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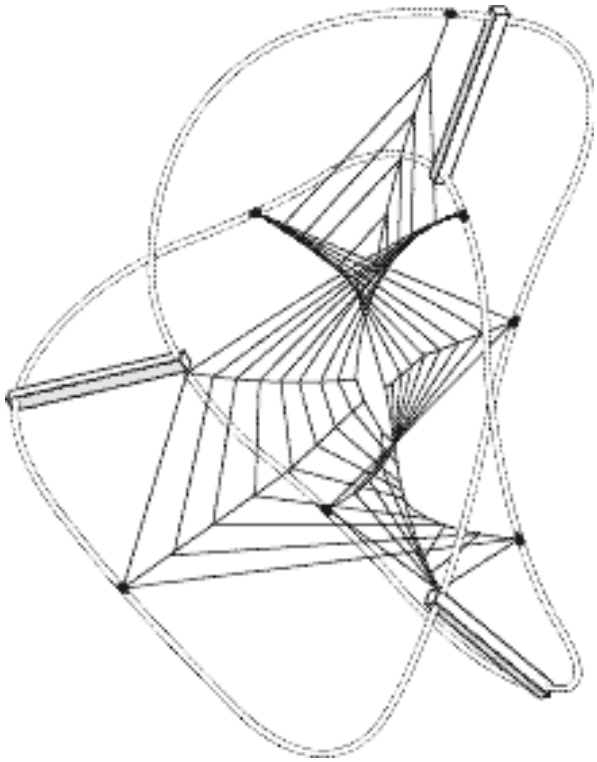

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RECONSTRUCTING LAKATOS

A reassessment of Lakatos' philosophical project and debates with Feyerband in light of the Lakatos Archive.

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You can still advertise that the Devil is coming, for the Devil chooses strange shapes to confound the innocent. For

example, he may choose the shape of a rationalist from London with a Hungarian accent.

Paul Feyerabend

1. A Pop-Hegelian philosopher

Feyerabend had claimed that his friend and fellow was a "*big bastard* - a Pop-Hegelian philosopher born from a Popperian father and an Hegelian mother." ("Lakatos-Feyerabend Correspondence", in Motterlini, ed., 1999, pp. 184, 194). In fact, some of Lakatos's most fruitful contributions - such as his dialectical conception of mathematical heuristic, the idea of rational reconstruction and, more generally, the emphasis on the role of criticism in the progress of knowledge - come directly from a peculiar philosophical conflation of Hegelian and Popperian ideas.

In this paper, however, I shall argue that, for the same reason, a tension in Lakatos's thought cannot be ultimately resolved. Like a seaman in the famous pirates' ballad caught "between the devil and the deep blue sea", that is between his captain who held near-dictatorial powers on the one side, and the dangerous boundless forces of nature on the other,[1] Lakatos too was caught between the devil of Hegelian historicism and the deep blue sea of Popperian fallibilism. This conclusion will be largely based on the material in the *Archive of Professor Imre Lakatos* at the British Library at the London School of Economics for Political and Economic Science (henceforth Archive).[2] Instead of giving a complete description of the available

[1] On the seaman's dilemma, see Rediker, *Between the Devil and the Deep Blue Sea*, Cambridge, 1987, p. 5.

[2] Lakatos' s Archive was listed by Michael Hallet in 1979 and updated by Sue Donnelly in 1995. It is arranged in the following thirteen sections: 1. Papers published in Hungary. 2. Early Notes on

material, I shall emphasise those items which illustrate most clearly Lakatos's method of "Proofs and Refutations", his revision of Popper's falsificationist approach, the shift that occurred in the conception of methodology from his early writings to his later papers, his criticism of the neoauthoritarian philosophies such as Toulmin's, and finally his struggle to defend "Reason" against Feyerabend's neo-sceptical challenge.

2. A critical Marxist polemicist

The contributions by Lakatos for Hungarian literary and academic journals in the early Fifties show how Lakatos's enquiry into science, mathematics, history and method has always been firmly linked to *pedagogical* and *socio-political* issues. The Hegelian-Marxist influence is clear in the paper entitled "'Le Citoyen' and the working class" (Archive 1.1). Lakatos contrasts the abstract figure of the *Citoyen* with the reality of the working class. By analogy, he contrasts the abstract principles of the philosophy of science with the substantive scientific practice of working scientists. In 1956, just before leaving Hungary, he was co-author of the Declaration of the National Committee of the Hungarian Academy of Science calling for "the freedom of science from political and moral pressure", and in particular for "the freedom of the Hungarian scientific life from its Stalinist shackles." (Archive 1.10). In a passionate speech Lakatos delivered at the Petöfi Circle pedagogy meeting the same year, he argued for encouragement of a critical attitude, absence of

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Mathematics and the Philosophy of Mathematics. 3. Essays in the Logic of Mathematical Discovery. 4. Other Papers in the Philosophy of Mathematics. 5. Early Papers in the Philosophy of Science. 6. Middle-Period Papers in the Philosophy of Science. 7. Later Papers in the Philosophy of Science. 8. Papers on General Philosophy. 9. Lectures. 10. Notes on Feyerabend, Kuhn and Popper and Miscellaneous Notes. 11. Miscellaneous. 12 Selected Correspondence (includes exchanges of letters with George Polya, Victor Kraft, Rudolf Carnap, Richard Popkin, Paul Bernays, Alan Musgrave, Adolf Grünbaum, Thomas Kuhn and Karl Popper. 13. General correspondence.

censorship and science as a guide to the party instead of the other way round (Archive, 1.9).[3]

These writings possess the sharpness, originality, forcefulness, clarity and, at the same time, ambiguity, which was to distinguish all of Lakatos's later works. Here is for example how Lakatos deals with the problems concerning the education of a new generation of scholars, claiming a role for talent, curiosity, original thinking, autonomy, right to doubt and dissent, demand for proofs and respect for facts.

> The first question is that of *talent*, its sociological role and evaluation. [...] A counter-selection has been going on for years at a national scale on this basis. Talented, courageous men of initiative were pushed more and more into the background of so-called "simple, colourless, decent, disciplined" men. When a post had to be filled or a prize given, it was always the latter type who moved up a rung, while the former moved down one. At the same time, when it came to sacking or even arresting someone, the same selective principles were at work, only this time operating in the opposite direction. [...] Education, if it is to produce scholars of whatever field, must have, as one of its central elements, the training for *original thinking*, must help develop a reliance on individual judgement, sense of justice and truth, and conscience. In the past years, however, there has been an ideological campaign against original thinking and for preventing us from believing our own sensory organs. It is enough to refer here to the unfortunately misunderstood or misinterpreted slogan: "The Party is our mind". Another vital

[3] Recall that Lakatos had worked at the Ministry of Education (Culture and Religion) between 1945 and 1948 dealing with education reform. (See Jancis Long, 1997).

quality of future learned men was also put in the dock, "petty bourgeois" branded on its forehead: *curiosity*. Curiosity and interest were restricted most brutally within narrow, brain-stifling limits. […] The history of science indicates that we ought to teach the future scholar to be modest, to be humble in his scientific claims, to be averse to all kinds of fanaticism. He ought to learn that what he does not understand, or disapproves of, still has a right to exist, and that no scientific theory, no theorem can conclude anything finally, in the history of science. […] New, hitherto unfamiliar chapters ought to be included in pedagogical textbooks, such as "Methods for stimulating curiosity and developing it into interest", "How to teach to think scientifically", "How to teach people respect for facts" and - God forbid! - "How to teach people to doubt". […] At the last Party Congress in China, Teng Xiao Ping talked about guaranteeing the *right to dissent* and remarked that if, perchance, truth happened to be on a minority side, this right would facilitate the recognition of that truth. This principle has enormous significance in science, where new conceptions are formulated at no instance by "the demand of the masses", but always by the single, solitary voice of a fragile scholar. It often takes many decades for his opinion to become that of a majority. That is to say, it would be good if our pedagogical textbooks devoted a chapter to "*How to teach respect for the right to dissent*". (Bearing in mind that he who tramples upon a dissenting individual opinion is usually not interested in the opinion of the majority either.) (Tudományra Nevelésröl" - "On Rearing Scholars", English translation by Ninon Leader, Archive, 1. 9, now in Motterlini, ed., 1999, Appendix A)[4]

[4] About fifteen years later - in "A Letter to the Director of the LSE (1978b, ch. 12) - Lakatos will

In the same period, during a discussion with friends, he is reported to have lost his temper when the question turned to defending Marxism: "You are talking about scientific method, why do you keep calling it Marxism?". Instead of preaching the dogmas of Communist orthodoxy, Lakatos claims an active role for dialectics as an instrument of criticism, rather than as a rhetorical figure for empty scholasticism. In his defence of "dialectical rationality" as opposed to "irrationalistic mystification" Lakatos was probably influenced by György Lukács.[5] The appeal to dialectics has to be seen in connection with the prevalence of vulgar Marxism in organised working class movements and pedestrian mechanistic materialism in an age of totalitarian systems in which mankind was repeatedly menaced by self-destruction.[6] Hence, asserting the validity of dialectical rationality was, according to Lakatos, an attempt, on behalf of detractors who had not grasped the point of Hegel's logic, to condemn all forms of irrationality and decadentism. Following this line of argument, history is not brought into the picture to "explain" the necessary realisation of the present society and, therefore, to vindicate the *status quo*, but rather to recognise that knowledge is fallible (for "no scientific theory, no theorem can be eternally established in the history of science", *ibid*.).

