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Scientific Discovery Reloaded

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Abstract

The way scientific discovery has been conceptualized has changed drastically in the last few decades: its relation to logic, inference, methods, and evolution has been deeply *reloaded*. The 'philosophical matrix' moulded by logical empiricism and analytical tradition has been challenged by the 'friends of discovery', who opened up the way to a rational investigation of discovery. This has produced not only new *theories* of discovery (like the deductive, cognitive, and evolutionary), but also new ways of *practicing* it in a rational and more systematic way. Ampliative rules, methods, heuristic procedures and even a logic of discovery have been investigated, extracted, reconstructed and refined. The outcome is a 'scientific discovery revolution': not only a new way of looking at discovery, but also a construction of tools that can guide us to discover something new. This is a very important contribution of philosophy of science to science, as it puts the former in a position not only to interpret what scientists do, but also to provide and improve tools that they can employ in their activity.

Keywords Logic · Discovery · Heuristics · Reasoning · Psychology · Algorithm

1 Scientific Discovery: The Matrix

A long-standing and influential tradition has shaped the way scientific discovery has been accounted for. It has been put forward in particular by logical empiricism, mathematical logic and the analytical tradition in philosophy, which moulded the matrix, that is, the origin and the conceptual framework, of the received theory of scientific discovery. It simply maintains that there is no way of accounting for scientific discovery in logical or even rational terms: "there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. […] every discovery contains an 'irrational element,' or 'a creative intuition,' in Bergson's sense" (Popper 1961, 32). This idea, which opens up the way to the 'psychology of discovery', breaks down into two approaches.

The first argues that scientific discovery is a *black box*: the final hypothesis is the only thing that we can see, and this is the outcome of a subjective, idiosyncratic, completely personal process and, as such, it cannot be reconstructed by rational means: "there are extraordinary aspects of the person who is able to produce significant new works" (Weisberg 2006, xii), and they are essential for discovery. Genius (see e.g. Murray 1989), illumination, 'faculties' such as intuition, insight, or 'divergent thinking', are common notions employed to support this thesis. Even if this line of argument ends up with a *obscurum per obscurius*, in principle that is not a problem: the whole process of discovery does not matter so much since what philosophy and science can reasonably do is evaluate a hypothesis only *after* it has been generated—since generation and justification are logically, temporally and in methods separated. This line of argument is maintained by famous philosophers like Popper (1961) and Laudan (1977, 1981) as well as scientists like Einstein (1958) and Frege (1960).

The second approach argues that even if discovery is a matter of psychology, we can understand it and account for several properties of it. This view has been popularized by Poincaré (1908), and refined and extended by Wallas (1926), Hadamard (1958) and Simonton (1988). Wallas made explicit the stages of the process of discovery discussed only informally by Poincaré, while "Hadamard marshaled additional evidence for the theory of unconscious processing and tried to demonstrate the wide role of such processing in other areas of cognition" (Weisberg 2006, 398–399). A typical example used to support this idea is Kekulé's discovery of benzene structure (Kekulé's dream). While somnolent in front of his fireplace, August Kekulé had a vision of atoms

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Table 1 Kinds of mental processes underlying the generation of hypotheses

	Parallel		Serial	
	Conscious	Unconscious	Conscious	Unconscious
Same	x	Parallelism (Poincaré)	Poincaré	x
Different	x	Associationism (Freud)	x	x

dancing one after the other and then, later, of a snake biting its tail (forming a ring). It was this dream that revealed the real structure of the benzene ring to the German chemist, according to his own report.

To provide more detail, this second viewpoint, in turn, breaks down into two branches. The separation is about the nature of mental processes, the conscious and unconscious ones, which are involved in the generation of hypotheses during the search for the solution to a problem.

A first branch maintains that conscious and unconscious processes are of the same kind: they are ‘homogeneous’ and they only differ in the way data-processing is performed. The conscious mind works in a serial way, while the unconscious works in a parallel way. Poincaré supported this idea, which is labeled *parallelism*.

By contrast, a second viewpoint states that the two mental processes are heterogeneous, they are different in kind and cannot be reduced one to the other. This hypothesis is labeled *associationism* and is supported by Freud and his followers.

