). After the scientist has acquired the skill of using an instrument, the procedure of using it becomes transparent. Gooding, *et al.* employ this notion when they describe the historical development of the practice of using instruments such as the glass prism or the telescope. The concepts of invisiblility and transparency might also refer to the usage of the most advanced experimental equipment, such as the bubble-chamber or counter experimental techniques in particle physics, where a much more intricate practice is involved. The process of establishing the reliability of the instrument is termed "black boxing" by the above authors. When an instrument becomes black-boxed, it is treated as transparent and the information it conveys is treated as the messages of nature. The scientist treats the instrument as if it were an extension of his own organs. Thus, when the particle physicist looks at a photograph of a bubble-chamber he sees particle trajectories. When the instrumentation becomes transparent, "only the phenomena remain" (ibid. p.217) and the process of discovery becomes discovery by exposure, although the black-boxed procedure may be highly generational relative to everyday practice, or relative to the previous state of knowledge. Black-boxing converts discovery by generation into discovery by exposure. Thus, we may say that observation and discovery are skillfiladen (Nickles, 1978, p.300). If we adopt Polanyi's distinction between focal and subsidiary awareness, we may say that the scientist has only a subsidiary awareness of his practice in using the instrument. Only the phenomena remain under his focal awareness. When we use a tool in order to perform a certain task, we are focally aware of the task; we have only subsidiary awareness of the tool (Polanyi, 1958, p.55).

The notion of invisibility can be applied to the use of theoretical tools as well. In constructing his theoretical arguments, the scientist relies on suppressed premises or material rules of inference which are invisible to him in the above sense. For the trained scientist these theoretical tools are transparent; he treats them as if they were part of his cognitive apparatus. In this sense, his theoretical argumentation sometimes looks like deductive inference. Scientific argumentation is contaminated with suppressed premises. This is the reason why, in many typical cases, when the scientist attempts to solve a problem, his choice is limited to only a few hypotheses; he does not have to choose between an unlimited number of logically possible hypotheses. It is the invisible paradigm which narrows the range of possible solutions. The scientist who has internalized the presuppositions of the paradigm takes them for granted; he is not focally aware of them.

3. Case Studies: Particularism

One of the contributions of traditional philosophy of science to the subject of discovery was the distinction between the *context of discovery* and the *context of justification.* This is one of the controversial theses of logical empiricism, which was one of the major reasons for ignoring discovery. It stems from the inference view of discovery. The works on discovery which appeared with the historicist trend provide us with only a fragmentary picture of the phenomenon of discovery. Once the distinction between the context of discovery and the context of justification lost it currency, and the context of discovery had become a legitimate topic in the philosophy of science, some philosophers of science started examining the micro-structure of scientific research and ignored the search for global patterns. The tool of "case study" has been extensively used by the historicist-particularist movement. The proponents of this approach cannot seriously claim that the methods or strategies of discovery they find in one context are valid in different contexts, *a fortiori* in different sciences. This trend is exemplified by one of the books that signify the recent revival of the study of discovery, i.e., *Scientific Discovery, Logic* and *Rationality*, edited by Thomas Nickles (1978), which was followed by a second volume (1980) devoted to case studies. It reflects the historicist trend in the philosophy of science which was popular in the 1970s, following Kuhn and Lakatos, and the attempts to justify the relevance of the history of science to the philosophy of science.

Two of the contributors to the latter volume deal with eminent discoverers, such as Copernicus and Darwin, attempting to arrive at general conclusions. But most of them do not even try to go beyond the specific case studies or the specific fields they are engaged with. Others deal with diverse topics – from research strategies in biological theory to theory change in plate tectonics. Beyond the hope expressed by one of the authors that certain "underlying structures" may emerge from the "quasi-phenomenological" study of particular discoveries, nothing substantial in this direction is offered.

One particular case study which is dealt with by Nickles, as well as other philosophers, is Kepler's discovery of the elliptical orbits of Mars, following Norwood Hanson. Hanson's *Patterns of Discovery* (1958) heralded the recent revival of the study of discovery. His main claim is that the context of discovery, as well as the context of justification, is amenable to logical treatment. He employs Kepler's discovery as a case study for demonstating his claim. He claims that Kepler retroductively inferred his hypothesis from Tycho Brahe's data.

However, his logical analysis has raised criticism among philosophers. In the last decade we find such criticism in an article by Scott Kleiner (1983). Andrew Lugg (1985), on the other hand, tries to defend Hanson's contention that Kepler reasoned his way to the ellipse hypothesis – although the plausibility of the logic of discovery he attributes to Kepler is questionable. This case study is, thus, employed by these outhors to support their general claims about the existence of logic of discovery.

