INTRODUCTION

The present collection is the result of a cooperation between the Institute Vienna Circle and the Institute for History and Philosophy of Science of Eötvös University, Budapest, which was dedicated to the philosophy of science in the Austro-Hungarian context. Probably no other protagonist of 20th century philosophy of science fits better into such a framework than does Imre Lakatos (1922–1974). The book *Proofs and Refutations* which made him famous, declares itself as a continuation of György Pólya's works on mathematical heuristics and plausible reasoning and, accordingly, stands in the eminent tradition of Hungarian mathematics. When Lakatos subsequently embarked upon general philosophy of science and developed his methodology of scientific research programs (henceforth MSRP), he became involved in at times heated debates with three offshoots and opponents of the Vienna Circle: Karl Popper, Thomas S. Kuhn and Paul Feyerabend. Before giving a short overview of the present volume, we shall briefly sketch this historical setting in order to illustrate the motivation of the Institute Vienna Circle to start its new book series with a volume on Imre Lakatos.

After completing his Cambridge Ph.D. disseration in 1961, Lakatos came to the London School of Economics to work under Popper. He subsequently attempted to extend – though with nuances of his own – Critical Rationalism to mathematics. Seeking more and more an independent stance in the late 1960s, Lakatos' relationship with Popper developed into a thematic rift, culminating in personal tensions. Lakatos' recently published last lectures are quite telling in this respect. Nevertheless, both Lakatos and Popper stood side by side in struggling against the 'new epistemology' of the 1970s. While they emphasized the unavoidability of a criterion demarcating science and scientific rationality from non-science, the 'new epistemologists' – among them Thomas Kuhn and Paul Feyerabend became the most prominent – insisted on the indivisibility of the whole of scientific practice, in particular on the impact of societal factors, institutions, ideologies, etc., upon scientists and their research programs. They held that – even for philosophical purposes – the actual course of history could not be replaced by a rational reconstruction of history, and that there existed no methodology guaranteeing scientific success. Hence no absolute justification of scientific rationality could be reached.

G. Kampis, L. Kvasz and M. Stöltzner (eds.),
Appraising Lakatos: Mathematics, Methodology and the Man, ix–xiv.

During the last two decades, historical investigations into Logical Empiricism have brought to the fore various historical facts pertaining to the battles waged during the 1960s and 1970s. On the one hand, Popper's relations with the Vienna Circle were much more complex than his claims to the effect that he had simply 'killed' Logical Positivism suggest; in particular, the *Logic of Scientific Discovery* played its part in the movement's discussions during the 1930s. Victor Kraft, who was, together with Béla Juhos, the last local representative of the Vienna Circle after 1945, shared some of Popper's core ideas. In 1956, Kraft recommended Lakatos to Cambridge to obtain a second Ph.D. under Braithwaite. The correspondence between Lakatos and Kraft preserved in the Lakatos Archive at LSE indicates that Kraft was basically the only Vienna Circle member whose writings – apart from Carnap's of course – Lakatos had thoroughly studied. Lakatos especially appreciated Kraft's early philosophy of mathematics.

On the other hand, Kuhn's seminal book *The Structure of Scientific Revolutions* appeared in the *International Encyclopedia of Unified Science.* When Otto Neurath had launched this project in the mid 1930s, he did so in the attempt to replace *the* system of science by an ongoing cooperation of scientists in the Vienna Circle's spirit. Because of the war and Neurath's untimely death in 1945, a tiny core of 19 booklets, along with bibliography, were the only elements of his enormous plan to make it into print until 1970. A closer look at the writings of Logical Empiricists after 1945 reveals that many Kuhnian themes were not unfamiliar or outlandish to that movement. Yet neither Philipp Frank's nor Otto Neurath's respective activities became widely known until the fairly recent Neurath renaissance. Paul Feyerabend, a physics student in post-war Vienna, started his philosophical career in Kraft's circle. In a way, his "anti-system" and "anti-method" crusades can be seen as pointedly exaggerating Neurath's criticism of Popper's pseudo-rationalism.

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A quarter of a century after Imre Lakatos' untimely death, and more than a decade after the hitherto immense popularity of Lakatos-style case studies has significantly decreased, it seems to be a suitable moment – and at a sufficient distance – to critically reevaluate the old debates: Is our historical picture of the classical controversy over scientific rationality still adequate? Are Lakatos' methodological proposals, above all the MSRP, still promising today, and can they be suitably refined so as to accommodate the lessons of the large number of detailed case studies ensuing from them? Or do they contain limitations and deficiencies in principle? Discussion, thus, of possible excess content – to phrase it in Lakatosian terms – will be one of the objectives of the first section of the present collection. Although the history of philosophy of science is itself presently becoming a research program, the goal of the papers assembled therein is not merely retrospection. To the contrary, the so-called 'science wars' ensuing from the Sokal hoax taught us that good arguments in defense of scientific rationality are still timely. Present combatants sometimes reveal a certain reluctance to draw lessons from classical debates. In this respect, this section might be a useful reminder of a former state of the art. Moreover, Lakatos' thinking still bears unexplored perspectives. He was a philosopher of many roots, some of which, such as the thinking of Hegel and Lukács, constitute a rather atypical background for a 20th century philosopher of science, especially for one who had started out in mathematics.