In more detail, unconscious thought and its relation to conscious thought is approached along two dimensions:

(1) the nature of the links that connect one unconscious thought to the next one. A link can employ the same processes as the conscious one or not. In the former case we have a homogeneity between conscious and unconscious thoughts, in the latter a heterogeneity.
(2) whether the stream of thoughts are *parallel*, in the sense that you can process multiple ideas at the same time, or *serial*.

Combining these two dimensions, which are not mutually exclusive, we obtain several possible approaches to the mental processes underlying the generation of hypotheses (see Table 1).

Two of them, *parallelism* and *associationism*, as stated above, are particularly interesting for the psychologistic approach to scientific discovery.

The first conception states that unconscious processes are parallel and that they involve the same links as the conscious ones. The only difference is quantitative, i.e. speed, as an outcome of parallelism: unconscious thought is much faster. Since the links that enable us to connect an idea to another are the same for conscious and unconscious processes, it follows that once the hypothesis has been generated, the person who produced it is in a position, in principle, to understand how it came out: “the unconscious does not do anything that conscious processing could not do if there were but time available” (Weisberg 2006, 395).

The second branch maintains that the links created by unconscious thought are different from those of conscious thought. That is, unconscious processing can generate associative links for reasons of which one is not aware and perhaps could not be. The difference is qualitative. So “a creative leap can come about because (1) the processing has occurred on an unconscious level, which results in the thinker’s being surprised by the sudden leap; and (2) the leap is based on connections that the person could never think of using conscious thought, which is a second source of surprise” (*Ibid*., 389).[1]

The psychology of scientific discovery has been refined several times (see for example Csikszentmihalyi and Sawyer 1995) and also popularized in movies and novels. An examination of all the different versions of it is beyond the goals of this paper. Its ‘philosophical matrix’ is clear enough. What is more interesting to discuss here are the several, critical weaknesses that the whole idea of a psychology of discovery faces. I will examine in particular two main problems:

1. the sources employed to extract the mental processes;
2. multiple discovery.

1. The source of traditional psychology of discovery and its findings is introspection, which is a conscious reconstruction of an unconscious process (the generation of a hypothesis to solve a problem). More specifically the pieces of evidence used in this case are auto-reports and interviews—such as the ones provided by Poincaré and Kekulé. But auto-reports and interviews are sources hard to trust. They are provided by individuals who “have developed their own theories of creative thinking that rely on the opportunity for unconscious processing” (Weisberg 2006, 429). Furthermore “the interviews provide evidence of the role of unconscious processing in producing illuminations”, but “the empirical support for the idea of unconscious processing in creative thinking consists of a number of anecdotal reports” (*Ibid*.).

   Auto-reports and interviews not only are fragmentary and anecdotal, but they also imply “that this particular

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[1] This theory, which of course stems from Freud, is a version of the out-of-mind view, as the person has no conscious awareness of, or control over, the links among ideas.

aspect of discovery is not available to the awareness of the discoverer, and hence that the discoverer cannot be a source of a reliable description of the process that produced the sudden image. It does not in any way imply that the process is fundamentally different from other processes of discovery—only that we must seek for other sources of evidence about its nature" (Simon et al. 1987, 328–329).

Bottom line: auto-reports and interviews "are not adequate grounds on which to build a scientific theory of creative thinking" (Weisberg 2006, 429) and they are "the unwarranted source of a great deal of mysticism about the discovery process. In particular, the subconscious nature of the process is a red herring" (Simon et al. 1987, 329).

2. A psychology of discovery does not adequately account for multiple discovery (or simultaneous discovery, see e.g. Lamb and Easton 1984) and neglects the role of the cumulative, collective construction of hypotheses to solve problems.

A multiple discovery happens when scientists working independently of each other achieve the same discovery at about the same time. History of science contains plenty of this kind of discoveries: the formulation of calculus by Isaac Newton and Gottfried Wilhelm Leibniz, and the discovery of oxygen by Carl Wilhelz Scheele and Joseph Priestley, Antoine Lavoisier (and others) are probably the most recognized examples.

Multiple discoveries are a problem for a psychologistic approach: if a discovery is the outcome of personal, intra-psychic, and unconscious factors, it becomes hard to explain how different researchers working independently can discover the same thing more or less at the same time. As a matter of fact an explanation of this specific, but non-rare, kind of discovery requires an analysis of the role of concepts such as 'zeitgeist', shared knowledge, theories and data, which are not reducible to an individual psychology of discovery: "if there is a common world picture shared by the members of the scientific community, certain discoveries are inevitable since these discoveries are 'in the air.' Consequently, several scientists who share the common repertoire of ideas and try to solve the same problem may arrive at similar solutions" (Kantorovich 1993, 186). Since the list of multiple discoveries is too long to be dismissed, this approach cannot provide an adequate account of scientific discovery.