A more recent volume, *Scrutinizing Science* (A. Donovan, L. Laudan, & R. Laudan, 1988), which is devoted to the testing of theories of scientific change, draws extensively upon case studies, some of which are related to scientific discovery. This book systematically explores the relationships between case studies and theories of science including theories of discovery.

In this connection, two more books should be mentioned: *Constructing Quarks* by Andrew Pickering (1984) and *Theory Construction and Selection in Modern Physics: The S Matrix*, by James Cushing (1990). Both books employ episodes from the history of particle physics as case studies which serve as evidence in support of methodological theories and theories of scientific change. Pickering expounds a sociologically-oriented theory of science. The history of particle physics in the 1960s and '70s serves as his grand case study. Cushing says that he uses his grand case study "to examine how theories are constructed, selected and justified in actual scientific practice" (ibid.,xv). Actually, he does not offer his own theory; rather, he compares available methodological principles and theories of science to the lessons which he draws from these case studies. He criticizes Pickering for going too far when he suggests that the conclusion he reaches accounts for *all* of science. The point is, that when the philosopher of science acts in an inductive spirit in constructing his theory of science, he cannot validly generalize from one case study, or a few case studies, to all of science. Only when he proposes a theory of science without relying on inductive justification and subsequently uses his case studies to test it, might he withstand the above criticism.

4. Mechanized Discovery

With the explosion in the field of computer science and artificial intelligence, the movement of "mechanized discovery" has emerged. However, this technology-driven approach does not yield universal methods of discovery either. For the time being, some of the proponents of this enterprise, the cognitive scientists, have succeeded in mechanizing only marginal stages of discovering limited kinds of regularities. Moreover, they claim to have some success in concept formation. A leading book in this direction is *Scientific Discovery: Computional Explorations of the Creative Process* by Pat Langley, Herbert Simon, Gary Bradshaw and Jan Zytkow (1987). Another book which has implications for discovery but is not devoted exclusively to it is *Computational Philosophy of Science*, by Paul Thagard (1988).

Bruce Buchanan is one of the pioneers in using mechanized procedures for discovering regularities and laws. In a typical experiment, a robot arm mixes chemical substances according to some initial hypothesis. Following the results obtained during the night, the hypothesis is changed the following morning according to certain heuristic rules (Buchanan, 1982). This is an example of a recursive procedure. This machine executes a discovery process which can be subsumed under the formula RP, described in section 2 as a semifiinference.

Others, who are engaged in more practical directions, have made contributions to such areas as medical diagnostics and drug research. For example, in a recent publication a we find description of such a product: "a drug discovery software system that enables medicinal chemists to design realistic new molecules interactively; construct, test, and refine hypotheses that explain and predict their bioactivity..." (*Science* 1992, p.1153). This kind of tool may serve as a useful technological aid for conducting research in a specific area, which may have implications for heuristic-guided discovery and for scientific reasoning strategies in general. In the wide spectrum which stretches from the philosophy of discovery to the technology of discovery this kind of treatment is situated far on the technological edge. Yet, in cognitive science one cannot draw a sharp demarcating line between the development of technological tools of discovery such as this and philosophical understanding of discovery; practical achievements in AI technology may have direct implications for philosophical theories of human cognition.

I would like to discuss now in more detail the most serious attempt to mechanize scientific discovery, which purports to reconstruct conceptual discovery as well as discovery of regularities.

Simon's Discovery Machine

Simon and his collaborators (Langley *et al.*,1987) developed a computer program, BACON (see also Simon, 1987), which "discovers" Kepler's third law of planetary motion in the following way: BACON is supplied with data on the periods of revolution (P) and the distances (D) of the planets from the sun, and the program is then applied to the data according to the following *recursive* heuristic rule:

> REC: "If two variables co-vary, introduce their ratio as a new variable; if they vary inversely, introduce their product as a new variable and test it for constancy".

With this rule, BACON first notices that P and D co-vary. It thus computes P/D, which is found not to be invariant. Then REC is applied recursively to the new variables P/D and D, which are found to co-vary. Their ratio P/D^2 is found not to be invariant. Then BACON finds that P/D^2 vary inversely with P/D, so it multiplies them, obtaining P^2D^3, which is found to be constant. The constancy of this variable is indeed an expression of Kepler's third law.

