Heuristic, Methodology or Logic of Discovery? Lakatos on Patterns of Thinking

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Heuristic is a central concept of Lakatos' philosophy both in his early works and in his later work, the methodology of scientific research programs (MSRP). The term itself, however, went through significant change of meaning. In this paper I study this change and the 'metaphysical' commitments behind it. In order to do so, I turn to his mathematical heuristic elaborated in Proofs and Refutations. I aim to show the dialogical character of mathematical knowledge in his account, which can open a door to hermeneutic studies of mathematical practice.

Heuristic or Methodology?

The subtitle of Lakatos' *Proofs and Refutations* is: *The Logic of Mathematical Discovery* (Lakatos, 1976). Although in the original version published in 1963 in the *British Journal for the Philosophy of Science* there was no such subtitle, it was not the invention of the editors. The unpublished 1961 Cambridge Ph.D. thesis of Lakatos has the same title ("Essays in the Logic of Mathematical Discovery"), and it was an early version of *Proofs and Refutations.*—But was it really a logic of mathematical discovery? And if it was, in what sense?

There is a footnote in "History of science and its rational reconstructions" (1970), a paper Lakatos wrote almost ten years later, in which he made the distinction explicit: heuristic means the rules of discovery, while logic of discovery or methodology makes the rules for appraising already

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existing results of science (Lakatos 1978a, p. 103). As Teun Koetsier pointed out, there was a change in Lakatos' use of the term 'heuristic'. While heuristic in the *Proofs and Refutations* was a set of rules to guide problem solving for the individual scientist, "MSRP [*Methodology of Scientific Research Programmes*] does not give any firm heuristic advice to individual scientists, but it definitely yields recommendations for a rational scientific community on the way it should act" (Koetsier 1991, p. 16). Although 'heuristic' is a central concept of the "Methodology of Scientific Research Programmes," there are very few explicit hints in MSRP what heuristic means exactly. Lakatos makes vague statements, such as, heuristic "tells scientists" things to do or not to do, but these statements remain very obscure—he does not show a real, elaborated heuristic. This is why, I claim, this late work cannot be fully understood without his early 1963–64 papers; his intentions become clearer, especially after we scrutinize his *Proofs and Refutations.*

1. Dialogue in the "classroom"

In his *Proofs and Refutations* Lakatos reconstructs a vivid debate among mathematicians. The genre of the paper is moral drama written with an excellent sense of humor. The actors of Lakatos' play—a teacher and his students—are in an imaginary classroom. In their talk, a fictional dialogue of mathematicians of different eras unfolds. Students do not represent particular mathematicians, but rather customs and typical behaviors of scientists faced with the coexistence of alternative proofs and refutations to the same theorem: Which one is wrong, and what is the problem with it? The actors are emotional, sometimes even passionate, enthusiastic, suspicious or desperate, but always witty. We can also see typical changes of their habits.

The students' names are Greek letters, to express their cross-cultural individuality. Each time they speak as someone in the 'real' history of mathematics, we can find an exact reference to the primary sources in the footnotes. In this way one can say, the 'real history' runs in the footnotes, while the dialogue in the classroom is its 'rational reconstruction'.

Science begins with problems.[1] The students in Lakatos' "classroom" become interested in the problem at hand: is there a relation between the number of edges, vertices and faces of a polyhedron? They find a conjecture (today it is known as the Euler theorem), and the teacher proves it for

1. At least in the Popperian logic of scientific discovery. Lakatos shows in this work the applicability of a 'sophisticated version' of Popperian methodology applied to mathematics. I borrow the term of 'sophisticated falsificationism' from his later papers (see Lakatos 1978a).