Moreover, "the discovery process typically is structured in time rather than being a momentary psychological experience of the solution popping into someone's head" (Nickles 1981, 87) and, in addition "personally, a scientist may make very few scientific discoveries, if any, during his lifetime; most scientific discoveries are products of collective efforts. [...] If the process extends over a long period of time, only the final step in the process is regarded as a discovery. Yet the contributions of the other participants are sometimes no less important than the contribution which constituted the breakthrough" (Kantorovich 1993, 12).

Bottom line: the psychology of discovery argues that not only is there no logic of discovery, but also that no method is possible for it, no inferential way, and the entire approach relies on the static, a-temporal and obscure concept of intuition.

But many problems of a psychology of discovery can be answered by simply noting that reasoning, argumentation and inferences play a decisive role in the process of discovery. These are rational means that can be applied to several problems and, as such, will often produce the same, or very similar, conclusions. No surprise then, for instance, for multiple discovery: "in normal science scientists who start from the same transparent presuppositions and employ the same theory and observational data would arrive at the same result" (*Ibid*., 186–187).

These and other open problems in the philosophical matrix of discovery made necessary a deep rethinking and reloading of it.

2 Reloading Scientific Discovery

The matrix incepted by logical empiricism and mathematical logic has been reset and then reloaded in several ways. New approaches have been developed by questioning some of its tenets (like the separation between context of discovery and context of justification see in particular Shelley 2003) and by reconsidering not only the possibility and meaning of a logic of discovery (Hanson 1958, Simon 1987; Nickles 1985; Cellucci 2013), but also the role played by logic, method, inferences and even evolution in scientific discovery (see in particular Nickles 1980a, b; Meheus and Nickles 2009; Clement 2008; Cellucci 2013, 2017; Grosholz 2007; Grosholz and Breger 2000, Ippoliti and Cellucci 2016; Nersessian 2008). I will examine three of them—the deductive (§ 2.1), the cognitive (§ 2.2) and the evolutionary approach (§ 2.3)[2]—and then I will show how the rethinking triggered by these approaches, and their lessons, made

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[2] This examination is not intended to be exhaustive: it analyses a few representative works of approaches that eased a new way of accounting for scientific discovery.

possible a new theory and practice of scientific discovery—a ‘scientific discovery revolution’ (§ 3).

2.1 Deductive Reloading

One way of reloading the problem of scientific discovery is the deductive one. It simply maintains that there is something like a logic of discovery, and this is just deductive logic. Of course this idea faces a straightforward objection. Since deductive inference and reasoning are non-ampliative, meaning that the content of their conclusion is contained in the premises,[3] nothing new can be inferred from them. Thus, nothing new can be obtained from their application: the function of deductions is to make explicit in the conclusion pieces of information that are only implicitly contained in the premises. A deduction allows us rewrite the information embedded in the axioms in a way that is much more understandable and testable but, from a logical viewpoint, it cannot extend them: at most it is a way to establish *new* logical relations between known findings, but it cannot produce *new* findings.

A response to this objection shapes the deductive view of discovery (see in particular Musgrave 1988, 1989; Zahar 1983, 1989).

First, a well-known line of argument of the deductive approach teases apart logical and psychological novelty (see also Ippoliti 2014 on this point). It argues that even if a deduction cannot be ampliative from a logical viewpoint, it is ampliative from a psychological viewpoint: by deducing consequences we gain genuine new knowledge since the truth of our premises, or postulates, is not enough to foresee the truth of their consequences. We need an effort to obtain a deductive conclusion from given premises or postulates—to choose and combine the premises in the appropriate way—and thus a deductive consequence is *new* knowledge. The deductive approach supports this claim with several arguments, such as the semi-decidability of the theories, the surprise of unexpected consequences, the need of new individuals in deduction, and the epistemic aspect of conclusions (see e.g. Dummett 1991; Hintikka 1973; Rota 1997).