Leaving his country for Cambridge, after the Uprising in late 1956, Lakatos would not entirely give up the outlook in which he was

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comment on the principle that students should determine academic policy defending academic autonomy on a similar line. Lakatos's political and pedagogical views are challanged by Feyerabend throughout their correspondence. See in particular the crucial years of students revolt, 1968 and 1969, when Lakatos and Feyerabend where at the centre of the event, respectively at the London School of Economics and at Berkeley.

[5] On Lukács's concept of dialectic, cf. Mészáros (1972). As we shall see, Lakatos's later criticism of his contemporary 'merchants of irrationality' such as Feyerabend and Toulmin will mirror the same kind of arguments. This point has also been raised by Larvor (1998), pp. 74-75 and Duseck (forthcoming).

[6] Lakatos's adventurous personal history clearly shows the danger to his life especially during the three years period of confinement in the Stalinist prison camp in Resk. (See Long, 1997)

brought up, rather he would take with him the “forbidden brew” of Hegelian-Marxist dialectic to employ in a creative way.

3. The entangled roots of Lakatos’s philosophical project

In the Acknowledgements of his Ph.D. thesis, Lakatos claims that his work was born from the aversion to a conception of mathematics as static and authoritarian. In fact, he aims at showing that “mathematics is *dialectics* and that it cannot exist without *criticism*”. Lakatos also remarks that

> The three major - and *apparently quite incompatible* – ‘ideological’ sources of the thesis are Pólya’s *mathematical heuristic*, Hegel’s *dialectic*, and Popper’s *critical philosophy*. (Ph.D. thesis, emphasis added, Archive, 3. 4)

The importance given to the "movements of concepts", i.e. the “unfolding” of mathematical developments seen as a product largely independent of the producer's psychology, is a clear reference to Hegel; whereas when Lakatos refers to Popper he is taking a position against any account of mathematics as certain and definitive knowledge.[7] Combined to this is also the reference to Pólya: mathematics is a problem solving activity.

With reference to Hegel, Lakatos never specified the kind of works and contributions he regarded as fundamental for his education. This is why such a source of inspiration is merely “ideological”. It is even possible that Lakatos never read Hegel’s works and that, like many others, he knew of Hegel what he read in Marx. (There are, for example, no works by Hegel in the Lakatos’s Collection, i.e. Lakatos’s personal library, at LSE.) Lakatos had certainly studied Marxism at the time he took part in Szabó’s seminar on Plato at

[7] In fact, Lakatos here extended to mathematics the falsificationist approach Popper had put forward in connection to empirical sciences. Popper remained an “infallibilist” as far mathematics

Debrecen University (Szabó for example recalls that Lakatos was more interested in Marxism than in philosophy), and he later attended Lukács's lectures on Aesthetic centred on Kant's *Critique of Judgement* and Hegel's *Phenomenology of Spirit*, at Budapest University. It is worth noticing that "Lakatos's mathematical Hegelism" does not endorse Hegel's dogmatically undialectical philosophy of mathematics. On the contrary, Lakatos criticises precisely that kind of "deductive style" and static rationality which is typical in Hegel's idea of mathematics as proposed in the *Phenomenology of Spirit.* In this work, Hegel regards mathematics as the "inert and lifeless" realm of "rigid, dead propositions", i.e. the very opposite of the dynamic self-movement of concepts which constitutes the subject matter of philosophy. (Larvor, forthcoming; cf. also Kadvany, 1995).

With reference to Pólya, it is worth quoting a passage from the "Preface" of his *How to solve it* (a book Lakatos translated from English into Hungarian):

> Studying the methods of *solving problems*, we perceive another face of mathematics. Yes, mathematics has two faces; it is the rigorous science of Euclid, but it is also something else. Mathematics presented in the Euclidean way appears as a systematic, deductive science, but *mathematics in the making* appears an *experimental*, inductive science. (Pólya, 1945, p. vii)

The idea that observation may also play a role in pure mathematics goes back at least to the great mathematicians of the seventeenth and eighteenth centuries, who had shown that inductive procedures are often present where least we would expect them; in geometry, for instance, or in the theory of numbers etc. (see Truesdell, 1984),

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and logic are concerned. In general Popper seems to be rarely interested in mathematics, apart from

although ultimately the reliability of results is guaranteed by a rigorous (Euclidean) proof. Lakatos separates reliability from certainty in mathematics. Suppose we express the proof of a theorem in a given axiomatic-formal system; if we accept that the latter is consistent, we could thereby exclude the possibility of formalising any counter-example in terms of the given system. But mathematics in the *making*, mathematics in its *growing process*, rarely expresses itself in axiomatic-formal theories; instead, mathematicians too make progress through conjectures, *experiments* and refutations. In line with Arpad Szabó's classic works,[8] Lakatos considers *informal proof* as just another name for *thought experiment.* Broadly speaking, just as *in physics* we have to deal with an entire experimental set-up in order to guess why a theoretical system has failed, and to find the possible ways out, so *in mathematics* we have to analyse 'proof-thought experiments' in order to find the hidden assumption from which a paradoxical result or contradiction follows. Similarly, just as it is not always easy to deal with an anomaly of a scientific theory, so it is not always easy to deal with a counter-example in mathematics.

In both cases we have to direct the refutations towards some identified auxiliary lemmas in order to save the "hard-core" of our research. *Feedback* from counter-examples is particularly crucial in mathematics because, in calling for a further analysis of the primitive conjecture and of the proof, it suggests *where* the amendments have to be made and which (no longer hidden) lemma has to be replaced. The whole process is not just a matter of conjectures and refutations,

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his considerations on the historical development of infinitesimal calculus in his (1956/1983).

[88] Arpad Szabó had taught Lakatos at Debrecen University in 1943. Lakatos (1976) refers to Szabó (1958) when he considers "thought experiments" as the pattern of mathematical proofs in the pre-Euclidean Greek mathematics. From Szabó (1960) Lakatos takes the idea that, in Euclid's time, "postulates" and "axioms" meant propositions in the critical (dialectic) dialogue put forward to be tested for consequences without being admitted as true by the discussants. Szabó showed his appreciation of his disciple by dedicating the English version of his *The Beginning of Greek Mathematics* (1978) "To the memory of my friend Imre Lakatos".

but rather of conjectures, *proofs* and refutations. This implies a fundamental *unity* between the *context of discovery* and *the context of justification*. Proofs are the engines of discovery. As is well known, according to Popper (and to Reichenbach), there is, strictly speaking, no "logic" of discovery. The psychological process of having a new idea or arriving at a new conjecture cannot be rationally analysed. Rationality is a matter of testing. It operates only in the context of 'justification'. But Lakatos does not follow Popper here. According to him, a third alternative between "mechanical rationalism" and the "irrationalism of blind guessing" is possible: a rational and non psychologistic heuristic providing a guideline, a set of instructions, from the criticism of an old conjecture to the "discovery" of a new, improved one.