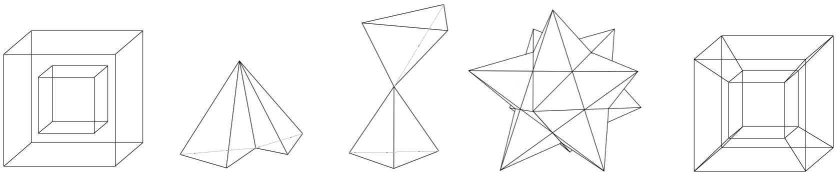

Figure 1. Nested Cube, twin-tetrahedra, "urchin" that is star polyhedron, "picture frame."

all polyhedra. The story could stop here, if the students were not advanced enough to ask questions. Is the proof correct? Is the theorem really valid? The first meta-problem—how to criticize mathematical arguments—already arises by these steps. Suspicion alone is not criticism, but enough to make the *Teacher* modify his proof slightly. In order to make the criticism much tougher, the *Teacher* suggests that the students start looking for counterexamples. Some of them attack the conjecture (which can be seen as a theorem after having a proof), while others attack some steps of the proof. What to do next?

The first counterexample, given by *Gamma,* makes the *Teacher* modify his proof, after which he says "Criticism is not necessarily destruction"! (Lakatos 1976, p. 10) He finds the guilty lemma that is falsified by this counterexample, and reformulates it in order to correct the proof. The second counterexample, found by *Alpha,* is a cube-within-a-cube; creating a cavity (Figure 1a—"nested cube") undermines not only the proof but also the theorem itself. The *Teacher* would have to surrender, but he does not.

At this point the debate expands. *Gamma* rejects the defense of the proof, while *Beta* rejects the counterexamples. According to him 'nested cube' is not a counterexample, because it is not a polyhedron at all. The students start to create definitions for the term "polyhedron" (which was originally treated as self evident) in order to defend the conjecture by excluding such objects from the field of polyhedra. In a very short time we find ourselves in a jungle of different definitions and crazy, perverted, and foolish counterexamples.

Are these really polyhedra? They are rejected as monsters by *Delta* and *Eta* in defense of the proven theorem. More and more new counterexamples arise to the increasing number of definitions and then again, new monster-barring definitions. *Beta* accepts all the counterexamples as polyhedra, he simply adjusts the theorem to them by excluding them as exceptions. When the theorem has too many counterexamples, he restricts its validity to a safe territory of convex polyhedra. Convex polyhedra must be

Euler-like. But how can we know?—*Beta* did not do anything with the proof. Does it prove the new formulation of the theorem? Who knows?

At this point *Rho* does something interesting: she does not change the definition of the polyhedra, but she *re-interprets* the counterexamples. She changes our way of seeing them. According to her, "urchin" is not a star-polyhedron with star-pentagonal faces but a simple, boring (although concave) polyhedron with triangular faces. "Monsters don't exist, only monstrous interpretations" (Lakatos 1976, p. 31). Lakatos refers here to the Skeptic criticism of the Stoic claim for the ability distinguishing '*phantasia*' ([mental] representation) from '*phantasia kataleptike*' (apprehensive [mental] representation) (Lakatos 1976, p. 32 n. 2), but *Alpha* considers this monster-adjusting attitude as simply brainwashing.

All of these struggles, however, seem to be 'much ado about nothing'. (And, of course, we can follow the bouts of real mathematicians in the footnotes.) The question, whether the conjecture is true or not, remained unanswered. I claim that the quarrel and the counterexamples had nothing to do with the theorem and its proof. This is the point when the *Teacher* introduces us to the *method of proof and refutations.* Faced with the counterexamples, he carefully inspects the challenged proof, because he now thinks that some of the lemmas must have been false. After making a thorough analysis, he incorporates these lemmas into the theorem as presuppositions. In this way the theorem is not about 'all polyhedra', but about 'simple polyhedra' with 'simply connected faces'. He now defines the terms 'simple polyhedron' and 'simply connected faces' in order to secure the previously attacked steps of the proof. These definitions cover theoretical concepts stemming from the proof-analysis, the final source of which was the debate about the counterexamples. This method of lemma-incorporation shows *the "intrinsic unity between the 'logic of discovery' and the 'logic of justification' . . ."* (Lakatos 1976, p. 37)

This "classroom dialog" could be seen as a socio-psychological account of the development of mathematics, but it is not so. All of the controversies become relevant to such a 'rational reconstruction', when we start to see their cognitive relevance, and the resulting concepts and trains-of-thought "frozen" in the papers, letters and textbooks. At that point these thoughts can be studied as 'objectified knowledge' (a "what"), as that which is alienated from its creator, and has its own laws.