A second line of argument of the deductive approach argues that deductive reasoning provides a logic of discovery since it is possible to rebuild ampliative inferences, such as induction and analogy, in terms of deductive arguments. In more detail, ampliative inferences can be reconstructed as deductive inferences with suppressed premises (a kind of enthymeme).

An inductive inference, argues Musgrave (1989), can be rebuilt as a deduction with suppressed premises in the following way. Let us take for example the following inductive argument I_1:

$$\frac{\text{A particular raven is black}}{\text{All ravens are black}}$$

In stating such an inductive hypothesis we are not walking in dark. As a matter of fact, we move from a certain assumption, such as “all ravens have some common color” (p_1). Of course, such an assumption is domain-specific and expresses common beliefs about our experience in a specific niche. In turn, the assumption p_1 can be obtained from another, more general premise p* such as “ravens belong to a family of kinds of birds whose members have a common color”. When another premise, p_2 “a particular raven is black”, is added (of course p_2 is drawn from observation), then we have the following deductive argument D_1:

$$\frac{\begin{array}{c}\text{All ravens have some common color}\\ \text{A particular raven is black}\end{array}}{\text{All ravens are black}}$$

So, I_1 can be converted into D_1 by introducing some appropriate assumptions or premises like p_1 or p*. The conclusion of an inductive argument I_1 at this point does not constitute a novelty with respect to the premises p_1 and p_2 and follows necessarily from the them.

The same holds for analogy. An analogical inference, argues Musgrave, can be rebuilt as a deduction with suppressed premises in the following way. Let us take for example the following analogical argument A_1, the Watson argument for the structure of DNA:

DNA has a chemical composition analogous to TMV (tobacco mosaic virus)

TMV has a form of nucleic acid as a major chemical constituent

$$\frac{\text{The TMV molecules are helical in structure}}{\text{The DNA molecule is helical in structure}}$$

A_1 can be converted into a deductive argument D_2 in the following way:

The same (similar) effects have the same (similar) causes

The chemical composition of the DNA and TMV are similar

The structure of a TMV follows from its chemical composition

$$\frac{\text{The TMV molecule is helical in structure}}{\text{The DNA molecule is structurally similar to TMV (helical in structure)}}$$

In this way we have a method of discovery based on deductive logic (a ‘deductive heuristics’): we discover something by starting with a hypothesis that is plausible and then

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[3] A dedutive inference is obtained by using primitive rules whereby the content of their conclusion is literally included in their premises. A stock example is *modus ponens* $(A, A \rightarrow B \therefore B)$—B is literally part of the second premise. Thus a deductive reasoning, as a chain of these basic rules, cannot expand *logically* the premises.

deducing consequences from it. In this sense, successful scientists are ones that employ the *right* assumptions, and a discovery is a deductive consequence of these assumptions (that are different from the ones employed by his colleagues at that time).

So, content-specific, suppressed assumptions are the key to scientific discovery and deductive reasoning is the necessary addition to them. In a sense, it is not a surprise, as the pivotal role of suppressed (or tacit) assumptions in scientific reasoning has been explicitly stated and examined (see e.g. Polanyi 1966).

On one hand, the presence of domain-specific suppressed premises in scientific reasoning would explain not only why it is often difficult for non-experts to understand what is going on inside a field of expertise, but also why the experts, have difficulties in explaining to non-expert their reasoning. These premises are invisible, or transparent, to experts as they employ them implicitly. They are not aware of them, they work as if such assumptions were self-evident, and continually use them to fill the gaps in their inferential activity.

On the other hand, the fact that scientific reasoning is full of domain-specific, tacit premises allows us to explain part of the process of generation and selection of hypotheses in problem-solving. These assumptions are used to fill some gaps in their inferential activity and are means to *integrate* the data and knowledge involved in the problem and in this sense they shape the construction and selection of hypotheses—for example they narrow the range of possible hypotheses to be pursued. As a consequence, tacit premises offer a possible solution to the problem of under-determination. In effect, in trying to solve a problem, scientists do not have to choose between infinite logically possible hypotheses since this range is narrowed to only a few hypotheses by these suppressed, invisible assumptions.