From Pólya, Lakatos took the idea that mathematical discovery follows some patterns that can be rigorously analysed. But it is mainly because Lakatos did not give up his Hegelian background that he was able to look at the process of discovery in a different way than both Popper and Pólya. In fact, the growth of mathematical knowledge is *autonomous* and *objective* and so must be its heuristic. As the Hegelian influence suggests, *growth* is not just a feature of mathematics and science, but their very *essence*. What Lakatos himself refers to as a "Hegelian conception of heuristic" follows:

> 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 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. (1976, pp. 145-146)

Heuristic is therefore concerned with the autonomous dialectic of mathematics. Furthermore, for Lakatos, Euclidianism and formalism endorse a static conception of rationality. ("Euclidism is antispeculative and puritanical"). Heuristic, on the contrary, relates to the *dynamic movement of concepts*. Incidentally, in 1966 "Proofs and Refutations" was translated into Russian in 70,000 copies. Lakatos himself was very proud of the success it met. Some hand-written *Notes* in the Archive are useful in explaining this success: the Hegelian dialectical triad lurks behind the historical reconstruction of Euler's theorem:

THESIS: Primitive conjecture
ANTITHESIS: Counterexample
SYNTHESIS: Theorem and proof generated concept
(+ lemma incorporation) (Archive, 3)

Russian readers of Lakatos's *Dokatatelstva i Oprovershenia* encountered no problem in detecting a certain familiarity linking mathematical heuristic and dialectic.[9]

4. The Hegelian Devil

Hegelian philosophy also has its 'diabolical' effects. Lakatos thought that the Hegelian language might "be generally capable of

[9] Cf. also Appendix 2 of Lakatos (1976). Lakatos presents Seidel's theorem as a synthesis of the Leibniz continuity principle (thesis) and Fourier's counterexamples (antithesis). "In Hegelian jargon, the counterexamples do not stand in 'bare opposition' to the Leibniz principle, but rather offer a 'determinate (i.e. specific) negation' of it. Synthesis in this three-step does not simply unite the best of the thesis and antithesis. Rather, the synthesis solves the problem posed by the antithesis and the thesis". (Larvor, forthcoming).

describing the various developments of mathematics", but he was aware of both "its dangers as well as its attractions." (1976, p. 145)

Lakatos had survived the dramatic experience of the Recsk punishment labour camp where he was imprisoned between 1950 and 1953, a place where Stalinist Hungary sadly tried to emulate the Soviet Union. Even though his unorthodox Communist faith remained apparently more or less intact after this experience, he later had changed his mind after having come across the classics of Western liberalism[10] and, above all, Popper's *Open Society*.[11] He was therefore aware that the impressive all-explanatory power of the Hegelian and Marxist dialectic might easily overshoot. Lakatos's Ph.D. thesis reads:

> 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 the alienated process* and can give rise to new ones. The neglect of this interaction was a central weakness of Marxist dialectic. (Ibid. p.148 and footnote 1, emphasis added)[12]

[10] After being released Lakatos gained asylum in the Hungarian Academy of Science where he had run the library thus having access to books censored to general public.

[11] Cf. Lakatos (1974a): "Popper's ideas represent the most important development in the philosophy of the twentieth century; […] Personally, my debt to him is immeasurable: more than anyone else, he changed my life. […] His philosophy helped me to make a final break with the Hegelian outlook which I had held for nearly twenty years." (p. 139)

[12] John Worrall and Elie Zahar, the editors *of Proof and Refutations,* have added footnote to this passage in which they comment as follows: "We feel sure that Lakatos would have modified this passage in some respects, for the grip of his Hegelian background grew weaker and weaker as his work progressed." (footnote 2*) I agree only partially. Since I am quite convinced that that the Hegelian background is the central *starting point* of Lakatos' research, I do not think that Lakatos would have changed this specific passage; a passage, moreover, which is a criticism of Hegel, and a criticism based on very good reasons. Nonetheless, and we shall come to this later, I agree that

Lakatos here criticises Marx and therefore Hegel in the light of Popper's fallibilism. Recall that Hegel regarded the aim of philosophy as delivering the "rule of Reason" in the "unfolding of Spirit" through world history. Hegel's logic can thus easily be twisted (by the Cunning of Reason) to justify anything that happens in history. The unfortunate implication is that whatever has been successful is thereby also somehow 'right' and superior to what has been unsuccessful. The dangers of the dialectical approach lie obviously in the *authoritarian* attitude and in the *inexorability* of the process as formulated by Hegel first and then by Marx. On the contrary, Popper's critical philosophy suggests that this process is never ending, and that our syntheses today are our theses of tomorrow.[13]

In sum, Lakatos made use of Popper's fallibilism to wring authoritarianism out from Hegel's dialectical process and made use of the Hegelian idea of a dynamically unfolding rationality underlying the growth of knowledge to reject the alleged irrationality of the context of discovery. At the same time, he discarded Polya's psychologism in favour of Hegel's 'alienated process'. To grasp the last point, recall that for Pólya (1945) heuristic mainly refers *to "mental operations typically useful for the solution of problems*" (p. 2). His well-structured rules of discovery (e.g. "Look at the unknown! And try to think of a familiar problem having the same unknown", "Separate the various parts of the condition", "Is the condition sufficient to determine the unknown?", "Could you restate the problem?", "Did you use the whole condition?" etc.) are intended as tactics for finding solutions to problems and, to a lesser extent, for

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Lakatos's Hegelianism "grew weaker and weaker" and that the key understanding Lakatos's late attitude concerning the aim of science is Popperian fallibilism.

[13] Probably at the same time Lakatos was putting the last corrections to his doctoral thesis, he discussed some philosophical implications pertaining to the method of proofs and refutations. This is how he described his *dialectical view point*: "a mathematical theory grows by new decomposition of the constituent conjecture: the theory has no level hypotheses – only for the given moment. There are no axioms but the axioms of today turn into theorems tomorrow." ("The Philosophical Implications of the Method of Proofs and Refutations", Archive, 4.1)

finding proofs of theorems. The heuristic rules of *Proofs and Refutations*, on the other hand, belong to the realm of the objective growth of knowledge (in the sense of world 3) rather than to mathematicians' minds (in the sense of world 2).[14] Here is how writing to his former student, Pólya explained main difference between his works in the pedagogy of mathematics and Lakatos's enquiry into rationality:

> Dear Imre, [..:] I can see clearly how *Proofs and Refutations* relates to my work. The basic difference is this: I myself would hardly be able to say anything on 'epistemology' which would be deserve the attention of the public. Had I been able to say anything about it, even then I would have refrained from it. It is difficult enough to have the public accept heuristic, and I would not have wanted to make this even more difficult by combining it which other controversial things. This main point of "Proofs and Refutations" is, at least according to me, to call attention to the possible connection between heuristic and epistemology". (15th December 1965, Archive, 12.9 item 236, translated from Hungarian).

5. From dialectics to methodology

Lakatos's works in mathematics were mainly carried out in Cambridge in the late Fifties and early Sixties, whereas his philosophy of science was developed at LSE, initially under Karl Popper. We have seen that Lakatos's philosophy of mathematics is a product of many influences, one of which was certainly Popper's falsificationism. So, on the one hand Popper's theory of scientific

[14] Cf. Lakatos (1976), pp. 50, 58, 76. Unlike Lakatos, Pólya (1945 and 1956) does not raise doubts on the certainty of mathematics and does not raise any foundational issue. For a more extensive and critical comparison of Pólya and Lakatos's heuristic, see Feferman (1978).

method influenced Lakatos's philosophy of mathematics. On the other, Lakatos's ideas in philosophy of science, which improve on Popper's approach, are clearly based on his previous research into the growth of mathematical knowledge. Lakatos's programme therefore develops along a single route, but in the two ways: from the philosophy of science to the philosophy of mathematics, and then back from the philosophy of mathematics to the philosophy of science.[15] As we shall see, however, the goal of these enterprises is quite different in one respect. Lakatos's philosophy of mathematics aims to challenge dogmatism (i.e. Euclidianism and formalism) rather than to defend fallibilism from the attack of sceptical irrationalism. Within an Hegelian framework, the *rationality* of the development of mathematics (and science) being the very premise does not need to be argued for. Lakatos's concern here was simply to show that knowledge in mathematics is not static but dynamic, that it cannot exist without criticism (i.e. dialectic). On the other hand, Lakatos's philosophy of science aims to grasp the unfolding rationality within the history of science and to defend it from any attack. These being anarchic (Feyerabend), elitist (Polanyi, Merton, Kuhn) or even Hegelian in disguise (Toulmin). If forced to choose one side in the battle between dogmatists and sceptics, this time Lakatos would have probably sided with the former camp.

Section 5 of the Archive allows us to reconstruct Lakatos's own pattern of discovery. I refer in particular to a couple of files catalogued by Lakatos himself under the significant titles: "Some Philosophical Implications of the Method of Proofs and Refutations" and "Research Programmes as a Continuation of the Method of Proofs and Refutations". Here is how Lakatos was extending the method of *Proofs and Refutations* to the empirical sciences:

[15] For the analogies between Lakatos's logic of mathematical discovery and his MSRP, see Zheng (1990).