But not so fast!

2. Patterns of thinking

In Lakatos' early papers, "logic of discovery" was a synonym for "heuristic" as well as for "methodology." As he put it: "I use the word 'methodology' in a sense akin to Pólya's and Bernay's 'heuristic' and Popper's 'logic of discovery' or 'situational logic'." (Lakatos 1976 p. 3)[2] His aim was to describe the patterns of thinking, the way of growth of knowledge. At that time all the subjects mentioned above were about the same topic for him. He studied mathematical activity, and discovered patterns of thinking one can find in the history of mathematics, as well as in individual mathematicians' thinking. *What makes the connections between an individual's work and the history of science is the debate itself.* For an individual mathematician thinking can be seen as "the dialog of the soul with itself." Just like in Pólya's heuristic, where one can put a question to oneself to catalyze the process of problem solving. Problem solving, driven by heuristic rules, can be a sort of "inner dialog." According to Lakatos, contemporary knowledge also grows in the discussion of mathematicians. And in a similar way, the growth of mathematics in history is like a great debate spanning several epochs. The participants of this debate live in different times or places; sometimes they do not even know one another. This search for patterns of thinking creates a rational reconstruction of the real historical debate. It happens in a "platonic heaven," or, in Popperian terms, in the third world. It is almost independent of what was happening in real history: The expressions or real reactions of working mathematicians only give occasions for Lakatos to find these patterns. They are not simply personal attitudes, they can be seen as patterns that we can learn, follow or avoid as well as improve. All of us can behave in these ways, and we actually do so. As Lakatos mentioned: "The student of recent history of metamathematics will recognize the patterns described here in his own field" (Lakatos 1976, p. 5). I think that the rational reconstruction, or "distilled history" has a moral for any field of scientific inquiry. These patterns are content independent. Methodological rules, if you like, that we can use not only to create but also to attack or defend theories.

Later on Lakatos distinguished these roles. Positive heuristic contains rules that help us in application, in the formation of a fallible version of the hard core—in the creation of the protective belt of auxilliary hypotheses. (The method of proofs of refutations as explicated in the *Proofs and Refutations* can be a fruitful part of any positive heuristics.) The role of negative heuristic is simply to defend the hard core—maybe in a less creative way. As Teun Koetsier mentioned—referring to a paper of De Vries[3]—"The method of monster-barring or the method of exception-

2. Lakatos himself has a detailed account of in what sense can his philosophy be seen as belonging to a research program grown out of Popper's logic of scientific discovery (Lakatos 1978a). I have already discussed elsewhere how far his conception can be treated a successor of Pólya's heuristic (Kiss 2002).

3. (De Vries, 1981) cited by Teun Koetsier (Koetsier, 1991, pp. 67–68).

barring are both quite suitable for defending a hard core" (Koetsier, 1991, p. 68). We will see later, how—and why—methodology became separated from both of them.

3. Dialectic of mathematics

According to John Kadvany *Proofs and Refutations* is a Hegelian romantic "Bildungsroman" ("educational-cultural novel") (Kadvany 1989), deeply rooted in the German philosophical tradition. As Kadvany pointed out, dialectical reasoning is not only a 'stylistic ornament' in Lakatosian philosophy, it shows us an educational process, the intellectual adventure of the Mathematical Spirit, just like Hegel's account in the *Phenomenology of Spirit.* When he contrasts the deductivist and the heuristic approaches to mathematical results, Lakatos introduces the mathematical concept of uniform convergence using Hegelian terms: *thesis* (primitive conjecture)—*antithesis* (counterexamples)—*synthesis* (improved theorem and proof-generated concept) (Lakatos 1976, pp. 144–146). In another example of using Hegelian terms, writing about the foundations of mathematics he says: " . . . 'Certainty is never achieved', 'foundations' are never found—but the '*cunning of reason*' turns each increase in *rigour* into an increase in *content,* in the scope of mathematics" (Lakatos 1976, p. 56) [my emphasis].