Recent publications on Lakatos in scientific journals demonstrate, above all, a revived interest in his philosophy of mathematics which represents one of those few approaches that are not centered around (or limited to) foundational issues, but center around mathematical practice as such. On this, the second section focuses. After all, Lakatos regarded himself primarily as a philosopher of mathematics. During the last years of his life, he was seeking to apply the MSRP to mathematics. Although in some papers from the 1970s one finds footnotes commenting upon *Proofs and Refutations*, Lakatos could not close the circle himself, and his papers do not provide a real clue about his plans. Today, it seems clear that major modifications in both the MSRP and his philosophy of mathematics are required to reach that goal. Mathematics is much more finely subdivided into sub-disciplines than are the empirical sciences. On the other hand, mathematical research is not so strongly concentrated around a small number of problems such as, say, hunting the Higgs particle or deciphering human DNA. More generally, mathematicians typically do not dispute a common set of facts. Instead, aesthetic criteria, such as conceptual simplicity or the beauty of a proof, essentially determine the quality of a mathematical argument; and they are typically at issue whenever mathematicians debate whether a theorem is not only true but really final, or whether we have fully understood a mathematical structure. Interestingly, the Hungarian tradition of mathematics to which Lakatos is indebted through György Pólya, Alfréd Rényi, and Arpád Szabó was much more problem-oriented than, for instance, the various French or German schools. Especially in Pólya's writings, heuristic beats justification by far. This stance not only opens many interesting perspectives for the connection between mathematics and the sciences, but is also most attractive in mathematics teaching.

Lakatos' philosophy emphasizes mathematical growth and conceptual evolution over the justification and consolidation of a theory's structure. But in his charges against Euclideanism, and through the all-pervasive fallibilism behind them, the Lakatosian dialectics of proofs and refutations visibly overshoots the mark. Although mathematical rigor is indeed often suspended in the early phase of a budding research program, and although understanding may play a role far more important than justification, the standards of rigor furnished by a commonly accepted metatheory unite the variety of mathematical sub-disciplines. Ultimately, all doubts will be cast on the axioms. Already the rift between the first and the second parts of *Proofs and Refutations* reveals that Lakatos was at odds with the axiomatic method, which utterly characterizes 20th century mathematics. For many modern concepts and theorems – even for those which have found applications in physics – there simply are no informal ancestors. Still, most mathematicians maintain that mathematical intuition must somehow precede proof. If this problem could be remedied, Lakatos' quasi-empiricism might be rather promising for assessing the spectacularly successful encounters between mathematics and theoretical physics.

In Lakatos' mature thinking, some apparently contradictory influences have merged, including Hegel and Popper, mathematics and critical fallibilism. Interestingly, some core themes, such as the idea of research programs, are foreshadowed already before his emigration. Both tendencies can well be seen from the paper "Modern physics, modern society" that was published in Hungarian in 1947. There, a rather standard Communist exercise in ideology suddenly develops into a discussion of L. Susan Stebbing's *Philosophy and the Physicists.* We provide here a full translation of this text mainly because it represents a major part of Lakatos' Hungarian Ph.D. dissertation defended in the same year. The dissertation as a whole is missing in the University archives at Debrecen, but the opinion of his supervisor Sándor Karácsony proves that "Modern physics, modern society" represented its first part. We thank Gábor Kutrovátz for translating both documents into English. The documentary section is rounded off by a fairly complete bibliography of Lakatos' Hungarian papers written prior to his emigration in 1956. Discussing the philosophical import of Lakatos' text, the different personal histories of the editors came to the fore. To a Western reader, both parts are somewhat separated, and historical distance renders them mainly just a document. Having been constantly imbued with – and often suffered under – the idea that there are deterministic laws governing class history in the same sense as Newtonian mechanics, the East-European reader gains quite another picture.