Thus, suppressed assumptions play a pivotal role also in the way the deductive approach reloads the issue of scientific discovery in relation to methods, inference, and logic. In effect, there is no need for creativity, intuition, or genius to explain scientific discovery. To sum up:

> logical empiricist orthodoxy is wrong: there is a logic as well as a psychology of invention, and there is a psychology as well as a logic of appraisal. Moreover, the logic of invention is best regarded as deductive logic. Finally, logical empiricist orthodoxy is also wrong to say that the context of invention is irrelevant to the context of appraisal. Consideration of inventive arguments yields minimal plausibility appraisals, and it also can tell us which facts are novel for the theory being considered (Musgrave 1989, 32).

Even if the deductive approach shows that at least in part the process of discovery can be accounted for in rational and logical terms, the idea of "deductive heuristics" as a logic of discovery is unsatisfactory.

As concerns the first line of argument, the distinction between logical and psychological novelty, the arguments put forward to support it do not work: not only is there no way to logically extend our knowledge by means of deductions from axioms but also in principle is there no need of any work, or effort, to get deductive consequences from a set of axioms, as this could be done in a mechanical way by using a brute force-exhaustive procedure, for instance the British Museum Algorithm (Newell et al. 1958). Such an algorithm would generate all the theorems from a set of premises, starting from the shorter up to the more complex ones. Even if computationally ineffective, it would not require any intellectual work.

As concerns the second line of argument, i.e. role of deductive heuristics, it does not account for a crucial step in discovery, that is generation of hypotheses (see Ippoliti 2017a). It does not tell us how to get premises to use to derive interesting results. In addition, a deductive heuristics can be applied only when concepts or properties are already known and available: it is not a way for generating new ones. Moreover, a deductive heuristics is a reconstruction in a deductive fashion of something that has been obtained in a different way. Thus, in order to be effective it needs a vantage point, i.e. the knowledge of the final result: "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" (Kantorovich 1994, 8). In addition the fact that a pieces of reasoning can be rebuilt in a deductive way, does not remove the uncertainty in that reasoning, which is simply moved to its premises. And deductive reasoning does not and cannot justify its premises.

Of course the fact that deductive reasoning cannot account for the process of discovery does not imply that it is useless, but simply that the generation of a hypothesis—a crucial step in discovery—cannot be reduced to a deduction (on this point see also Cellucci 2013; Ippoliti 2017b).

2.2 Cognitive Reloading

An alternative way of reloading the problem of scientific discovery is the cognitive approach, and in particular a main branch of it: the computational approach.

This approach, incepted by Simon and Newell, draws upon several features of Poincaré's account of scientific discovery, as it aims at providing an "examination of the phenomena of incubation and illumination and their explanation" (Simon 1977, 292). This theory of discovery argues for the so-called *nothing special view*, "which proposes that creative products of all sorts are brought about by our ordinary cognitive processes, such as those involved in our day-to-day problem-solving activities" (Weisberg 2006, xii).

Thus the cognitive approach sees scientific discovery as a *problem-solving* activity and it "does not assign any special role to the unconscious—or, for that matter, to the conscious. It assumes, implicitly, that the information processes that occur without consciousness of them are of the same kinds as the processes of which the thinker is aware" (Simon 1977, 292). This tenet is essential to develop the most advanced version of this approach, a mechanization of scientific discovery—*conceptual* discovery as well as discovery of *regularities*. In effect the cognitive approach argues that the processes underlying a scientific discovery are ordinary (not special) and can be examined, reconstructed and reproduced in a rational way. In addition it argues that "human beings should be looked upon as information- processing systems, analogous to computers, and that the concepts underlying our understanding of the functioning of computers could be applied to understanding human cognition" (Weisberg 2006, 119–120).

The practical outcome of it is the BACON software package (Simon et al. 1987), a kind of 'mechanical discoverer'. Simon provides us with several examples of historical scientific discovery that can be obtained by BACON, such as Kepler's third law. Fed with data on the periods of revolution (P) and the distances (D) of the planets from the sun, the program runs the following recursive heuristic rules:

i.) if two variables co-vary, introduce their ratio as a new variable;
ii.) if they vary inversely, introduce their product as a new variable and check if it is constant.

The iteration of these two simple rules is enough to obtain Kepler's third law in a purely mechanical way (see Simon et al. 1987, 68–81). In a similar way, Simon shows us how it is possible to account in purely computational terms for many scientific discoveries, such as Boyle's law, Ohm's law, the Law of Uniform Acceleration, Coulomb's law, and the ideal-gas law. Moreover, Simon argues that BACON would be able to generate also new properties (concepts), for instance 'gravitational mass' (Simon et al. 1987, 155–156).