In this example, the discovery machine is doing only part of the job. The first important step is choosing the variables P and D. The choice of the "right" variables sometimes constitutes the main step in the discovery, after which the regularity is immediately exposed. In this particular case the programmer and Kepler alike did not have many alternatives available from which to choose: P and D were inherited from the prevailing scientific tradition of circular planetary motion. As in the case of Blachowicz's logic of correction, this procedure can only apply for closed systems. The situation where a limited set of variables is available and only combinations thereof can be formed is typical of a discovery which does not involve the construction of a new explanatory theory in which novel theoretical terms are introduced. Furthermore, Simon claims that the new variables constructed from the original ones are theoretical terms. However, neither P/D nor P/D^2 can be treated as genuine theoretical terms. The reason for this is that a theoretical term should appear as part of a unifying or an explanatory *theory*. Neither of the variables has any role in any theory; they are formed merely as steps in the computation. They do not refer to any physical phenomenon or to a significant physical magnitude. They do not appear in any law of nature. In the process of developing a theory, many expressions are obtained along the way. We would not call all of these expressions "theoretical terms".

Yet, Simon mentions another variable which is created in the process, and which corresponds to a new theoretical *concept*. By using the word concept, he presumably means that this is a theoretical term having a physical significance. The new concept, which he calls "gravitational mass", is created in the following way. In a given planetary system, the magnitude P^2D^3 has a constant value K. If BACON is applied to different planetary systems, such as the satellites of Neptune, different values of K will be obtained. In this way, the concept of gravitational mass will be discovered, since in Newton's theory of universal gravitation, K is proportional to the gravitational mass of the central body in the planetary system. This seems to be a creative discovery since a new physical magnitude appears to have been discovered here. However, this is only an apparent discovery. If a machine or a playing child who are supplied with two physical magnitudes such as P and D were to form a new combination from them which turns out to play a role in a theory such as Newton's mechanics, it would by no means mean that the child or the machine had discovered the new concept. Had BACON discovered a theory or a law in which gravitational mass plays a significant role, would be possible to say that it had discovered the new concept? The concept of gravitational mass has wider signoficance than merely being related to a certain combination of P and D. BACON plays the role of a "Kepler machine" but not of a "Newton machine". The process carried out by BACON is not one of inference. Indeed, the recursive heuristic rule (REC) programmed in BACON is not equivalent to a rule or a set of rules of deductive or inductive inference. Nevertheless, REC guides a discovery process which is not generational but which is a process of exposure; it exposes a regularity hidden in the data. If a mechanical procedure generates discoveries in a data- driven process, it means that the heuristic rules are good ones. Thus, an important step of the discovery is the discovery of the heuristic rules. In itself, BACON is a product of creative discovery or invention; it is a machine which, when fed the right data, discovers regularities hidden therein. We can make the analogy with an observational instrument. For example, after the telescope was invented, Jupiter's moons were discovered by exposure. The telescope magnifies our sensual capabilities whereas the heuristic-instructed machine amplifies our capability of discovering regularities. A successful heuristic rule is, therefore, an instrument for the discovery of regularities, just as the telescope is an instrument for observational discovery. Thus, in order to make significant discoveries by exposure, we sometimes have to discover first an appropriate exposing instrument.

BACON is, thus, an example of how the machine can magnify our discovery capabilities. The computer is much more efficient than the human discoverer in the case of recursive procedure. The computer also magnifies our computational and data-processing capabilities. These are examples where the computer is an important device for the process of discovery. However, from this we cannot draw the sweeping conclusion that (in all, or even in most, cases) "discovery can be mechanized".

5. Discovery Naturalized

The fourth alternative, which seems to be the most viable in studying the phenomenon of discovery, follows the naturalistic trend in the philosophy of science. When empirical sciences, such as psychology, sociology and biology, are allowed to contribute to the field, philosophers of science can draw upon the results which have been accumulated in these sciences regarding creativity and discovery. Moreover, the philosopher of science can develop new scientific theories of scientific discovery with an interdisciplinary approach. Two of the most notable contributions in this direction, which have implications for discovery, are *Explaining Science* by Ronald Giere

(1988) and *Science as a Process* by David Hull (1988). Giere adopts a cognitive approach to science, while Hull develops an evolutionary theory with sociological perspectives, drawing heavily upon a grand case study from the field of systematics in biology.