Let us look closely to see what he was speaking about:

> Mathematical activity is human activity. Certain aspects of this activity—as of any human activity—can be studied by psychology, others by history. Heuristic is not primarily interested in these aspects. But mathematical activity produces mathematics. *Mathematics,* this *product* of human activity, 'alienates itself' from the human activity which has been producing it. It becomes *a living, growing organism, that acquires certain autonomy from the activity which has produced it; it develops its own autonomous laws of growth, its own dialectic.* The genuine creative mathematician is just a personification, an incarnation of these laws which can only realise themselves in human action. Their incarnation, however, is rarely perfect. The activity of human mathematicians, as it appears in history, is only a fumbling realisation of the wonderful dialectic of mathematical ideas. But any mathematician, if he has talent, spark, genius, communicates with, feels the sweep of, and obeys this dialectic of ideas. Now *heuristic is concerned with the autonomous dialectic of mathematics* and not with its history, though it can study its subject only through the study of history and through the rational reconstruction of history.(Lakatos 1976, p. 146—my emphasis)

This looks like an idealist rationalist approach to mathematical thinking. The patterns of mathematical development are objective; they seem to be almost completely independent of the human mind. This sheds a different light on Lakatos' joke: "My respectable historian colleagues sometimes say that the sort of 'rational reconstruction' here attempted is a caricature of real history; but one might equally well say that real history is a caricature of its rational reconstruction" (Lakatos 1962, p. 157).

His aim is, however, not pseudo-history. Neither is he interested in historiography, but in finding the essence of arguments. The aim of his "rational reconstruction" is to find the best arguments, and to formulate the *patterns of thinking.* Indications of that attitude can be his several references to primary sources saying nothing about the Euler theorem. That is, the primary sources did not take part in the debate around the Euler theorem explicitly. Lakatos refers to them, because his main interest is tied to the usual controversies. And it can also explain why chronology in such a historical debate can be irrelevant. Use of historical sources only makes it possible to discover something more important to him. Of course, others can say that these patterns are irrelevant *because* they are inadequate to describe *real* historical changes (or growth) of science. But Lakatos never intended to be a defender of pure historiography. According to him, the aim of philosophy of science is not *to describe* real occurrences. Its aim is *to appraise* these occurrences. It was his aim already in *Proofs and Refutations:* some of the patterns he described were ridiculous, and some of them were treated as fruitful and worth following. The real descriptive history of science is as irrelevant to this as psychological description of a great scientist's thinking. I claim, this may be how he came to turn from heuristic to methodology, as I show below.

Finding "the essence" of the arguments requires an interpretive-evaluative process. One has to select what is relevant to the subject and what not; what depends on it, and what belongs to the speaker's subjective personal (historical, socio-cultural, etc) background. As far as I see, the main achievement of Lakatos' philosophy is showing that scientific thinking is fallible, but not because of such "subjective" ties. Informal mathematical thinking in its ideal form is not final-deductive, but fallible-dialectical. As long as informal mathematics exists, even in the coexistence of fully formalized theories, mathematics is a living and changing subject. Not because the mistakes mathematicians make, but because of the laws (or patterns) of thinking.

"How do we think?"—is too "soft" a question, and the possible answers are on too large a scale. "Which are the best patterns of thinking?"—is a much more interesting question for Lakatos. Description alone is weak. Description, after evaluating and selecting the best strategies, is much more exciting and fruitful for future generations. The difference lies in the evaluation process,—and Lakatos started to focus on it after the mid-sixties.

One more remark: Lakatos never thought that the methodology of proofs and refutations would be universal, as some critics supposed (Kvasz 2002). As far as I see, he did not pretend that there would be no other patterns of mathematical growth—either in the sense of heuristic rules or in the sense of changes in the development of mathematics in a historical perspective. Lakatos explicitly stated at the end of the originally published version[4] of "Proofs and Refutations" that the "*revolution in mathematical criticism changed the concept of mathematical truth, changed the standards of mathematical proof, changed the patterns of mathematical growth.*"(Lakatos, 1976, pp. 104–105—italics in original) Lakatos gives us no heuristic rules for that new period of growth. He described, however, the basic patterns of mathematical thinking of the new situation: it was published as the second part of *Proofs and Refutations,* "The problem of translation" (Lakatos 1976).