Until his imprisonment in 1950, Lakatos was a radical communist who considered his own deeds – "unforgivable" as they appear today – to be part of a historical process governed by dialectical laws, which could be rationally reconstructed. While such a parallel must not be overstressed, it does provide an interesting perspective from which to start studying Lakatos' biography. To be sure, only few members of his generation had the boring sort of biography that philosophers are alleged to have. Yet Lakatos' was exceptionally rich in curves. There have been many intellectual émigrés from the communist East, but only a few of them made such a significant shift from the far left to the far right. The rupture in his biography became almost tantamount to meeting two personalities. His earlier political involvement were stories he hardly ever told anybody in the West. As rumors today abound, a comprehensive volume on Lakatos has to tell them and contrast them to his political activity at LSE. But there is more at stake than just being comprehensive. The historical investigations into scientific philosophy in exile have set a high standard in contextualizing this tradition with the general history of intellectual emigration. As an Austro-Hungarian joint project, the final section of this volume intends to provide a certain basis for undertaking similar scrutiny with Lakatos in the future.

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The present volume emerged from two workshops which were held in Vienna, from 12–14 September 1997, and Budapest, from 30–31 October 1997. The second was very close to what would have been Lakatos' 75th birthday, which provided an excellent occasion for assessing the genuinely Hungarian aspect of his personality. Many contemporaries of his are still alive; some of them joined forces with him, and some suffered from his deeds. We held a panel discussion moderated by Lee Congdon, in which took part Alex Bándy, Alex Bellamy, György Litván, Jancis Long, Sándorné Kántor, Gábor Palló, Éva Pap, László Ropolyi, Miklós Szabolcsi, and Gábor Vajda. Panelists also reported on their experiences and difficulties in securing biographical material on Lakatos. But at the core of the exchange were the recollections of Lakatos' political activities, and of science policy of the 1950s, in general. So this event, in a way, became a part of Hungary's coming to terms with its Communist past. The fact that our initial plan to hold this discussion in the Eötvös Collegium – an institution which was closed down under the influence of its former student Imre Lakatos – was first accepted and then turned down by its current Director, is a clear sign how present this past is still for many. For a certain time, the editors considered reproducing selected passages from the tape of his panel discussion in the present volume. We refrained from doing so because a substantial part of the facts presented there is contained in Jancis Long's contribution, and the 'event-like' character of the panel discussion cannot be put into print.

A series of photographs of Imre Lakatos from different epochs of his life completes the volume. The pictures show him with his second wife Éva Pap, in scientific and political company. We are very grateful to Éva Pap for granting us permission to publish these photos from her personal archive for the first time. She holds the copyright for all of them.

On July 26th, 1999, Professor John Watkins suddenly passed away. It had been a great honor for us that he accepted our invitation to the Budapest workshop and we well recall his talk and the most interesting personal remarks about the linkage of Imre Lakatos' two lives. There have appeared many thoughtful obituaries since. With some delay we present a collection of papers that also show the important role of John Watkins within post-war philosophy of science.

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The cooperation that led to the present volume would have been impossible without the constant and generous funding of a bilateral organization

John Watkins†

The Propositional Content of the Popper–Lakatos Rift

Imre Lakatos telephoned me on the morning of February 2, 1974. He was in a fury. He had received page proofs of his contribution to the Schilpp volume on the philosophy of Karl Popper. He had been promised that his notes would appear as foot-notes; he now found that they were collected together as end-notes. He urged me to join him in getting the volume held up while this was put right. The volume was already several years behind schedule and I declined. It was our last conversation. He died later that day, going down, one might say, with guns blazing. The volume appeared a few months later (with an editorial apology about the placing of his foot-notes).[1]

The title of his paper, 'Popper on Demarcation and Induction,' harked back to the two problems which the young Popper had taken as fundamental and which he claimed to have solved at a stroke. The previous orthodoxy, as Popper saw it, was that science is differentiated from metaphysics, pseudo-science and other kinds of non-science, by its use of an inductive method. But induction, as Hume's deadly analysis had showed, is without justification – the glory of science but the scandal of philosophy, as C.D. Broad (1952, p. 143) had put it in 1926. Popper sought to solve the demarcation problem by saying that science consists, not of verifiable hypotheses, but of falsifiable conjectures which it tests as rigorously as it can. However well they survive testing they are never verified or quasi-verified or probabilified or confirmed in any inductive sense. They can never be more than well-tested and unfalsified *conjectures*. A theory such as Freudian psycho-analysis, although it may seem to gain a great mass of confirmations, is only pseudo-science because (or so Popper claimed) it is untestable. This solution of the demarcation problem solves the problem of induction by eliminating it; science needs only deductive inferences, from premises to a prediction and from a falsified prediction to the falsity of the conjunction of premises which entail it. "There is no need even to mention 'induction'," Popper declared (1959, p. 315, italicized in the original); it plays no role in science.

Besides demarcation and induction, Lakatos' paper in the Schilpp volume took up a third issue; he brought in, and then discussed and amended with much appeal to the (largely unspoken) value-judgements of the scientific elite, a meta-criterion by which Popper's and others' demarcation criteria may be assessed and amended. My heart rather sinks when a discussion turns

G. Kampis, L. Kvasz and M. Stöltzner (eds.),
Appraising Lakatos: Mathematics, Methodology and the Man, 3–12.

to meta-criteria; I feel like protesting, "First provide me with the meta-meta-criterion by which I may judge your meta-criterion." In the present discussion I am going to stick to Lakatos' challenges to Popper over the two main issues. This will provide quite enough material.