Within the naturalistic movement, one of the most promising approaches is Evolutionary Epistemology (EE). (For a selection of recent articles on EE, see Hahlweg & Hooker, 1989. For a comprehensive bibliography of this approach, see Cziko & Campbell, 1990.) Although EE has direct implications for discovery, until recently no significant contributions in this area had been made by its proponents. The reason for this was that the theory of discovery implied by EE seemed to be a non-starter. Scientific discovery is described by EE as "blind variation". This evoked the most serious objections to EE, since discovery in science seems to be intentional (see for example Thagard, 1980). However, in a recent attempt to answer this objection, it was suggested (Kantorovich & Ne'eman, 1989) that the counterpart of blind biological mutation in science be interpreted as serendipitous discovery, which means that scientists proceed in a methodical or guided way, even though their final discovery may solve a problem they had not originally intended to solve. (For a reaction to this approach, see Baggott, 1990.)

Once this major obstacle is removed, the way becomes clear for developing a theory of discovery inspired by EE. This is what I have attempted to do in my book, *Scientific Discovery: Logic and Tinkering* (1993), where I try to integrate evolutionary, social and psychological models of discovery and creativity. I suggest that creative processes in science are governed by two *interrelated,* mechanisms of natural selection: the intrapsychic (subconscious) process of creation, such as the process of *incubation,* and the interpsychic process of *epistemic cooperation.* These processes can be categorized as unintentional or involuntary. The incubation process is well known, but is not well understood. It can be explained by the model of natural selection which is applied to mental elements created quasi-randomly in the discoverer's mind; the discoverer hosts the process in his mind, so to speak. Here I employ the psychological theory of discovery expounded by Dean Keith Simonton in his book: *Scientific Genius: a Psychology of Science* (1988). The second creative process takes place in a socio-historical setting which fosters unintentionality and serendipity; epistemic cooperation generates "blind" variations, or unexpected discoveries. Here, too, the scientist is not fully in command of the process. Another book which deals with the implications of the cooperative and historical nature of the process of discovery is *Multiple Discovery* by Lamb and Easton (1984). (These authors deal also with what they call evolutionary aspects of discovery, although they do not refer to *biological* or *Darwinian* evolution).

The notion of tinkering sheds further light on the above facets of scientific creation. Levi-Strauss (1962) introduced this notion in describing savage thought and Francois Jacob (1977) borrowed it for characterizing evolutionary progress. In my book, I suggest that an inevitable consequence of the natural selection model is that the creative steps in the evolution of science are the products of tinkering. And this implies that scientific creation is not method-governed and that science has no predetermined goal. The notion of tinkering encompasses all kinds of unintentional, serendipitous and opportunistic processes of scientific creation. I try to elevate the phenomenon of tinkering in science from the level of anecdotes and curiosities. I attempt to show that it is part and parcel of the very nature of scientific discovery and human creativity in general. Scientists rarely include descriptions of tinkering in their scientific writings. In popular stories and autobiographies, scientists sometimes do include such descriptions in order to entertain the reader. This reflects the official attitude of traditional philosophy of science towards this phenomenon; cases of tinkering should be hidden from the public, since they contradict the ethos of science. Science should appear as a nice and neat rational enterprise. This is one of the reasons why the nature of scientific creativity is wrapped in a shroud of mystery.

In applying this theme to science as an evolutionary phenomenon, new light is shed on some chapters from the history of science. I offer examples which illustrate this aspect of discovery. To make the case stronger, I bring some evidence from what is considered to be one of the most advanced natural sciences: theoretical physics. The main piece of evidence is drawn from the history of particle physics, which demonstrates the role of tinkering in generating novelty.

Major kinds of generational or creative processes of discovery are involuntary processes: the incubation process, the eureka event and the cooperative-historical process of discovery. What is common to these processes is that the discoverer is not in full command of the process. Traditionally, method-governed discovery has been contrasted with so-called "chance discovery". The notion of "chance" or "accidental" discovery is employed whenever the process of discovery is unintentional. But this does not mean that there is no *explanation* for this kind of discovery. In these cases, the discoverer does not generate the product of discovery. The discoverer can only cultivate and expose it. Cultivation can be guided by recommendations for the discoverer. Following the phrase "chance favours the prepared mind", coined by Louis Pasteur, we may interpret cultivation as preparing the mind for unexpected discovery.

The attempts to find "logic" of discovery should perhaps be restricted to processes of discovery by exposure, whereas creative processes might be handled by naturalistic theories of discovery. For the time being, philosophers of science are still dealing with creative discovery using mainly historical and sociological tools. Much data on "chance" or serendipitous discovery have been accumulated (for recent books dealing with serendipity, see Shapiro, 1986; Kohn, 1989; and Roberts, 1989), and the time is now ripe for treating these phenomena with the naturalistic tools, rather than regarding them as mere curiousities.

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