The quotation I mentioned above is enough reason for me *not to agree* with Kvasz when he says: "If Lakatos had lived longer, he would have applied his arsenal of heuristic strategies to this material, as well" (Kvasz 2002 p. 218). I think, instead, that if he had more time to study that problem, he would have written more on the translation problem, that is, about the problem of interpretation in mathematics. Translation and interpretation constitute the central processes dealing with an informal problem, when one already has well-formulated theories to work within. "The translation procedures are vast reservoirs of problems, historical trends which represent huge patterns of thought at least as important as the Hegelian triad" (Lakatos 1976, p. 125 n. 1). And it shows that his approach to dialectic does not mean to accept a small set of schemes given by Hegel. Dialectic is meant much more in a classical sense, where the dialogue and the debate are the basic sources of knowledge. Hegel is a good source for Lakatos, because he emphasizes the positive role of contradiction in the development of knowledge; "Hegel's philosophy offered . . . a radical break with its infallibilist predecessors . . . " (Lakatos 1976, p. 139 n.1) But I deeply agree with Kvasz in that the study of mathematical development is not a finished project after the *Proofs and Refutations.* What Pólya and Lakatos have only started, others can continue by describing new and fruitful routes to (not only mathematical) discovery or patterns of

4. Only the first chapter of (Lakatos 1976) was published in he *British Journal for the Philosophy of Science,* but the second chapter, a Euclidean reply to a challenge to produce a proof of the Descartes-Euler conjecture, was also a part of his PhD thesis.

historical development of science. It was done, among others, by his students and friends: John Worrall (Worrall 2002) and Elie Zahar (Zahar 1989).

In any case, Hegelian dialectic forms were not clichés to Lakatos. Patterns of thinking in the dialectic of mathematics are not prescribed. Although Lakatos speaks about laws, they are not unchangeable ones, once and for all fate-given ones. There are laws of the growth of objectified or "alienated" knowledge (as mentioned above), but "human activity can always suppress or distort the autonomy of alienated processes, and can give rise to new ones" (Lakatos 1976, p. 146 n.1).[5] The *Bildungsroman* ("educational-cultural novel") as well as any other form of history is written in hindsight. It is about the process of how mathematics achieved its present state. It differs from the well-known Whig history in its guiding methodology—it does not presuppose linear development. John Kadvany emphasizes that *Proofs and Refutations* was "one of the earliest, most radical, and ingenious twentieth-century philosophical texts to actively exhibit the theory-ladenness of historical inquiry" (Kadvany 1989, p. 33), and I completely agree with this appraisal at least in the field of history of mathematics.

4. A note on scientific achievements

Here, I would like to stress one more aspect of the Lakatosian account that seems to escape the notice of critics and followers. I mean the "locus," the place of generation of scientific achievements. If we accept that history of science is just like a great (perhaps rational) discussion among scientists of different eras, his dialogue in the classroom could be seen as a model of creating knowledge by "thinking together." Of course, all scientists think individually, but their opinions and statements fertilize each other's

5. In the original context Lakatos refers to both the Hegelian and the Marxian concept of "alienation": "This Hegelian idea of the autonomy of alienated human activity may provide the clue to some problems concerning the status of methodology of social sciences, especially economics. My concept of the mathematician as an imperfect personification of Mathematics is closely analogous to Marx's concept of the capitalist as the personification of Capital. Unfortunately Marx did not qualify his conception by stressing the imperfect character of this personification, and that there is nothing inexorable about the realisation of this process. On the contrary, human activity can always suppress or distort the autonomy of alienated processes and can give rise to new ones. The neglect of this interaction was a central weakness of Marxist dialectic." (Lakatos, 1976 p. 146 n.1).

Here we can see how far Lakatos became alienated from his original Marxist views. While his first dissertation published in Hungary—as Kutrovatz pointed out (Kutrovatz 2002, p. 355)—was completely under the influence of Marxist philosophy, by the time he wrote *Proofs and Refutations* he turned back to the original Hegelian version of dialectic.

thinking. They create a world with new phenomena (either accepting or rejecting them), and theorize in new ways. Seeing the heuristic of science in that way, it can constitute rules not simply for the individual scientist, but also for the scientific community.