In this way the cognitive approach provides us with an answer to scientific discovery based on a rational account of it: it acknowledges that the solution provided by the deductive approach is untenable, while providing us with a better method and a theory of problems, and it shows us how inferences and simple heuristics shape the process of generation of hypotheses.

But while acknowledging the merits of this approach, one cannot ignore all its weaknesses and the fact that this idea of a mechanical discoverer appears untenable (see in particular Nickles 1980a; Kantorovich 1993, Gillies 1996; Weisberg 2006; Ippoliti 2017a).

In effect BACON is capable of doing, at most, only a specific part of the discovery, and in a sense the (most) trivial one.

First of all, BACON cannot perform a crucial step in the generation of a hypothesis: the identification of relevant variable and data. They are chosen and provided by the programmers: the software simply runs its heuristics over them. This means that the *real* problem that BACON solves is simply the estimation of two parameters from a set of data. It provides us with an alternative solution to this elementary problem and it does not discover a new generalization.

Second, BACON suffers the same weakness of the deductive approach: it is a reconstruction of a historical process. As such, it benefits from the knowledge of a 'backward look'—the advantage of knowing that something is a problem, that its data can be approached by certain heuristics, and that the problem is solvable. Thus, at most, we can say that this software discovers in *a new solution* to a solved problem.

Third and even more important, it lacks the fundamental capability of producing a *really* new concept or property, or better to build a conceptualization that is required to solve and pose problems. Just like any software, it helps and boosts human beings in computational tasks such as finding regularities and making calculations.[4]

2.3 Evolutionary Reloading

An alternative way of reloading the problem of scientific discovery is the evolutionary one. This approach draws on the evolutionary epistemology and on an explicit analogy with biology, namely "the metaphor of an organism as a problem-solver" (Kantorovich 1993, 224). Thus, this approach is a way of naturalizing the process of discovery, that is, to explain it without assuming supernatural (in the sense of not being part of natural world) entities or events (such as genius).

Tellingly, while this view connects scientific discovery to evolution, it denies not only the possibility of a logic, or also a method, for discovery, but even the possibility of a rational approach to its core—the generation of hypotheses.

In particular this view makes use of the mechanism of evolutionary process, namely the processes of blind

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[4] Simon (Kulkarni and Simon 1988) relaxed the structures of BACON in later programs such as KEKADA, which, unlike BACON that "was concerned mainly with the ways in which theories could be generated from empirical data, with little or no help from theory" (Kulkarni and Simon 1988, 140), tries to deal with issues such as the question of where the data came from, the processes of designing experiments and programs of observation.

variations and selective retention (BV + SR), as an explanation for discovery. Such a reading of the process of discovery employs a dynamic view, according to which growth of a theory can be seen as the growth of an organism: in effect "both a research program, and an organism are engaged in problem-solving. A research program solves, for instance, problems arising from the need for adjusting its basic ideas [...] to new observational data. Similarly, an organism adapts to new environmental conditions on the basis of its genetic makeup" (*Ibid.*).

In effect the BV + SR mechanisms seem to be "incredibly creative problem solvers" (Nickles 2009, 193) and "biological evolution can be interpreted, metaphorically, as an innovative problem-solving process" (*Ibid.*, 182). One of advantages here is the fact that they seem to offer a way to get *more* knowledge from *less* knowledge: the recombination and blind variation of few simple items seems capable of producing something (more) complex and articulated.

Two of the main consequences of an evolutionary approach to scientific discovery, or better of the idea that discovery is a blind variation, are that it cannot be method-governed and that science has no predetermined goal. These two features, of course, are worrying since they seem to support the thesis that discovery not only is unintentional, but also that it happens by 'chance'. An answer to this objection (see e.g. Kantorovich and Ne'eman 1989) is "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" (Kantorovich 1994, 19). An outcome of this way of modeling scientific discovery is that the human mind is the 'host' of the process of blind variation and selective retention and "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" (*Ibid.*, 20). This idea of our mind as a mere container of a combinatorial-selective process enables a surprising version of the evolutionary approach, that is a computational one, which uses genetic algorithms (see Koza 1992, 1994; Koza et al. 1999). In this particular sense, the evolutionary approach does defend an idea of a method for discovery, even if a limited one. Here rationality is at the beginning and at the end of a process, which takes place by combining items in random way: humans can set-up the process, wait for the outcomes and select one of them. So in this sense, against Popper and Campbell, an evolutionary approach would not be the antithesis of method and to some degree it can methodized.[5]

Nonetheless, the endpoint of this evolutionary approach is hard to defend.

