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REASONS AND OBSTACLES FOR A LOGIC OF DISCOVERY

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I

The possibility of formulating a logic of discovery appeared to have been discarded given the attitude adopted in this regard by the philosophers who developed the standard conception of science. Beginning several decades ago, nonetheless, reactions against this conception have revived interest in the problem. Nowadays, there is a notable tendency to consider the philosophical relevance of the problem of discovery and, as a consequence, proposals have been advanced for characterizing the procedures which lead to scientific discoveries. Disconformity with the classical view resides, mainly, in the belief that its principal proponents have utilized a conception that is too narrow in a rational sense and, thus, have excluded from their analysis those aspects of scientific activity that do not conform to a strict logical examination. It is not simple, however, to establish the terms of this debate. There are various interlocking questions and diverse nuances that emerge. Hence, some authors identify rationality with the existence of rules such as those in deductive or inductive logic[1]. Others, in contrast, conceive of rationality in a broader sense that includes the consideration of facts, objectives, values, scientific traditions, etc. A third group of authors shares with the more orthodox thinkers the idea that logic should be interpreted in a restricted sense; but

[1] In the present text, for reasons of space, we are not concerned with research carried out in the field of Artificial Intelligence as related to the problem of scientific discovery. However, we consider that the interesting contributions resulting from the design of computer programs which permit the reconstruction of certain scientific discoveries (cf. Langley e.a. 1987) would not alter, fundamentally, our conclusions.

they consider that the latter were mistaken when they concluded that discovery does not respond to logic in a strict sense. It may be useful, then, to identify some of the different aspects of the problem and to sketch out the positions that they give rise to.

First, various features may be attributed to the conception of discovery: to indicate, for example, as Nickles has done, whether "to discover" is an achievement verb, a "got it" verb, or a process verb (Nickles, 1980, p. 9). In addition, we may wonder what should be understood by "rationality" and to what extent rationality includes logical procedures. In turn, the conception of logic itself becomes problematic. However, we do not consider the prospect of trying to resolve these questions here as very promising. Rather, it seems more appropriate to compare the different positions that have been put forth with respect to the problem. To begin, we will enumerate the theses that are generally attributed to the orthodox conception and, then, we will present the alternatives which, to a greater or lesser degree, emerge. In the subsequent sections we will concern ourselves with the individual authors who maintain each of these theses. As we shall see, in each case, one same author may subscribe to various alternatives. In the final section we will offer our own reflections upon the problem.

The position that can be attributed to the orthodox conception is expressed in the following theses explicitly maintained by Popper, Hempel, and Koestler:

(1) Scientific activity is fundamentally composed of two distinguishable aspects, discovery and justification of a hypothesis.

(2) The justification process obeys the application of logical rules.

(3) Justification is a rational activity.

(4) Discovery, on the other hand, is of a decidedly non-rational character.

Those who disagree with the orthodox conception may maintain, essentially, various of the alternative theses:

(a) Discovery and justification are distinguishable aspects, but each responds to the application of logical patterns.

(b) Discovery and justification are distinguishable aspects, and each responds to the application of logical patterns, but discovery also incorporates non-logical elements.

(c) The discovery-justification dichotomy must be replaced by a characterization that better responds to real scientific activity, introducing an intermediate stage in which the scientist decides upon the convenience

of pursuing the elaboration of a theory, but without this stage being identified with justification.
(d) Essentially, a conceptual distinction between both aspects may be established but, in the measure to which they are found to be interlocking or to take place simultaneously, it is impossible to differentiate both aspects over the course of scientific research activity.
(e) Discovery incorporates non-rational elements, but this is not a characteristic that differentiates it from justification because the same may be asserted of the latter.
(f) Although a distinction between the context of discovery and the context of justification cannot be established because both aspects constitute an indistinguishable whole, the combination of these two aspects qualifies as a rational process.
(g) In reality, there can be no distinction between discovery and justification because scientific activity occurs through a unique process that is forcibly penetrated by irrational elements.

II

The four classical theses were defended, fundamentally, by Popper and Hempel. In *The Logic of Scientific Discovery*, Popper restricted "the logic of science" exclusively to questions regarding the justification of hypotheses, while referring the examination of processes which lead to the discovery of new theories to the fields of psychology and sociology. And he did not vacillate in declaring discovery to be an irrational process.