Dialectic in Hegel's view, as well as in Lakatosian philosophy, is driven by criticism. Objection, however, is only one opportunity to take part in the discussion. Agreement and questioning are as important as the effort to understand each other. It may result in much more complex "laws" or patterns of thinking, but as I see it, it could be closer to a fruitful way of thinking together.

In such an approach *the essence of scientific thinking is neither the methods of proof, nor the ways of discovery, but rather their common basis—the dialogue of scientists about their experiences and truth.* Science is intrinsically a public activity. The locus of scientific rationality therefore is not necessarily the individual scientist—it is the community as well (with or without its spatio-temporal limits). Logic of discovery must work within such a community. Discoveries are not the exclusive achievements of individual scientists. The community also works out these results by means of its members (or sometimes outsiders).

In this process, understanding and interpretation play a decisive role.

5. The role of interpretation in Proofs and Refutations and in MSRP

> "*For any proposition there is always some sufficiently narrow interpretation of its terms, such that it turns out true and some sufficiently wide interpretation such that it turns out false. Which interpretation is intended and which is unintended depends of course on our intentions*" (Lakatos 1976, p. 99).[6]

Is this really true? Can you imagine that Lakatos, who defended his critical-rationalist position so passionately, and attacked the philosophies of Kuhn, Toulmin, Polányi and Wittgenstein with no mercy, could accept, that there is no truth in sentences—that truth or falsity simply depend on interpretation? In the following sections I shall argue, that he did. After enumerating the results of debates in nineteenth century mathematics about the problem of foundations, his critics and all his words against his colleagues have to be seen in that perspective. *In spite of his severe criticism of elitism, pragmatism and collectivism, he did not think that these were simply false*

6. The next sentence of the paragraph is: " . . . The first interpretation may be called the *dogmatist, verificationist* or *justificationist interpretation,* the second the *sceptical, critical or refutationist interpretation*"(ibid—emphasis in original).

theories. In philosophy of science we are in the same situation as the actors in Lakatos' "classroom dialog": "*Why not accept that our ability to specify what we mean is nil, therefore our ability to prove is nil?* If you want mathematics to be meaningful, you must resign of certainty. If you want certainty, get rid of meaning. You cannot have both. *Gibberish is safe from refutations, meaningful presuppositions are refutable by concept-stretching*" (Lakatos, 1976 p. 102—emphasis in original). He was fully aware of the importance of the influence of scientific elite, the relevance of praxis in reasoning, and—as I tried to show here—the role of scientific community in the elaboration of theories. His intention was however not to go very far in those directions. It was his theory choice.

The above quotation shows that Lakatos was fully aware of the fallibility of reasoning. The usual understanding of terms, propositions and theorems gives truth-value to them against the background knowledge achieved by that time. This is well known from his later papers: theories are never tested alone. All experiments and experiences are "theory-laden", and it depends on the interpreter's intention, whether to attack the theory (the hard core of the program) or the circumstances (auxiliary hypotheses) (Lakatos 1978a).

The same can be found in his early paper about mathematics.—Let me turn back briefly to the original story in the classroom! *Being confronted by mistakes, mathematicians may discover 'hidden lemmas' in their arguments which were so trivial that it seemed to be unnecessary to state them explicitly. The 'correct interpretation' assumes these hidden lemmas as background knowledge.* But as *Kappa* notes ironically: "Background knowledge is where we assume that we know everything but in fact actually know nothing" (Lakatos 1976, p. 37). To make this knowledge explicit, we have to go deeper and deeper into the background of our implicit presuppositions. By making the hidden lemmas explicit, we interpretatively understand something we already knew. The dynamics of these hermeneutic circles is stimulated by the debate, the critics, and the counterexamples.

Here, as in the case of mathematics earlier, the question arises: where to stop criticism and start justification? I claim that, according to Lakatos, it is up to one's decision and the historical situation. There is no infallible basis of knowledge. Criticism of Kuhn, Polányi, Toulmin, and Wittgenstein was his choice, and, I'm sure, he was completely aware of the validity of their truth-claims. For him, the passion of his criticism of them stemmed from his passion for clarity in (philosophy of) science.