> In science the same heuristic pattern [one frequently comes across in mathematics] has an important role:
> 1.A problem is proposed.
> 2.A solution is put forward in the form of a naive conjecture
> *3.*The naive conjecture is being *explained and refuted.*
> *4.*The explanation is analysed into lemmas, and lemmas are incorporated in the naive conjecture. The result is an irrefutable *theorem.*
> 5.The global counterexamples are tentatively traced to lemmas
> 6.The lemmas - in particular those pointed out in 5 - are refuted (local counterexample) and replaced by more general ones, and the theorem is correspondingly generalised. Refutations lead to rival theories.
> 7.Total local refutations lead to rival theories.
> 8.After saturation point: rejection.
> (Hand-written notes, "The logic of Explanations and Refutations", Archive 5.8)

These notes should be seen in the context of Lakatos's struggle with the "Duhem problem" and the related difficulties this implies for a falsificationist account of science. [16] As is well known, Duhem had claimed that physics, far from being a "machine which lets itself be taken apart" is rather "an organism in which one part cannot be made to function except when the parts that are most remote from it are called into play, some more so than others, but all to some degree" (Duhem, 1906, pp. 187-188). In this sense, Lakatos noticed that

[16] The Archive (5.4) includes three different contributions to Popper's philosophy: "The Popperian historiography", "The so-called deductive model of explanation", and "The metaphysical presuppositions of Popperian methodology". Lakatos was probably aiming to write a single article, which however was never put together. This material eventually leads to Lakatos (1970) and (1974a).

> In physics, if you have a global counter example you do not know what you have refuted (Poincaré, Duhem, Quine). Deductive model - sorting out possible lemmas - devise pinpointing tests. Heuristic versus deductive model. (Hand-written notes Archive 5)[17]

Lakatos goes on from his consideration of the role of *criticism* in mathematics to the methodology of scientific research programmes (MSRP) as solution of the "Duhem problem". It is peculiar that Duhem had always set aside mathematics from the context of his challenge - and yet the passage in *La théorie physique* in which this occurs "is the only one in Duhem's great book that has aged since was written" (Truesdell, 1984, p. 490). It remains valid for mathematics too that one should examine a series of propositions rather than a single one. Since 1961, Lakatos had claimed that when handling a counter-example to the initial conjecture ("global counter-example") or to any of the lemmas ("local counter-examples") one has to *choose* between various lines of behaviour, each characterised by promises and risks peculiar to itself. The main case study of *Proofs and Refutations* illustrates the different possible strategies available in dealing with polyhedra that are exceptions to Euler's conjecture. A possible choice involves "eliminating monstrosities" by refining the concept of polyhedron; another consists in "surrendering to the counterexamples" thereby declaring the conjecture to be false; yet another in making certain "hidden lemmas" from background knowledge explicit and inserting them into the theorem enunciation, etc. The first kind of tactic reduces the

[17] There follow a few notes in which Lakatos shows his intention of articulating the method of *Proofs and Refutations* in order to give an explanation of instances taken from the history of science such as, for example, Bohr's discussion of the structure of the atom (see Lakatos, 1970). Among the papers present in the section there are also other draft notes on the *case study* of the alleged "deduction" of Newton's theory from Kepler's laws. Lakatos meant to present it in dialogue form, in analogy with his doctoral dissertation. The title: "From Facts to Empirical Law". The same problem has been tackled from a Lakatosian point of view by Worrall (mimeo) and Zahar (1983).

content of Euler's conjecture and turns it into a "miserable convention"[18]. The second move is that of "naive" falsificationism in mathematics. The point is, of course, that one must recognise the element inherent of decision both in *restricting* the meaning of basic terms (such as polyhedron, vertex, edge, face etc.) and in *extending* it**.** There is growth of mathematical knowledge in those same "adjustments" which allow what in the empirical sciences is an increase in (corroborated) content. I.e. those "stratagems" which allow one to invent/discover new problems, solve some of them and give a more elegant formulation of a simple conjunction of restrictive clauses. Only this third way corresponds to the "sophisticated falsificationism" in the empirical sciences.

Here comes Lakatos' *heuristic model of explanation* as an attempt to meet Duhem's challenge:

> … of course, I take the "Duhem-Quine thesis" for granted in the sense that any refutation undermines a large bulk of our knowledge and not a uniquely specified part of it. But Duhem and Quine give no sufficient indication of how to make a reasoned guess *which* part of our knowledge is responsible for the inconsistency; *indeed, they insinuate that no such reasoned guess can be proposed*. This is the variant of the Duhem-Quine thesis that Popperians reject. Also, the Duhem-Quine thesis does not lay sufficient emphasis on the "implicit" or "hidden" character of the bulk of the background knowledge. For instance, let us take Newton's theory of gravitation *G* with suitable initial conditions *I*; and let us consider an anomalous phenomenon described by *A* such that *G*, *I* and *A* are inconsistent (since, on our assumption, the conjunction of *G* and *I* implies $\neg A$). Let us now introduce

[18] One can read into it Poincaré's (1902) conservative stand on relevant cases taken from the history of physics.

a (content-increasing) auxiliary theory *M*, say about magnetic fields which slightly perturb the spin of a planet: it may then seem that *G*, *I* and *M* in conjunction do imply *A*. Then it would seem that while *G*, *I* and *A* were inconsistent, the addition of a new proposition turned the inconsistent theory into a consistent one. But according to elementary logic, if a theory is inconsistent, so are all its extensions. The solution of this paradox is that *G* says not only that there is a Newtonian field of gravitation, but also that the whole field is *nothing but* that of Newtonian gravitation. When we add *M*, we delete *G* and replace it by a weaker *G'*. "Addition" of a hidden premise is not simply incremental; it is coupled with a modification in the extent premises. But if we formulate the hidden lemma as "there is no magnetic field", we may be asked: where has the hidden lemma been "hidden"? One may say: in the Platonic world of ideas we gradually (but *not* cumulatively) invent/discover. The concrete, positive version of a hidden lemma is always invented/discovered under critical pressure. Imagination and criticism unfolds - slowly and with frequent hitches - ever more of the deductive structure. One may say that *the target of the arrow of refutation is shaped while the arrow is already in the air*. Criticism does not *assume* a fully articulated deductive structure: it creates it. The true deductive model of explanation is an ever-changing one; one in which propositions keep being added and deleted. *One may not explain what one has set out to explain; one may not refute what one has set out to refute*. ("On the so-called 'deductive' model of explanation", Archive 5. 4)

Since theories are not fully-fledged deductive systems, counter-examples are not entirely negative as far as they help to reveal the

hidden assumptions and unfold thereby a *new* deductive structure. In the very spirit of *Proofs and Refutations*, this long passage is nonetheless only a declaration of intentions. In fact, in promoting the MSRP in analogy to the method of *Proofs and Refutations,* Lakatos found himself giving too much credit to the theoretical autonomy of research programmes thereby undermining the role of counter-examples and also the intrinsic unity of the context of discovery and the context of justification. He thus lost sight of the main question - *How do you improve your conjecture* -, and did not fully exploit the possibilities he had outlined in his programme.[19]

From the method of *Proofs and Refutations* to the MSRP an important shift in the concept of *heuristic* occurs.[20] This took place during Lakatos's career at LSE, when Lakatos decided he wanted to *escape* the Hegelian devil by moving towards the Popperian blue sea. Lakatos, the philosopher of mathematics, claims that he uses the word "methodology" in a sense akin to Polya's 'heuristic' and Popper's 'logic of discovery'. (1976, p. 3)

According to Pólya, *heuristic* is a set of strategies for solving mathematical problems — to learn, to teach and to reconstruct mathematics. Discovery and invention are mainly considered in their *psychological* aspects. According to Popper, the *logic of discovery* (or, better, the "Logik der Forschung") in the sense of a theory of scientific method, is both descriptive and normative. Popper's demarcationist project not only evaluates scientific products but also offers *standards of intellectual honesty* that scientists have to meet in order to fulfil the aim of science. According to the author of *Proofs and Refutations*, the role of heuristic-methodology is strictly related to its object of inquiry. As we have seen, mathematics is a product of

[19] A similar point has also been made by Fine (1978) who has stressed the difference between playing the game "save the proof" (by stretching the concept)" and the game of "advancing our science by developing new theories". See also Forrai (1993), pp. 170-174.

human activity that 'alienates itself'. It is autonomous and objective. So the purpose of heuristic-methodology is to grasp the *logic* of the development of mathematics, the *dialectical* pattern of growth, the rationality of mathematics in the making. From this point of view, the heuristic-methodology looks *backward* to identify the rules that made such a growth possible in the past, and at the same time it looks *forward* to advise on how to obtain progress in the future. Heuristic-methodology, although fallible, is both evaluative and normative. Generally speaking, from the Hegelian-Marxist view the aim of philosophy is not a contemplation of eternal truths, but rather an effort to interpret the present in the light of the past with a view to shaping the present for a better (utopian) future (Larvor, 1998, p. 1). Thus, for the Hegelian philosopher of mathematics, methodology, heuristic and logic of discovery are synonymous. But this is not the case for the author of the MSRP. For the Professor at the LSE, the hope that methodology "would provide scientists with a mechanical book of rules for solving problems has been given up: modern methodologies or logics of discovery consist merely of a set of [...] rules for the *appraisal of ready articulated theories*. [...] The term 'normative' no longer means rules for arriving at solutions, but merely directions for the appraisal of solutions already there"; it follows that

> methodology is separated from heuristic rather as value statements are from 'ought' statements. (1971a, p. 103 footnote 1)

Methodology no longer concerns the set of rules and strategies to be adopted in the context of discovery. Only heuristic does. But it does so in a different way than before. Heuristic principles (as separated from methodological ones) are not "objective" and "autonomous". They instruct scientists on how to go on *within* a

[20] Forrai (1993) has already raised this point and also suggested the importance of the Hegelian

particular research programme. They are therefore subjected to change alongside with changes in science. (For example, we have an Aristotelian Ptolemaic heuristic, a Copernican heuristic, a Newtonian heuristic and an Einsteinian heuristic and so on.)