> [...] my view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains "an irrational element", or "creative intuition", in Bergson's sense. (Popper 1959, p. 32)

Similarly, from such examples as the episode in which Kekulé, according to his own account, was inspired by the twisting of flames to conceive the formula for the benzene molecule, Hempel sustained that there are no logical methods for arriving at the discovery of new hypotheses.

Many critics do not consider episodes such as that described by Kekulé to be representative of the history of science. Still, they are taken seriously by Arthur Koestler. Koestler maintains that the processes of scientific discovery are clearly differentiated from the mechanisms of verification and are similar to the generation of artistic ideas. According to Koestler, there are two types of thought processes: associative and bisociative. The former are routine, guided by certain rules, as occurs, for example, in the context of a game. Thus, for instance, if the game consists in finding opposing expressions, then the terms, 'black', 'good', 'bit', and 'long', are expected in response to 'white', 'bad', 'small', and 'short'. The bisociative processes, on the other hand, arise from the combination of two systems of different rules and are unexplainable because it is not possible to represent them in propositional form. They can only be described in psychological terms, like the phenomena indicated by *Gestalt* theories. Scientific discovery, then, arises from the combination of initially disconnected ideas, like magnetic and electric phenomena, thanks to an unexpected bisociative inspiration. This experience corresponds to what Koestler refers to as the "Aha reaction", similar to the "Haha reaction" which expresses the perception of humor in a situation, and the "Ah reaction" which corresponds to the artistic experience (Cf. Koestler 1967).

III

Recently, Musgrave, in contrast to the standard position, defended the idea that discovery, although different from justification, proceeds according to strict logical criteria. Although he recognizes that the description of inventive processes is a task for psychology, he reserves for logic the task of reconstructing the corresponding reasonings. Still, in contrast to Reichenbach, he maintains that deductive logic is involved. He does not discard the possibility of offering inductive reconstructions for the emergence of scientific hypotheses, but he estimates that deductive reconstruction is more promising. For carrying this out it would be necessary to specify all the premises that a scientist takes into account at the moment of proposing a new hypothesis. These premises include methodological principles such as simplicity, or metaphysical ones such as causality. Thus, for example, while an inductive focus would

reconstruct the emergence of Rutherford's hypotheses, according to which the structure of the atom is similar to the planetary system, as the application of a heuristic rule with an analogical character, the reconstruction of Musgrave would replace such a rule with the incorporation of premises referring to the similarity of effects and causes in conjunction with certain similarities of behavior between the solar system and atoms. Musgrave warns that even as his proposal appears plausible in such cases as that described, it could not take into account discoveries such as that which exemplifies the hypothesis of Kekulé regarding the molecular structure of benzene; but he minimizes this type of objection as he considers that this would be the same as counseling scientists to invent hypotheses at random instead of trying to deduce them on the basis of previous knowledge (Cf. Musgrave 1989). In this way, the examination of Musgrave can be classified under thesis a).

Also Hanson did not oppose the thesis that discovery follows logical patterns. But he does not believe that it involves either induction or deduction. In accordance with Peirce, he maintains that scientific discovery utilizes abductive inference. Hanson establishes a distinction between the reasons that suggest a kind of hypothesis that should be considered, and the reasons for accepting a particular completely specified hypothesis. Thus, the reasons for considering a hypothesis are not sufficient for judging it as true, which can only be carried out by means of empirical testing. The retroductive or abductive reasonings have the following form (Hanson 1960, p. 407):

> 1) Some surprising, astonishing phenomena p_1, p_2, p_3 ... are encountered.
> (2) But p_1, p_2, p_3 ... would not be surprising were a hypothesis of H's type to obtain. They would follow as a matter of course from something like H and would be explained by it.
> 3) Therefore there is good reason for elaborating a hypothesis of the type of H as a possible explanation for p_1, p_2, p_3 ...

This scheme would permit the reconstruction of, for example, the origin of Kepler's hypothesis regarding the orbit of Mars: the irregularities observed by Tycho Brahe suggested to Kepler that Mars must follow a trajectory best represented by a continuous curve that is closed and not circular. According to Hanson, inductivism is correct in maintaining that laws are obtained through the inference of data, but its error consists in

maintaining that those laws only express a summary of the data. The hypothetico-deductive account, in turn, is correct in considering that laws offer explanations of data, but does not clarify the initial connection between the data and the laws. Abductive reasoning, in fact, takes into account this connection. Insofar as discovery, then, operates in conformity with strict rational guidelines, the analysis of Hanson – as in the case of Musgrave – falls into the category of thesis a).