There is no absolute rigor, absolute truth or final basis of knowledge. Not only theories, but even the ways of reasoning—whether logical or heuristic—are subject to change.

6. Historicity of heuristic and logical rules

When we look for patterns of thinking, we can use several sources. What are really important, however, are not all the ways of thinking, since there are also mistakes, faults, errors, and Lakatos does not intend to study these. He is interested only in good, rational, productive scientific thinking. Therefore he first has to separate good steps from mistakes, fruitful thinking from dead-ends. Then he can formulate descriptive rules of heuristics. But soon, for future scientist, these rules could become normative: good ways that they have to follow. However, Lakatos is too radical a thinker to accept such a standpoint. He is revolutionary in the sense, that he holds that one can always reformulate one's theory. (He is a critical rationalist.) That is, heuristic rules are not obligatory, and "there is no infallibilist logic of scientific discovery, one which would infallibly lead to results" (Lakatos 1976, p. 143 n. 2). Heuristic rules are those that are usually worth following—sometimes they work, sometimes they don't. There is room for methodological invention.[7]

Logical rules are also subject to change—they are also fallible. When Lakatos discusses the problem of changing patterns of inference in the nineteenth century, he notes:

> "The modest stretching of 'all' by removing 'existential import' from its meaning and thereby turning the empty set from a monster into an ordinary *bourgeois* set was an important event—connected not only with the Boolean set-theoretical re-interpretation of Aristotelian logic, but also with the emergence of the concept of vacuous satisfaction in mathematical discussion" (Lakatos 1976, p. 104. n. 2).—That is, the rules of logic are not necessarily valid forever. And declaring that for now we have achieved the final rigor in history, is simply funny for him.[8]

If logical rules are subject to change, what can we say about informal inferences? They are also important for him, because Lakatos intends to speak about science as it is being made. He wants to write about mathematics in the process of birth and not as frozen into axiomatic systems. Though he is always interested in theories, concepts, examples and counterexamples that seem like they are part of the Popperian third world,

7. Incidentally, the method of proofs and refutations was discovered not by Lakatos, but by P.L. Seidel in 1849, and made it possible for him to formulate the concept of uniform convergence (Lakatos 1976, p. 50 n. 1 and p. 136)

8. When one of the participants in the classroom says: "Today absolute rigor is attained."—He adds: "Giggling in the classroom",—and notes:—"(As we already pointed out, the class is very advanced)"(Lakatos 1976, p. 52 and n.3)

he does not mean *formal* representation. His subject matter is the patterns of growth of informal science, which is important to distinguish from its idealized (formal, axiomatic) counterpart. He focuses on rational discussion expressed in the usual language of science. If there is among philosophers of science someone who knows what sort of problems can stem from not differentiating mathematical expressions and their formalizations, he is the one. It is always difficult to translate a problem, a theorem or a proof into the language of an existing mathematical field, or to formalize it as a part of an already formalized axiomatic system. That means that even if we had a perfect, formalized logic, it would always be problematic how to validate our non-formal inferences by means of it.

7. Mitigated sceptic and mitigated dogmatic

From the recognition that "reason is too weak to justify itself" (Lakatos 1976, p. 54 n.2), Lakatos concludes that justification is always conjectural. He states the alternatives of dogmatism and skepticism,—and at first glance he is with the latter. "Popperian *critical fallibilism* takes the infinite regress in proofs and definitions seriously, does not have illusions about 'stopping' them, accepts the sceptic criticism of any infallible truth-injection . . . The new central question: *How do you improve your guesses?* . . . The indefatigable skeptic however will ask again: 'How do you *know* that you improve your guesses?' But now the answer is easy: 'I guess.' There is nothing wrong with the infinite regress of guesses" (Lakatos 1962, p. 165, and Lakatos 1978b, pp. 9–10).

His sort of fallibility thesis seems to be able to save him from accepting dogmatism.