The last nail in the coffin of the early Lakatos is that methodology (divorced from normative heuristic) "presumes to give advice neither about how to arrive at good theories nor even about which of two rivals programmes the scientist should work on":

> Whatever scientists have done, I can judge: I can say whether they have made progress or not. But I cannot advise them – and I do not wish to advise them – about exactly what to worry and in which direction they should seek progress. (Lakatos, 1971b, p. 178)

6. The understanding of (Toulmin's) *Human Understanding*

At the time of his death, Lakatos was working on a review of Stephen Toulmin's daring book *Human Understanding* (Oxford, 1972). He had already written and thrown away three increasingly detailed versions of his comments. A fourth longer version, set out in draft form in the summer of 1973 was never completed. Lakatos's intention was to place Toulmin's approach in the more general framework of comparison and conflict between the great traditions in the history of epistemology: *scepticism*, *demarcationism* and *élitism*.[21] Lakatos assimilates the enquiry on "human understanding" with the teaching of the "second Wittgenstein" from which Toulmin clearly descends, were it not for an important specification. From the

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inheritance in this context.

[21] Lakatos was not fully satisfied regarding the way he accomplished this task. (This is why the editors of *Philosophical Papers II* have chosen to divide the material between Chapter 6 "The problem of scientific theories: three approaches", and Chapter 8 "Understanding Toulmin", i.e. those pages dedicated to analysing Toulmin's idea of human understanding.) A reconstruction of the problems of epistemology around different traditions such as inductivism, probabilism,

point of view of Lakatos, the Wittgenstein of the *Philosophical Investigations* is an intellectual defender of the *status quo.* The mission of new Wittgensteinian philosophers is to discourage every incursion from outside and attempt to overthrow from inside a "linguistic game" or a "form of life". Toulmin, however, also supports the usefulness of change. Thus, in Lakatos's view, he avoids this reduction of philosophy to a mere "thought police" but at the cost of appealing to the "Hegelian Cunning of Reason", which justifies *change* in the name of *progress*. (If progress is guaranteed by the cunning of reason, the description of change is the description of progress. But what - Lakatos asks - if there is disagreement within the scientific community over some proposed change? Answer: only history will decide. See Lakatos, 1973/1976, p. 237)

Toulmin therefore recurrently finds himself "between the Wittgensteinian devil and the Hegelian deep blue sea", which is precisely the title of an earlier draft of the same paper on Toulmin (Archive 8.5). Lakatos had surely thought about this, since, in another version, "Professor Toulmin" was caught "between the Hegelian devil and the Wittgensteinian deep blue sea". The uncertainty here is understandable. The outcome of both of Toulmin's stands were diabolical to Lakatos, since he considered a last ditch defence of the 'closed society':

> Following the tradition of Wittgenstein, Polanyi and Kuhn […] the picture described [by Toulmin] is that of a society without radical alternatives, where one can only 'improve' but not replace 'the current repertory of concepts', a society whose membership depends on oaths of loyalty to specific doctrines ('commitment to collective ideas') and where only 'professional forums' can judge the implications of these

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verificationism, conventionalism, elitism, neoscepticism is also provided in the "Lectures on Scientific Method" Lakatos gave in the same period at LSE (Archive 9, in Motterlini, ed., 1999)

> doctrines for specific cases. In this closed society critical reappraisal and modification are allowed only if done by 'qualified judges'. The layman is powerless, the elite self-perpetuating. (1973/1976, p. 241)

However, it would be possible for Lakatos's rhetoric to *backfire* at this point, since *he* would himself remain undecided between the devil (of Hegelian authoritarianism) and the deep blue sea (of Popperian fallibilism).[22] In particular, the early Lakatos takes for granted that "After a saturation point: we reject the theory" (clause 8 of the passage cited above at p. 17). Lakatos (1970) states, on the other hand, that there is "no such a thing as a 'natural' saturation point" for a research programme.[23] The MSRP does not in fact fix any *time limit* for the final assessment of the empirical progressiveness or degeneration of a programme. At the dawn of a new and ambitious scientific idea a certain methodological tolerance is called for, and this applies to research programmes whose heuristic has "run out of steam". It is 'not irrational' for supporters to defend their theory with *ingenious ad hoc* stratagems and to hold out for a long time even without any empirical success. "Crucial" experiments are hence seen to be crucial only decades later, "after long hindsight", and

[22] In fact, in his late years, Lakatos became more and more disappointed about Popper's achievements to such an extent that, in this case too, reverting the terms (i.e. "Professor Lakatos between the Popperian devil and the Hegelian deep blue sea) could have worked just as well. To Lakatos, Popper's lack of tolerance with respect to the criticism directed towards his own falsificationism had to appear as diabolical as the old Hegelian authoritarianism. (See Lakatos-Feyerabend Correspondence, in particular, the letter dated 26.16.1972, where Popper is mentioned as "Al-Poppuni", "the great tyrant of Reason") Here is for example how Lakatos taught his students about Popper's philosophy while lecturing at LSE in 1973: "Popper's three major contributions to philosophy were: 1) his falsifaibility criterion - I think this is a step back from Duhem -; 2) his solution to the problem of induction - where I think he is a step back from Hume, and 3) his literary masterpiece 'The Open Society by one of its enemies'" (Motterlini, ed. 1999, p. 189).

[23] Cf. Lakatos (1970, p. 72 footnote 1): " […] in my (1963-4) I was more of a Hegelian, and I thought there was [a natural 'saturation point']; now I use the expression with ironical emphasis. There is no predictable or ascertainable limitation on human imagination in inventing new, content-increasing theories."

> rationality works much more slowly than most people tend to think and, even then fallibly. Minerva's owl flies at dusk. (1970, pp. 72, 87)

Apart from the stress on fallibility, Hegel lives on. “Absolute knowledge” in the form of complete “self-consciousness” and “self-possession of spirit” is only available at the end-point of the thinking process:

> Philosophy always comes to the scene too late to give instruction as to what the world ought to be. As the *thought* of the world, it appears only when actuality is already there, cut and dried, after its process of formation has been completed. […] It is only with the fall of the dusk that the owl of Minerva spreads its wings. (Hegel, *Philosophy of Right,* "Preface", p. 23)

It may be the case that (the rationality of) science can be understood backward, but it has to be done forwards. (This is actually nothing but Kierkegaard’s criticism to Hegel; simply substitute “life” for “science”.)

Having set aside Popper's “instant rationality" in order to bestow on methodology the role of the owl of Minerva, Lakatos has been accused by Feyerabend of oscillating between a *conservative* use of the MSRP, which would eventually lead to benefits for the *status quo*, and a *revolutionary* use which amounts to nothing but “anything goes”. [24] Lakatos betrays his *authoritarian* nature by strengthening his methodological standards not on an argumentative level, but by shaping a historical and social situation that renders it difficult, *in practice*, to cultivate a degenerating programme. (For example, he claims that people who support a degenerating research programme should not enjoy as much freedom as might appear: “they can do this

[24] Feyerabend’s criticisms of Lakatos are freely taken from his (1975a), Ch. 17, and (1976).

mostly in private. Editors of scientific journals should refuse to publish their papers, which will contain either solemn reassertion of their positions, or absorption of counter-evidence by *ad hoc* linguistic adjustments. Research foundations, too, should refuse money." 1971a, p. 117). On the other hand, taken by themselves, these standards are incapable of forbidding the most outrageous behaviour. (For example, Lakatos claims that "there is freedom [...] in creation and over which programme to work on […] . *Appraisal* does not imply *advice.*"1973, p. 110)[25] Thus, according to Feyerabend:

> Combining the common sense standards of scientists with the methodology of scientific research programmes, Lakatos utilises the intuitive plausibility of the former to support the latter: a splendid Trojan horse that can be used to smuggle real, honest (a word so dear to Lakatos) anarchism into the minds of our most dedicated rationalists. ("Theses on Anarchism", in Motterlini, ed., 1999, p.177)

Whether or not Lakatos has really gone a long way towards epistemological anarchism is a problem we can better solve in the more general framework of Lakatos's late struggle against the "modern intellectuals' betrayal of reason".