Although it was Reichenbach who introduced the expressions, "context of discovery" and "context of justification", it would be erroneous to believe that he subscribed to all the theses attributed to the classical conception. In effect, while Popper and Hempel stress the irrational aspects of discovery, Reichenbach, on the contrary, emphasizes its rational character. He explicitly denies that scientific discovery can be compared to experiences such as mystical presentiment. He maintains, adversely, that the utilization of inductive methods marks the difference between the formulation of a scientific hypothesis and what would otherwise be the same as a hunch.

> Scientific genius does not manifest itself in contemptuously neglecting inductive methods; on the contrary, it shows its supremacy over inferior ways of thought by better handling, by more cleverly using the methods of induction, which always will remain the genuine methods of scientific discovery. (Reichenbach 1938, p. 383)

In fact, it does not attempt to describe, by any means, the mental process that corresponds to scientific discoveries.

> I shall not venture any description of the ways of thought followed by them in the moments of their great discoveries; the obscurity of the birth of great ideas will never be satisfactorily cleared up by psychological investigation. (*Ibid.*, p. 381)

It is apparent, however, that he fully recognizes the existence of an unyielding component of logical analysis. Thus, there is a certain difficulty in reconciling the ideas of Reichenbach. What he suggests is that there are two elements involved in scientific discovery: the idea that arises in the mind of the scientist, and at the same time an appreciation for its inductive relation with the events that originated the idea. The rationality of discovery resides, precisely, in this appreciation for the

inductive relation; while, perhaps, not even psychology would be able to formulate the laws that would explain the first element mentioned, that is, the conception of the idea. In this way, in the context of discovery, inductive logic plays the role of a kind of anterior evaluation to empirical testing. That would explain why Reichenbach maintains that "the situation before the empirical confirmation differs from that after it only in degree" (*Ibid.* p. 383). Thus, while Popper and Hempel discard the possibility of logically reconstructing discovery, Reichenbach considers the task of epistemology to consist in, on the one hand, the reconstruction of the process of discovery and, on the other hand, the evaluation of its merits according to methodological criteria. Hence, in spite of the presence of a non-rational component, discovery constitutes an essentially rational process, which is an idea that is reflected in thesis b).

IV

Among those who propose replacing the traditional dichotomy with a more adequate characterization, Martin Curd indicates that the discovery of a scientific theory is not produced in a determined moment but, rather, over the course of a process that he refers to as "the period of theory generation". This period begins when a scientist or a group of scientists begins to consider a problem and it ends with the writing of the final research report. With respect to this period, Curd identifies *a logic of theory generation* and *a logic of prior assessment*. The logic of generation corresponds to the inferences that scientists make in order to arrive at hypotheses and the justification of their reasonableness. The logic of prior assessment, on the other hand, corresponds to the appreciation of the degree to which they deserve to be developed and constitutes, therefore, a *logic of pursuit*. It should be pointed out that the logic of prior assessment takes place "after [the hypotheses] have been generated, but before they have been tested" (Curd 1980, p. 203), as a mode of selection between multiple hypotheses. However, he also says that the logic of generation depends on the categories of analysis of the prior assessment and, in particular, of the logic of pursuit (*Ibid.*, p. 205). The relationship between the two logics, then, is not clear. It would appear that Curd uses the word, "generation" in an ambiguous sense. On the one hand, it is used to refer to the emergence of a set of alternative

hypotheses in the mind of a scientist and, on the other hand, to refer to a more extensive process in which prior assessment is included.

Although Curd does not accept the distinction exactly as it had been traditionally understood, because the stage of discovery includes aspects that others would consider as pertaining to justification, what is certain is that, in some way, he draws upon the differentiation between discovery and justification. The latter would correspond to deduction and empirical testing of new predictions. Thus, Curd exemplifies thesis c).