Later he parted ways with the skeptics. As he wrote ten years later: "Scepticism regards scientific theories as just one family of beliefs which rank equal, epistemologically, with the thousands of other families of beliefs. One belief-system is no more 'right' than any other belief-system; although some may have more *might* than others. There may be changes in belief-systems but no progress" (Lakatos 1978b, p. 225). Lack of progress is the reason he cannot share their position. He holds that appraisal of scientific results is possible and necessary. In his reconstruction, one has two kinds of positive answers to that problem: demarcationism or elitism. His own choice here is the former one, which is a sort of dogmatism[9] (in the sense that objective knowledge is possible, although it is not necessarily infallible). This approach appraises the products of knowledge, not the

9. Koetsier tries to harmonize Lakatos' early and late approaches to skepticism, saying that the common ground is a "mitigated skepticism" (Koetsier 2002). As Kadvany pointed out, Hegel also constructs his historiography as mitigated skepticism (Kadvany 1989, p. 36).

procedures. "In the demarcationist tradition, philosophy of science is a watchdog of scientific standards. Demarcationists reconstruct universal criteria which great scientists have applied sub- or semi-consciously in appraising theories or research programmes"(Lakatos 1978b, p. 110). This aim of course belongs to methodology and not heuristic, as was mentioned above. I quote it because it reveals Lakatos' attitude: a methodology either can or cannot explain these choices of great scientists.—Methodology is also a theory, and as such, it is fallible.

His passion in criticism can be understood if we consider his credo: "*I want clear theses in both science and in philosophy of science where logic can assist criticism and help to appraise the growth of knowledge*"(Lakatos 1978b, p. 243). Not because "clear and distinct" formulation would be enough to justify the truth of these theses, but because this is a necessary condition of rational discussion, which is the essence of science in Lakatos' view.

Summary thoughts and remarks on the ways of positive hermeneutics

At the beginning—in the late fifties when he wrote his Ph.D. thesis "Essays in the Logic of Mathematical Discovery" and "Proofs and Refutations"—heuristics, methodology and logic of discovery were synonymous for Lakatos. By the time he wrote his Methodology and Scientific Research Programmes this was no longer the case. Heuristic remained the study of discovery, while methodology took the role of appraising already formulated pieces of knowledge, such as theories or steps in a research program.

In his early philosophy, Lakatos aimed to find the general patterns of mathematical thinking, which he supposed to be the same in history as in the life of the individual mathematicians: the process of improving theorems and proofs. Later he abandoned his heuristic studies, and restricted himself to elaborating his methodology that could be a ground for appraising scientific progress. That is, his methodology was made not for the individual scientist—because it has very little to say about what to do or to avoid. Methodology is a meta-theory guiding historiography and the appraisal of research programs.

The original unity of heuristics, methodology and logic of discovery however, was not baseless: both heuristics and methodology deal with the improvement of theories. Heuristic studies the patterns of thinking—these patterns can be transformed into rules which are worth following (but not necessary). It is always possible to improve the way one thinks, that is, on one's method. Methodology is also a theory (a meta-theory) that looks for universal criteria that can explain the greatest scientists' appraisals of theories. (It can be fallible, since it was designed to appraise already given theories and thoughts.) This is why methodology can serve as a guide to historiography.

Of course we also need appraisal when we intend to improve our theories. Appraisal is however clearly distinguished from heuristic—at least logically. Heuristic contains "sign-posts", while methodology tells us, even if provisionally, whether we are going in the right direction. One step in a research program can be treated as progressive or degenerating only in hindsight, when we see future developments. Appraisal of research programs is as fallible as the theories themselves.

This is the historicity of knowledge, truth, rationality and methodology in Lakatos' philosophy.

Patterns of thinking in the Lakatosian interpretation emphasize the dialectical nature of mathematical development. In his account it is driven by criticism. My aim was rather to emphasize the dialogical side that can give us an opportunity for a hermeneutic approach to mathematics. Heuristic in this respect plays a central role, because it can tell us how mathematics actually works. Instead of studying the well-formulated theories of mathematics, we can see the ways mathematicians really create their world. Psychological and sociological approaches can also be helpful, but the way Lakatos studied this field is much closer to the everyday scientists' interest. He treated the process of theory-formation as a part (or result) of a rational discussion among mathematicians. This is a good way to show why *pure mathematics can do well without hermeneutics.* I wanted to show, *however,* that *hermeneutic problems are deep inside mathematical practice,* waiting for a much more detailed and intellectually rich account.

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