First, the idea that serendipity[6] can be used as a general model to account for scientific discovery seems untenable for at least two reasons—one quantitative and one qualitative. On one hand, the discoveries made in that way are few, and, at most, they are an epiphenomenon. On the other hand, the very notion of 'discovery by chance' does not explain precisely the cases that it employs to support its own view. Fleming, Columbus, or Poincaré, did not discover 'by chance', but the discovery was made after they used a lot inferences, strategies, and plans—all rational tools. Of course the final product was in a sense unexpected, but it was found after many options had been explored and rejected by the discoverer or others (e.g. in the case of Poincaré's mathematical discovery), or after several controlled trials (e.g. in the case of Fleming's discovery of penicillin), or simply the final discovery was not excluded from the very beginning (e.g. Columbus in trying to circumnavigate the earth could not exclude that other lands could be encountered).

Second, evolution can be used to support a different and more rational account for scientific discovery. In effect, it can be argued that evolution shows us that biological processes are not simply a kind problem-solving, but *inferential* problem-solving also at a very basic and primitive level (see von Helmholtz 1866; Gregory 1970, 1980). Vision would provide a stock example in this sense. Following this tradition and its findings, our visual apparatus can be reconstructed in inferential term, as it continually would make hypotheses to solve problems: it is not a camera, but an inferential machine (see e.g. Cellucci 2013, 2017a). These inferences are ampliative, unconscious, non-propositional and built-in by evolutionary adaptation: they processes data (light beams) and generate hypotheses using non-deductive rules on the base of past experience. So, here there is not a neat distinction between generation and justification of a hypothesis, as the data to generate and verify a hypothesis can be the same. In addition these inferences and hypotheses are domain-specific: a change of environment or niche can lead them to error.

This way of naturalizing scientific discovery, which explicitly connects a method-governed approach to discovery and evolution, changes again our theories of discovery and it paves the way for the construction of a variety of rational tools and *rules* to advance knowledge: it harmonizes theory and practice of scientific discovery in a *new* way and

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[5] I would like to thank Tom Nickles for a clarification of this point. 5FL01

[6] See also (Gillies 2014 and Roberts 1989). 6FL01

opens the door to a *logic* of discovery. It initiates a scientific discovery revolution.

3 Scientific Discovery Revolutions

This new view on theory and practice of scientific discovery has its roots in Lakatos' work on heuristics (Lakatos 1976) but has expanded, revised and improved it a lot.

It borrows from Lakatos' work and from the several reloaded versions of the issue the idea that scientific discovery is problem-solving,[7] and argues that it can be accounted for in an inferential way. Moreover it reconciles evolution and logic, since it argues that the way we discover something is by using a kind of reasoning that has been built-in our cognitive and biological make-up by adaptation (see Cellucci 2013). This new way of characterizing scientific discovery not only removes the separation between the generation and the testing of a hypotheses (as heuristic procedures play a role also in testing and selecting a hypothesis), but it also provides a unifying approach to problem-solving and problem-finding.

I will examine its theory of discovery and then I will look at the practices suggested by its way of conceptualizing scientific discovery.

As concern the former, it maintains that discovery is a *natural* process that has a *method*, which is bottom-up, and a *logic* with *rules*. By methods it means simply a means to an end, a way of pursuing a certain aim. It is provided by a specific version of the analytic method, which offers a very general bottom-up framework for solving problems (see Cellucci 2013). More precisely, we start looking for a hypothesis that is a sufficient condition for solving the problem—that implies a solution to the problem. This hypothesis is obtained from the bottom, i.e. from the problem and possibly other data already available, by the application of some non-deductive rules. The only requirement is that it has to be plausible.[8] Then the hypothesis undergoes a plausibility test and, in positive case, is provisionally accepted. In turn this hypothesis is a new problem, which must be solved and its solution is sought in the same way.