Gutting, in contrast, does not share the idea that the testing of a hypothesis should be distinguished from other aspects of scientific activity. In his opinion, the problem of the rationality of discovery is resolved by the extent to which scientific activity is identified with "the development of justified hypotheses" and by the recognition that in addition to logic in the strict sense – which considers the reasons supporting a conclusion – which he refers to as $logic_1$, scientific activity must be analyzed in terms of a logic in a broader sense, $logic_2$, which takes into account the process of hypothesis invention. $Logic_2$ includes, principally, three types of rules: heuristic principles, scientific intentions and cosmological principles. Its scope is so broad that Gutting compares it to what could be called, for example, "the logic of art".

But although Gutting explicitly rejects the distinction between context of discovery and context of justification, he recognizes a difference between the initial thought of a hypothesis and its subsequent development:

> By "invention" I mean the initial thinking of a scientific hypothesis – or rather, of what will eventually be elaborated into a specific scientific hypothesis. For a hypothesis almost always enters the scientific world as a vaguely and incompletely formulated suggestion of how some scientific question might be answered. Following Robert Monk (1977), I will call such a suggestion an idea. (Gutting 1980, p. 222)

He adds, however, that the generation of the initial idea also responds to a process pertaining to $logic_2$. Thus, Gutting clearly represents what is expressed by thesis (f) in our classification.

> Now even if we grant that the initial thinking of a hypothesis need not be a matter of logic in the sense of being the outcome of an argument ($logic_1$), why does it follow that it is not a process suitable for

epistemological analysis ($logic_2$)? (*Ibid.*, p. 223)

The replacement of the classical dichotomy by a more subtle differentiation – an idea that is reflected in thesis c) of the taxonomy that we present – is clearer in Thomas Nickles' proposal. Nickles conceives of scientific activity as a process that includes three stages: generation, pursuit and acceptance. These three stages constitute phases of the discovery process. Generation refers to an individual process, while the pursuit stage corresponds to the consideration of the new proposal by the scientific community, although its acceptance is not assumed. The latter, that is, legitimization, is posterior. But the disconformity of Nickles with the classical conception does not imply, simply, recognition of an intermediate stage between the emergence of an idea and the acceptance of the theory by the scientific community. Nickles suggests that scientific activity combines creativity with consideration of justifying aspects, but he appears to believe that it is not possible to specify the scope of each one of these factors.

> Thus discovery includes justification; but equally, justification includes discovery, especially if justification is extended to include low level evaluation and plausibility assessment on one side and the interpretation and understanding of "justified" results on the other (topics neglected by the positivists). (Nickles 1980, p. 10)

In a recent article, "Deflationary Methodology and Rationality of Science" (1996), Nickles expounds upon this intermediate stage, referred to as "pursuit" or "preliminary evaluation", and for which he reserves the name, "heuristic appraisal". In contrast to "epistemic appraisal", pertinent to empirical testing and, therefore, final justification, "heuristic appraisal tells us which of the novel ideas produced by the 'discovery' phase are worth pursuing, that is, worth testing and developing..." (Nickles, 1996, pp. 27-28). Heuristic appraisal does not question the truth of a theory, but rather to what degree the theory is promising. The emphasis is not placed on the final product, but rather on the process, on the real scientific practice, on the so-called external factors that condition the activities of the scientists. Thus, Nickles advocates a more permissive methodology, a "deflationary methodology" in his own words, and he resumes along the path initiated by representatives of the historical philosophy of science.

In any event, the role of creativity and of non-rational factors in discovery are much more manifest in the case of Kuhn. But it is worth noting that in spite of the notorious opposition of this author to the classical conceptions, he recovers, in his own way, the distinction between discovery and justification. As indicated by Lamb (1991), certain references by Kuhn regarding the emergence of a new paradigm are similar to the expressions formulated by Koestler with respect to scientific discovery.

> [...] the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. What the nature of that final stage is – how an individual invents (or finds he has invented) a new way of giving order to data now all assembled – must here remain inscrutable and may be permanently so. (Kuhn 1962, pp. 89-90)

Kuhn recognizes that there would be a difference between the emergence of an original idea as the result of a new way of ordering known information, on the one hand, and establishing that the very idea effectively constitutes a solution, on the other hand. However, inasmuch as both aspects compose a unique *gestalt* phenomenon, the position of Kuhn draws close to that maintained in thesis d). In addition – although Kuhn always refused to recognize it – in the period of *The Structure*, he stressed the influence of non-rational factors to which would correspond, within the traditional scheme, the context of justification. And in *The Essential Tension*, referring to the well-known problem of choosing between two theories, Kuhn highlighted that the questions that are pertinent to the context of discovery – the consideration of the subjective factors – are also pertinent to the context of justification. In a certain sense, then, Kuhn appears to also adhere, in some way, to thesis e). Many of his later clarifications, however, approximate that which is expressed in thesis f), that is, although it is not possible to draw a clear distinction between discovery and justification, nonetheless, both processes occur as part of a rational whole. In effect, Kuhn rejected the objections of the critics who felt that his conception presented science as an irrational enterprise. Although he did not specify his notion of rationality, it is clear that he understood the concept in a very broad sense; so broad that it would exceed all possibilities of speaking of a logic of discovery.