7. Historicism: i.e. dealing with explosives

In his review of Toulmin, Lakatos goes on distinguishing his position from both the élitist "thought police" and the Cunning of Reason:

> I agree with Toulmin that no demarcation criterion is absolute. I am a fallibilist with regard to demarcation criteria, just as I am a fallibilist with regard to scientific theories. They

[25] On the alleged existence of 'two Lakatos', the supporter of a more or less close relation between appraisal and advice and the one who, denying that MSRP does not give any advice at all, has gone "a long way towards epistemological anarchism", see Motterlini (1995).

> are both subject to criticism and I have specified criteria not only by which one programme can be judged better than another, but also criteria by which one demarcation criterion can be judged better than another. But I do not draw the Wittgenstein inference from fallibility of propositions to their dismissal. I do not panic: I do not switch from articulated propositions to inarticulable skills of doing and judging science. For to do so is to reintroduce through the back door a pragmatist version of justificationism with the help of the Hegelian Cunning of Reason. I want clear theses in both science and philosophy of science where logic can assist criticism and help to appraise the growth of knowledge. […] It is largely because of my conviction that without deductive logic there can be no genuine criticism, no appraisal of progress, that I stick to old fashioned Popperian-type *Criticism and the Growth of knowledge*". (1973/1976, pp. 242-243)

Three relevant claims need to be distinguished here. *Firstly*, any demarcation criterion is fallible. Yet Lakatos maintains that we can avoid collapse either to historical relativism or élitarist authritarianism, only by appealing to a 'core' of ("old fashion Popperian-type") standards of appraisal. In particular, to avoid historical relativism, "rational appraisal must *precede* and not *follow* full-scale empirical history" (i.e. 'internal' normative history is primary and external descriptive history is secondary.) Similarly, to avoid the regressive problem-shift from the problem of demarcation to the problem of knowledge expressed "in the form of skills and activities" (the latter leading to the Orwellian world where history is for the winners), a "universal criterion of progress" is called for.

Secondly, such a criterion of progress is provided by MSRP in the form of very general principles of theory-appraisal. These will include

"the basic tenets of deductive logic and intuitive rules for weighing evidence (especially the principle that special weight is to be given to a theory's predictive success)" (Worrall (1989), p. 377).[26]

Thirdly, as we appraise scientific theories, we should be able to appraise the standards of appraisals. The tool for this task is history. The meta-criterion is provided by the methodology of historical research programmes (MHRP). In particular, we accept a methodological proposal if it can be shown that it was effective in paradigmatic cases of growth of knowledge. In fact, even though there has been no general agreement concerning *a* methodological criterion, Lakatos maintains there has been considerable agreement about whether a particular step in the game was scientific or crankish, or whether a particular gambit was played correctly or not. Thus Lakatos's meta-criterion appraises methodologies on their ability to provide historical reconstructions minimising the influence of "external" factors and maximising "internal" explanations.

Yet Feyerabend objected that the very idea of a "common scientific wisdom", i.e. a wide range of accepted 'basic' value judgements on singular scientific achievements, is but a chimera.[27] The strength of *scepticism* flowed from the realisation that, together with single results, the *criteria* to assess them *change* as well. Thus, revolutions do actually challenge all the ideas born in connection with those procedures, including 'basic' value judgements. It might therefore be the case that the better a methodology seems to capture the rationality of science, *the greater its mystification*. Moreover, according to Feyerabend, the fact the new astronomy of Copernicus, Kepler and Galileo took root, or the fact that witch hunts came to a

[26] On the value of 'novelty' for theory-confirmation, see Worrall (1985) and (1989), Zahar (1989). Lakatos's criteria of progress have been applied in many different case studies both in the natural sciences and social sciences. It goes beyond the scope of this paper to assess them. The question of course remains whether these principles are stable or change alongside changes in substantive theories. I shall deal with this problem in the next section.

stop, happened because independent thinkers resolved to introduce obsolete theories and defend them even *in spite* of all the traditional methodological rules. If this is the case, then putting forward *a* 'theory of rationality' is a tyrannical intellectual act which assumes (without argument) that progress has occurred, that it has been obtained thanks to such a normative rule and, finally, it constitutes the best possible advancement, i.e. that of science as it has *de facto* developed "in the last three hundred years".

Feyerabend criticism needs to be handled carefully. Firstly, I shall clarify the aim of rationally reconstructing history, and, secondly, in the next sections, I shall raise the related question of the stability of some core of methodological principles.

To start with, there is no history of science without methodology, and no methodology without history of science. Lakatos's parody of Kant's maxim is well known: philosophy of science without history of science is empty, history of science without philosophy of science is blind. This attitude to history reveals once more the philosophical mix of Hegel and Popper. The *'historicist'* Lakatos starts from the unquestionable premises that knowledge (at least in the 'most advanced' sciences) does grow, suggesting that we need to *extract* rationality from its historical development. The *critical* Lakatos claims that we have to *test* our theory of rationality against history. Lakatos's appeal to rational reconstructions of single historical cases should thus be taken as an attempt to grasp the (objective) reasons and strategies that have brought new ideas. [28] Despite Feyerabend's criticism, there is nothing "mystifying" in appraising past beliefs according to given methodological standards. On the contrary, such

[27] In presenting Feyerabend's criticism I shall quote liberally as well as paraphrasing from Feyerabend (1975a), (1976) and the Lakatos-Feyerabend correspondence.

[28] This does not imply that we need necessarily to pry into scientists' psychology in order to assess the 'reasons' or peculiar aversions which have governed their choices, but simply that we should analyse and evaluate the case we are faced with in the light of some explicit heuristic, see Zahar (1983).

judgements lead to historical data that are not easily obtainable in other ways, and allow one to outline and explain the whole process. Any appraisal of this kind is twice desirable: the historian may discover (old) 'new' facts, the philosopher tests his own standards. [29] In this way, the *circularity* implicit in checking an epistemological proposal against the history of science is not vicious; rather it may turn out to be a *virtuous move*.[30] On the other hand, Lakatos concedes to Feyerabend that one has to be moderately sceptical with regard to an "a priori statute law". This is why he advocates a "pluralistic system of authority" thanks to which the authority of scientists' judgement on particular cases criticises the general authority of the immutable rules and *vice-versa*. In this way only, the proliferation of different points of view, the comparison between different 'rational' reconstructions, the awareness of local strategies and the reasons behind researchers' moves specify how we can learn from history and, especially, how we can escape from the influence of the 'bad' philosophies (i.e. theories of rationality).

Once more, however, it should be noticed that Lakatos's balance between Hegel and Popper is not stable. In particular, the "whiff" of historicism may turn into a full-blown storm. Lakatos was probably aware of the breaking power of his own stand already in 1961. At that time he wrote:

> I am afraid that some ardent Popperite may already be rejecting all that I am about to say [but] I am quite convinced that *even the poverty of historicism* is better than the

[29] Writing to Lakatos on the 18th April 1973, Feyerabend acknowledges that "research programmes are good for history […] because they are connected with a *historical* method of evaluation while paradigms etc. etc. are not connected with any method of evaluation at all. Research programmes make you look for things in history which are much more interesting than what historians or philosophers look for. They are excellent for history, much better than anything that went on before (except, of course, Hegel)". But, he continues, "they do not solve the problem of rationality, that is, even the greatest success in history does not refute anarchism". (p. 325)

[30] Lakatos thus takes into account that "inclination towards history" which Federigo Enriques (1936) considered essential in a philosophical understanding of success in science and scientific practice.

> complete absence of it - always providing of course that it is handled with the care necessary in dealing with any explosives. (1959-1961, p. 61)

Yet, Feyerabend would probably have added, ... *providing it is placed under the 'right' targets*. But here comes the main disagreement between Lakatos and Feyerabend, for they held different opinions regarding what the 'right' targets should be. This brings us to my second point, what changes in "The Changing Logic of Scientific Discovery"?

8. Back to the future: Feyerabend's neo-Pyrrhonian challenge and beyond

Lakatos left unfinished his reply to *Against Method* and the related proposed book "The Changing Logic of Scientific Discovery". Sections 5 and 8 of the Archive contain Lakatos's *Notes* (from 1963 up to his last days) on his life-long project of writing a history of the critical (and no so critical) interaction between *methodological standards* and *scientific achievements*. Lakatos had partially attempted this task in his paper "Newton's Effect on Scientific Standards"[31], which precisely reconstructs the battle between Cartesians and Newtonians about the very standards of (scientific) proofs and (scientific) criticisms. In an unpublished paper of the same period, Lakatos puts forward an excellent analogy to depict the problem-shift brought about in epistemology by the rise and downfall of the Newtonian research programme:

> Let us imagine that mankind has set up moral standards and maintained them for centuries without anybody being able to live up to them. Some claimed, hypocritically, that moral acts

[31] Early draft of this paper were written in 1963-1964, but Lakatos returned to it several times in the following years.