Of course in order to work, and to produce a logic of discovery, this framework must be endowed with procedures for the generation of hypotheses. These procedures are non-mechanical, as "the purpose of a logic of discovery is not to dispense with the need for intelligence by use of an algorithmic method, but rather to expand natural intelligence providing it with heuristic means–means capable of guiding natural intelligence, albeit not infallibly" (Cellucci 2017a, 143–144). In more detail, these procedures are heuristic in kind, for they do not guarantee a solution of the problem: they are ampliative and as such they can lead to erroneous conclusions. Nonetheless, they are a *genuine* way to construct new hypotheses in a rational way.

Here comes the practical, and in some ways more important, side of this new conceptualization of scientific discovery. In this sense 'scientific discovery revolution' argues that we have to stop thinking about a *theory* of discovery, and to concentrate on the investigation of procedures to be *used* to generate new hypotheses to solve problems. The study, production and classification of heuristic procedures are the frontier of a logic of discovery, and not surprising they have been improved a lot recently (see in particular Cellucci 2013 ch. 20–21; Jaccard and Jacoby 2010; Ippoliti 2017b; Ippoliti and Cellucci 2016).

In essence these heuristic procedures are tools for *modelling* the research space of hypotheses—what Simon defines the 'problem-space'. To produce a new hypothesis we have to combine ideas and concepts in many ways and this will generate a combinatorial explosion in the problem-space. A heuristic procedure is a means to handle such a combinatorial space: it is a tool to build such a space, as this space essentially depends on the existing knowledge and the available data. Such a space is determined not only (i) by the way the data are processed using a heuristics, but also (ii) by the way data and the corpus of existing knowledge change over time. The application of a heuristic procedure can reduce as well as expand a problem-space.

A heuristics can be classified in several ways, starting from the well-known distinction between *positive* and *negative* heuristics (see Lakatos 1976).[9] A further useful distinction is the one that tells apart *primitive* and *derived heuristics*, or *generative* and selective *heuristics* (see Ippoliti 2017b). The primitive are analogies, disanalogies, inductions and their combinations (e.g. analogies between analogies). Any heuristic procedure is a variant, combination or juxtaposition of these primitive heuristics, for it *presupposes* them. They enable us to build a more articulated and complex (derived) heuristics, e.g. metaphors or scenario-building. The set of derived heuristics is *open*, as new rules can be produced as new problems are solved, nonetheless we can classify them into several classes: inversion heuristics, heuristics of switching, scenario building, thought

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7FL01 [7] This idea goes back to Plato, and Aristotle (see Quarantotto 2017)
7FL02 and has been re-proposed also recently (Laudan 1977, 1981; Nickles
7FL03 1981; Cellucci 2017b).

8FL01 [8] Plausible here, following Aristotle's notion of *andoxa*, simply
8FL02 means that the arguments for the hypothesis are 'stronger' (in quality)
8FL03 than those against it on the basis of the existing knowledge.

[9] A positive heuristics guides us in the construction of admissible paths during the search of a solution for a problem. A negative heuristics prevents us from building certain paths—by blocking the *modus tollens* on a specific part of the theory. 9FL01 9FL02 9FL03 9FL04

experiments, the analysis of extreme cases, the analysis of a deviant case amongst others (see Jaccard and Jacoby 2010; Ippoliti 2017b). For example the heuristics of switching[10] is based on a specific change of viewpoint, which we get by switching from one order of analysis to another one. Thus it is based on the study of explicit analogies and disanalogies.[11] Moreover these heuristic procedures cover both sides of problem-solving, namely to *solve* and *pose* problems.[12]

Thus this new, inferential, approach has revolutionized not only the way of theorizing discovery but also, and more importantly, the ways of practicing it in a rational and more systematic manner. We have now a better, finer-grained understanding of methods and toolboxes to use as a guide to discover something new. And we can improve them. This is a remarkable contribution of philosophy of science to science, as it puts the former in a position not only to interpret what scientists do, but also to provide and improve tools that they can employ in their activity.

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[10] Stock examples are: change of *unit* of analysis, change of *level* of analysis, focus on *processes* vs focus on *variables*.

[11] An *atomistic* view of heuristics, that there is an ultimate, base set from which all others can be compounded, has also been put forward by Gigerenzer and Todd (see Gigerenzer et al. 1999).

[12] There are many examples of problems generated by heuristic procedures, like Poincaré's conjecture.

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