With a much more provocative attitude than that of Kuhn, Feyerabend explicitly denied that scientific activity is a rational enterprise. But he not only rejected the discovery-justification dichotomy, he also assumed the defense of a decidedly irrationalist position.

> 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 hypothesis, and appeal to prejudices of all kinds. We need these 'irrational means' to uphold what is nothing but blind faith. (Feyerabend 1975, p. 154)

According to Feyerabend, science exists and could arise due to the fact that the methods of criticism and proof, pertaining to the so-called context of justification, were set aside in favor of the irrational elements attributed to the context of discovery. His fundamental intention is to deny the rationality of the supposed mechanisms of justification, so that it is the second term in the classical dichotomy that is totally annihilated. And in the measure to which we find ourselves with one sole dominion of procedures, all of which are non-rational, the traditional distinction, then, is annulled. Thus, in the proposed taxonomy, the position of Feyerabend exemplifies thesis g).

V

The presentation of the different alternatives that we have mentioned above would appear sufficient to risk making a balance of the situation. If it is true that a crucial aspect of the problem resides in the rationality of the procedures used in arriving at a scientific discovery, the position adopted by representatives of the classical conception is clear. As they identified rationality with logic, and thought that no set of logical rules would permit totally explaining the emergence of a scientific idea, they concluded that discovery is not a rational activity. This manner of confronting the problem has a major defect: it leads to thinking that the process of discovery is totally non-rational or, what is worse – in view of the connotations of the term – an irrational process. It is disturbing, in effect, to admit that the most outstanding activity of scientists should lie outside the sphere of rationality. Furthermore, it is hardly plausible

that the elaboration of new scientific hypotheses should depend exclusively upon sudden and inexplicable occurrences or chance circumstances. The efforts of some critics of the traditional conception, as in the case of Hanson, Musgrave and Reichenbach himself, have placed in evidence that within the complexity of the process of discovery there is also the participation, at least, of factors that could be considered to be of a logical character in a sufficiently restricted sense. Thus, while the affirmations of Popper and Hempel tend to ignore the participation of logical resources in the discovery process, those who are opposed to this – leaving aside attitudes such as those adopted by Feyerabend and Kuhn in their early periods – either highlight the role of the logical mechanisms and minimize those aspects that are not reducible to these mechanisms, or try to manhandle the concept of logic so that in some way it will include them.

The criticisms formulated in opposition to the classical position do not lack, then, a certain reasonableness. If we understand that discovery includes the appreciation on the part of the scientist for the plausibility of an idea, there is no reason to negate that they might merge in certain logical relations with previous observations or knowledge. Reichenbach alludes to these relations when he maintains that the great scientific discoveries arise from the extraordinary capacity of their authors for grasping the inductive relations of a new hypothesis with elements of available judgement. Also, the explicative relations contemplated in the concept of abduction proposed by Peirce and Hanson may be considered logical. But it is necessary to highlight that not one of these philosophers may, in all justice, attempt to reduce discovery to the application of such resources. As we have seen, Reichenbach, in effect, admits an irreducible element to logical analysis – and possibly to psychological research. And, with respect to abduction, it can only operate within a set of hypotheses that are present in the mind of the scientist; but abductive reasoning does not account for the mode in which the types of hypotheses are generated among which a choice must be made. Notice that Peirce himself is aware of this circumstance. In spite of having introduced the concept of abduction, he revealed that he had a clear conscience of its limits when he indicated that the capacity of scientists to imagine reasonable hypotheses exceeded the general powers of reason and he compared it to the instinctive faculties of animals (Peirce 1960, p. 5,173).