- and, indeed, moral men, did exist; others kept unmasking these reports and either claimed that the high moral standards will necessarily remain utopian forever, or set up programme after programme for making man moral and noble. Then, suddenly, some actually started behaving in a new way which soon struck most people as truly moral. First they hailed them as having finally realised the old dreams. But then slowly it dawned on them that although the new men's acts were undoubtedly virtuous, they did not in fact comply with the old standards. They tried gradually to weaken the standards, but no matter how one weakens the standards for they remained unsatisfied. [...] People had two ways out. The sceptics celebrated the end of all morality. But a new school of dogmatists accepted the *new moral facts* and devised *new moral standards* in the light of which their morality - a new morality - could be seen.

This was then *a revolution* - first in morality, then in moral theory. Not necessarily the last one: inconsistencies remained, new types of moral acts (expressed in "moral basic propositions") further upset the new standards. The problem of morality was shifted. Moreover, the revolutionaries set up a theory of moral progress which sets up standards for standard change; a theory in the light of which their revolution was not just a change in subjective fashion, but progress towards moral truth. (“On the Intellectuals’ betrayal of Reason”, Motterlini, ed., 1999, pp. 396-397)

Lakatos’s well known intention was to recapitulate the problem-background and to assess the problem-shift within the dispute between Cartesians and Newtonians. This would have helped to

explain the success that Newtonian physics achieved *regardless* of its violation of the standards commonly accepted by the scientific community. In particular, Lakatos points out the discrepancy between Newton's *actual* procedure (i.e. his *method*) and his *Regulae philosophandi* (i.e. his *theory of method*) put forward in explicit form to defend his discovery and to defeat his rivals. Thanks to a peculiar historical paradox, following generations were to accept not only the richness of Newton's results, but also the 'poverty' his *theory of* what made up his scientific achievements:

> *In this sense one may say that Newton's theory of method created modern philosophy of science.* But this turn separated science and philosophy of science from 1686 to 1905 [when Einstein's theory had superseded Newton's] or, rather, till 1934 [when Popper had first looked at the problem in this way]. *Science*, the schizophrenic genius, marched from victory to victory. *Philosophy*, unaware of the split between the wonderful Newtonian method as practised and the mad Newtonian method as professed, tried to clarify the professed method, and thus turned into *a mad study of madness*. ("The Rise of Defensive Positivism", Archive 5.5, this passage is a different version of the concluding section of Lakatos, 1963-1964/1978).

Underlying this paradox is the fact that Newton took his methodological standards from past centuries dominated by the all-pervading idea that religious knowledge was certain and indubitable; by analogy, science was expected to respect similar standards. In fact, the split brought on by the Reformation (though against the original intentions of the founding fathers) raised the problem of *fallibilism*. How is the Church expected to be the authority on its own infallibility, given that the point in question is

precisely whether the Church is the 'true' authority on religious matters? This question was to unleash a sceptical crisis not only in theology but also, shortly thereafter, in the sciences and in all other areas of human knowledge. (Consider, for instance, the shift in the criterion of truth from Erasmus to Descartes described by Popkin 1979, ch. 1).[32] Thus, sustaining Newton's views was the intellectual inconclusiveness and political destructivity of rival theological stands as well as the belief that science could achieve the certainty that theology at the time seemed painfully denied. A century later, the stunning success of modern natural sciences, and primarily of Newton's research programme, was to breathe new life into the battle between the dogmatics and the sceptics. On the one hand "some radical dogmatists, undeterred by the long series of defeats of reason in human affairs, have been trying to generalise Newtonian method to social, ethical and political problems". On the other, "some radical sceptics, undeterred by the long series of successes of Newtonian science, have been trying to show that all these successes were sham successes and even the best theories of the exact sciences were nothing more than irrational (if possibly 'great') beliefs." ("On the Intellectuals' betrayal of Reason")

Lakatos refuses to side either with the radical dogmatists or with the radical sceptics ("the dogmatists tried to prove too much, the sceptics tried to explain too much"). Instead, he points to a new appraisal of this controversy, by recognising "the basic unity of opposites (dogmatism-scepticism) and the possibility of their

[32] Popkin's works on the historical passage from the epistemological (constructive) scepticism of the *crise pyrrhonienne* to the antireligious (dogmatic) scepticism, which through the Enlightenment brought the idea of science we today have, had an important influence on Lakatos. See, in particular, Lakatos (1963-1964b). It is thus not surprising that Lakatos had chosen Popkin for the opening speech at the 1965 International Colloquium in the Philosophy of Science held at Bedford College in London. See Popkin (1968). Popkin, on the other hand, was interested in Lakatos's fallibilist approach to mathematics (refer to Popkin's letter to Lakatos of June 18th, 1962, Archive 12.2, item 31), and when he later extended his *History of Scepticism* up to Spinoza (1979), he dedicated it "to the memory of Imre Lakatos".

dialectical ‘Aufheben’ [superseding].” (“Lakatos conversation with Popkin”, in Beck, Yourgrau 1970, 22)

Yet Lakatos regards the “modern betrayal of reason”, consisting in “the intellectual attack on the objective epistemological value of the exact sciences”, as “criminal”. If, on the one hand, Feyerabend has shown many different ways in which any principle of rationality can turn into a prison, and has consequently claimed along with the sceptics that the betrayal of ‘reason’ by man is preferable to the betrayal of man by reason; on the other hand, Lakatos has challenged his rival’s anarchism insisting on the bias underlying the sceptico-dogmatist’s point of view, by showing how radical relativism ultimately leads to forms of intellectual surrender which maintain that *might is right*.

In order to propose his (fallibilist) stand as an alternative to dogmatism and scepticism, therefore, Lakatos himself - a worthy theologian of rationality - has to face the challenge of the Greek Pyrrhonians, whose arguments after lying forgotten for centuries had suddenly come to the forefront, especially in regard to the primary epistemological problem brought up by the Reformation. Luther’s denial of the authority of the Church and his assertion of a new “rule of faith” (i.e. ‘true’ is what conscience is compelled to believe when reading Scripture) for determining religious truths, constitutes a rather neat example of the “problem of the criterion” as it appeared in Sextus Empiricus:

> In order to decide the dispute which has arisen about the criterion, we must possess an accepted criterion by which we shall be able to judge the dispute; and in order to possess an accepted criterion, the dispute about the criterion must be first decided. And when the argument thus reduced itself to the form of circular reasoning the discovery of the criterion becomes impracticable, since we do not allow the Dogmatic philosophers

> to adopt a criterion by assumption, while if they offer to judge the criterion by a criterion we force them to a regress ad infinitum. (Sextus Empiricus, *Outlines of Pyrrhonism*, in Popkin, 1979, p. 3)

Feyerabend's criticism of Lakatos is clearly a revival of this classical Pyrrhonian challenge. The problem of justifying the standards of theory appraisal does not arise as long as there is an unchallenged criterion. But once scientific revolutions are brought into the picture epistemological scepticism may be back again.

In order to meet the Pyrrho-Feyerabendian challenge, let us distinguish three levels of commitment of the scientific community: (I) the factual level of accepted general theories, (2) the methodological level of accepted standards of appraisals, (3) and the axiological level of the aims of science. According to Feyerabend, history reveals diversity in method as well as diversity in science. Hence, change is not restricted to substantive scientific theories but involves methodological rules, and possibly even the more generally aim of research. Once such a "big picture" or "holistic" view of scientific change is held, *relativism* is unavoidable. (How can changes be rationally accounted for if even the basic principles of rationality are subject to change?) The Lakatos-Feyerabend quarrel "For and Against Method" can be regarded as a dispute about the scope of the concept of "methodology". Feyerabend categorises as methodological any general metaphysical and heuristic principle. In his battle against "Reason" he over-stretches the elastic term "methodology" to such an extent that the dispute on the criterion can never be "first decided", since no criterion is ever fixed and any attempt to assert it is prey of the circularity argument well outlined by Sextus Empiricus. If we do interpret methodology in the very wide sense of "Weltanschauung", "forms of life" or general heuristic principles, no doubt

"methodology" is subject to change as science changes. Lakatos would have no objection to Feyerabend in this respect. But this is not at stake in Lakatos's defence of scientific rationality. Lakatos allegedly works on a narrower concept of "methodology". He maintains that heuristic norms change alongside of research programmes, but he stands for some "core" of invariant standards of theory appraisals. The idea underlined here is that even though we have "learned how to do science better alongside doing better science", we maintain that the (implicit) standards of theory appraisal are not themselves historical (see Worrall, 1988 and 1989). Bearing in mind the distinction between explicit (and changing) methodologies and implicit (and fixed) standards of appraisal, the task of Lakatos's *Changing Logic of Scientific Discovery* would become perfectly clear: to grasp and to articulate sharply and clearly the unchanging standards (or, to put it against the Hegelian background, to grasp the "unfolding of reason" and present it "cut and dry", after its process of formation has been completed).