Similar reflections may be formulated with respect to Musgrave's

proposal. It is certain that scientific discoveries could be reconstructed according to deductive guidelines if the principles to which they apply could be successfully identified. But this does not explain, by any means, the way in which such principles came to be considered by scientists.

Nor does this type of difficulty appear to be resolved with the replacement of the traditional discovery-justification dichotomy by means of the introduction of concepts such as "preliminary evaluation", "pursuit", or "heuristic appraisal". In all cases, the procedures that determine the convenience of continuing to develop a new idea take for granted an initial generation. And this is precisely the point that would remain unexplained and that led Popper and others to think that discovery could not be analyzed in logical terms. We insist that this problem cannot be overcome by proposing a new and broader concept of logic. We would say, in passing, that whoever formulates this proposal, such as Gutting, appears to take advantage of a rhetorical resource in order to argue in favor of the rationality of discovery. Broadening the concept of logic is not a great help unless the rules for such can be identified. It is not useful, for example, to maintain the existence of a broad logic pertaining to discovery comparing it, as does Gutting, to a supposed logic of art or other human activities.

In contrast to the authors that are disposed to recognizing the role of logic in a restricted sense in the process of discovery, such as Reichenbach or Musgrave, those who defend a broader sense do not succeed in specifying its content in an acceptable and consistent manner. We have already indicated the difficulties involved in the concepts of the logic of generation and the logic of prior assessment in the case of Curd. In addition, the resource of appealing to a logic of preliminary evaluation creates a suspicion that there is an attempt to introduce logical factors into discovery through the inclusion of aspects that orthodox authors would surely consider as pertaining to justification. We may recall that Popper himself, with respect to the empirical testing of a theory, previously described certain procedures that could be considered, as a whole, a species of preliminary evaluation:

> First there is the logical comparison of the conclusions among themselves, by which the internal consistency of the system is tested. Secondly, there is the investigation of the logical form of the theory, with the object of determining whether it has the character of an

> empirical or scientific theory, or whether it is, for example, tautological. Thirdly, there is the comparison with other theories, chiefly with the aim of determining whether the theory would constitute a scientific advance should it survive our various tests. And finally, there is the testing of the theory by way of empirical applications of the conclusions which can be derived from it. (Popper 1959, p. 33)

In the case of Curd, this maneuver is evident insofar as it only leaves outside of the discovery stage deduction and verification of new predictions.

As we indicated at the outset of this paper, a large part of the complexity of the discussion is owing to the circumstance that the participants in the debate do not tend to clearly distinguish the differences that subsist between the different theses submitted for discussion. A symptom of this is that often the problem is formulated in terms that question the distinction between the context of discovery and the context of justification. These are the expressions coined by Reichenbach and that do not appear, for example, in the original discourse of Popper, but they have come to be utilized for characterizing what we have called the orthodox conception; thus, they obscure not only the peculiarities of Reichenbach's thinking, but also the differences he maintained with Popper and Hempel. Another symptom is the tendency to exaggerate the contrasts between the ideas of the classical authors and the new currents in the philosophy of science. We recall, in this respect, that Hanson is often cited as one of the pioneers of the reaction against orthodoxy without duly recognizing the fact that his defense of abduction as a method pertaining to discovery requires accepting the classical distinction between discovery and justification. And we have also seen that Kuhn could not avoid, on some occasions, describing discovery with words that imitate those used by classical authors.

The course we have followed permits the formulation of certain conclusions. We do not deny that the problem of discovery comprises the legitimate field of philosophical interest. And, as we do not share the ideas of Feyerabend, we do not believe that discovery and justification of scientific knowledge essentially develop because irrational elements emerge triumphant. We also admit that in real scientific activity certain evaluative aspects can appear jointly and simultaneously with the discovery of new hypotheses. This does not mean, in our judgement,

ignoring the conceptual difference between discovery and justification. At the same time, it appears to us that none of the diverse attempts to reconstruct the mental mechanisms that operate in discovery succeed in explaining all the ingredients involved. We suspect that there will always remain an impregnable element against any type of logical analysis. Upon this base, we believe that what is most probable is that in the process of generating a hypothesis or a scientific theory there may operate inductive, analogical, or deductive rules. And in this aspect it seems to us that a rational attitude is manifest. However, insofar as the application of such rules is not exempt from the presence of a creative contribution independent of those rules, the formulation of a logic of discovery in an absolute sense will remain elusive.

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