Since Lakatos had a higher opinion of historicism than Popper, he was aware that this task cannot be realised *a-priori*, but rather by looking at science in its progress. At the same time, since he had a lower opinion than Hegel of the Cunning of Reason, he was aware that these standards should not be themselves subject to change. Lakatos could thus *consistently* defend his attempt to rationalise changes in professed methodologies only because progress in science is characterised by standards which are independent of them.[33] Of course, the burden of the proof is with Lakatos. That is, he

[33] The title "The Changing Logic of Scientific Discovery" does not help in clarifying the ambiguity of Lakatos's position on this issue. The title clearly suggests that methodology goes through changes. However, if by "logic of discovery" we intend methodology in the narrow sense, the question arises of how could we appraise "changing" without anything remaining fixed. Consistency can be re-established only by distinguishing between implicit (fixed) methodology, and explicit (changing) methodology. A less ambiguous (though admittedly less charming) title could have been: "Changes

has to show that an "invariant core" of standards of appraisal, which is common to all different (explicit) methodologies, and which maximally satisfied the unchanging, abstract formal principles of good science, actually *exist* (Worrall, 1988, p. 272).

Lakatos perhaps made up his mind only near his end. Here is for example how he writes to Feyerabend on the 10th January 1974:

> Dear Paul, I was amused by your suggestion that the scientific revolution was a revolution in the standards. This is of course the story I encapsulated in the announced title: *The Changing Logic of Scientific Discovery*. A chapter of this book would have been my Newton paper in which I discuss this change in standards in detail. The trouble was that Worrall and Zahar persuaded me that the standards which I ascribed to the seventeenth century were already there at the end of the sixteenth. Bashi Sabra almost beat me up since according to him these standards were there in the age of Ptolemy. Now your letter caused a conversion effect, and now I think that my Newton paper is perfectly correct and can be made consistent with the Copernicus paper, and now I am going to publish it. (Motterlini, ed., 1999, p. 355, see also p. 357)

Lakatos might eventually agree with Feyerabend that the history of scientists' methodological beliefs is irrational. After all, scientists may well be unscrupulous opportunists or anarchists. They possibly know as much about doing science as fish do about hydrodynamics.[34] This is why philosophy of science that confines itself to the history of professed methodologies may easily result into a "mad study of

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in the (Explicit) Logic of Scientific Discovery" (in analogy with Lakatos's "Changes in the Problem of Inductive Logic") clearly pointing to an analysis of the changes undergone in the professed methodologies in the light of a fixed set of standards of theory appraisal.

madness". But, *contrary to Feyerabend*, he does not make the inference from anarchic scientists to anarchic science. He maintains that we should be able to rank Copernicus's research programme better than Ptolemy's and Einsten's better than Newton's with respect to some methodological standards implicitly endorsed by the scientific community. Only in this way rationality can be defended and the collapse into either sceptical relativism (disguised as anarchism) or the cunning of reason (disguised as authoritarian elitism) ca be avoided.

One question still remains. That is, whether the fact that Lakatos's methodological appraisals are backward-looking means that they are not intended to have practical import. Lakatos's stand on this point can hardly be misunderstood. Here is for example how he once replied to his alleged fellow anarchist.

> I do not mind your anything goes, but when it comes to moral theory I even make mincemeat of Pyrrho. You certainly would not hurt a fly - as you put it. The question is what the anarchist does when he is in a position to hurt either one fly or another but is bound to hurt one. Will he commit suicide? You may remember that before I started off on research programmes, I discovered that I had to substitute *acceptance* and *rejection* of theories by *preferring* one theory to the other. And this of course also applies to ethics and politics. For instance, I would not like to hurt the North Vietnamese, but if not hurting the North Vietnamese hurts the South Vietnamese I am faced with a problem which *I* am willing to face but *you* are not. (14th August 1972, ibid., p. 296)

[34] On the "implausibility" of Lakatos's resort to scientists' "false (methodological) consciousness" to account for the rationality of theory-change, see Newton-Smith (1981, p. 71), Laudan (1989, p. 321); for a reply Worrall (1985) and (1989).

So much for Lakatos's attitude concerning the socio-political relevance of any enquiry into the method of science. Let us finally turn to Lakatos's position on the relationship between methodology and its aim. Once again, I shall deal with this last point against the background of Lakatos being between Hegel and Popper.

9. The Popperian Deep Blue Sea

Ian Hacking wrote an influential paper (1979) in which he stressed a tension within Lakatos' philosophy. According to Hacking, Lakatos oscillates between realistic metaphysics and the idea of "methodology as an objective surrogate for truth". Educated in an Hegelian-Marxist tradition, Lakatos accepted the Hegelian demolition of the correspondence theory of truth and tried to develop a methodological account in the context of which the assumption of a realistic metaphysics seems to have no place. Hacking's "neo-Hegelian Lakatos" dispenses with the notion of truth as correspondence to the facts and *replaces truth by method*. The starting point of this enterprise is the simple fact that knowledge grows. What is then required is an analysis of growth. This being given, the aim of science is not to increase verisimilitude but to meet the requirements that the scientific method suggests.

It is my view that Hacking's story is just a part of the whole story. The late Lakatos could not have regarded methodology as a substitute for truth because he explicitly demanded a connection between the 'game of science' (method) and its 'rational' end (truth):[35]

> [We need] to posit some extra-methodological inductive principle to relate - even if tenuously - the scientific gambit of pragmatic acceptances and rejections to verisimilitude. Only

[35] I have argued this point extensively in my (1995). Cf. also Larvor (1998), pp. 62-65.

> such 'inductive principle' can turn science from a mere game into an epistemologically rational exercise; from a set of light-hearted sceptical gambits pursued for intellectual fun into a - more serious - fallibilist venture of approximating the Truth about the Universe. (1971a, p. 101; cf. also *ibid*., pp. 97, 108-9 and 1974a, pp. 154-159)

We have seen that the Hegelian-Marxist influence on Lakatos is articulated and important. However, contrary to Hacking's interpretation, Lakatos had eventually made the convergence of science and truth the matter of a separate metaphysical conjecture. Popperian *fallibilism* rather than the post Kantian demolition of the copy theory of truth is therefore the correct key to understand Lakatos's view in this context. Of course, for a fully coherent fallibilist "there is no ultimate proof that […] we have been heading the Truth" We can only (non-rationally) believe, or rather hope, that we have been. Unless hope is a 'solution', there is no solution to Hume's problem." (Lakatos, 1974b, p. 213). Having disentangled himself from the Hegelian devil, Lakatos is doomed to find the Popperian deep blue sea waiting at the end of the road he travels.

10. Final remarks

On the one hand, Lakatos is the Hegelian in disguise who advocates method as an objective surrogate for truth; *on the other hand*, he openly appeals to some sort of metaphysics to bridge the gap between method and truth. *On the one hand*, Lakatos claims a normative role for the heuristic (as synonymous of methodology) which he associates with the objective dialectical pattern of the growth of knowledge*; on the other*, he separates (normative but research-programme laden heuristic) from (retrospective) methodological appraisals. *On the one hand*, Lakatos appeals to Hegel and criticises Popper for leaving history outside the picture; *on*

the other, he denounces Hegel's misuses of history (in its cunning of reason version) and appeals to Popper's fallibilism. *On the one hand*, Lakatos claims there is no such thing as an "immutable statute law" to distinguish good and bad science. Taken literally, this would imply that our theory of rationality is not eternal and apriori, but rather submitted to continuous change under criticism (as is suggested by the title "The Changing Logic of Scientific Discovery"). *On the other*, he distinguishes between implicit "methodology" and explicit methodologies, whereas the former has to be fixed if we want to account for the changes in the latter (as is also suggested by "The Changing Logic of Scientific Discovery"!).

In this paper it has been shown that the gap between these different stands can be bridged only partially because of the very nature of Lakatos's philosophical project. This is also why, from time to time, Lakatos is deemed to concede something to historicism, anarchism and elitism. After all, Popper's clear-cut falsificationism, Feyerabend's anarchic slogans and élitist positions equating might and right immediately clear up science from its obscure aspects, but - as Lakatos himself has put it - they are like "*Lucifer,* the chap who brings *false* light", while "I'm shrouding you in the darkness of truth." (from Lakatos to Feyerabend, January 23rd, 1973, in Motterlini, ed. 1999, p.313)

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