When Popper read Lakatos' Schilpp volume paper, late in 1969, he responded angrily. The dispute escalated into a bitter quarrel whose reverberations continue to this day. Spectators have tended to take sides, some friends and pupils of Lakatos concluding that an old tyrant had been humbled, while others regarded his paper as a shameless work of deconstruction. Among the latter is Joseph Agassi. He writes: "Lakatos died suddenly in 1974 ... His posthumous contribution to *The Philosophy of Karl Popper* of 1974 says of Popper that he has made no contribution to philosophy worth mentioning" (1993, p. 9). Does it? Lakatos' paper opens with these sentences:

> Popper's ideas represent the most important development in the philosophy of the twentieth century; an achievement in the tradition – and on the level – of Hume, Kant, or Whewell. Personally, my debt to him is immeasurable: more than anyone else, he changed my life. I was nearly forty when I got into the magnetic field of his intellect. His philosophy helped me to make a final break with the Hegelian outlook I had held for nearly twenty years (1978, p. 139).

You may say that those are only opening compliments. Well, in the body of the paper he mentioned, quite truly, that some great scientists whose judgement had been warped by previous philosophies had been helped by Popper's philosophy (1978, p. 154). More relevant to an issue that will come up later is Lakatos' evaluation of Popper's theory of verisimilitude. He said that it was "an achievement marvellous both in its simplicity and in its problem-solving power. It became possible, for the first time, to define *progress* even for a sequence of false theories" (1978, p. 156). Agassi's memory let him down here.[2]

Popper's Reply contains a passage which is out of line with his critical rationalism. He said of a thesis of Lakatos: "Were the thesis true, then my philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting" (1974, p. 1005). The inference the reader was presumably intended to draw was that since his philosophy of science is not completely uninteresting Lakatos' thesis is not true. But why should finding his philosophy of science to be greatly mistaken render it uninteresting? According to his critical rationalism, the exposure by a pupil of a master's great mistake is the lifeblood of intellectual progress. He praised Thales in this connection. By contrast with dogmatic schools which seek to preserve a doctrine pure and unchanged, Thales created a new type of school based on a new relation between master and pupil (1963, pp. 149–150). Thales had explained the stability of our earth by saying that it is held up by water on which it floats (when it is rough, earthquakes occur). His pupil Anaximander explained it by saying that the earth "is held up by nothing, but remains stationary owing to the fact that it is equally distant from all other things." This implies that Thales' explanation is greatly mistaken; does that have any tendency to render it uninteresting? Not in Popper's eyes: he found it a 'beautiful theory.' Or consider Popper on Kant. Popper made a short but devastating analysis of confusions in Kant's idea of synthetic apriori truth (1963, pp. 47–8). Since that idea was central to Kant's whole philosophy, Popper was convicting him of a great mistake; but Popper would not have dreamt of saying that this renders Kant's philosophy uninteresting. In the Preface to *The Open Society* Popper said that great men may make great mistakes; there was no suggestion in that book that ideas of Plato and Marx that are mistaken are thereby uninteresting.

One can regard the Popper-Lakatos dispute as a World 2 contest and ask how the contestants fared: was there an outright winner, and if not, did either man win on points? But I want to view it as a World 3 contest, and see how the underlying propositions fared. More specifically, I want to find out what revisions, if any, to the propositional content of Popper's philosophy of science are called for by Lakatos' challenge. However, that is easier said than done. The propositions involved here are sometimes distorted or disguised or even hidden by the words of our two protagonists, and to get at the World 3 content we will need to work through a good deal of World 2 material.

The lack of a straightforward correspondence between written sentences and propositional content takes different forms with the two men. I begin with Lakatos. In his case the problem is often that he was writing in a coded way or using a kind of Doublespeak. In his Schilpp paper and elsewhere he talked a good deal about different falsificationists; for instance, 'the' naive or dogmatic falsificationist and 'the' methodological falsificationist. For a time he seemed to be pairing the former with a mythical 'Popper' invented by Ayer, Medawar and others, and the latter with the real Popper; thus he often brought in quotations from Popper in the course of presenting 'the' methodological falsificationist's position (1978, p. 24). Yet at one place he claimed to have exposed the main weakness of *Popper's* naive falsificationism (1978, p. 150), and at another place, where he referred to "our savage falsificationist" (1978, p. 26), the foot-note trail leads back to Popper. He also operated for a time with numbered Poppers, 'Popper_0' (invented by Ayer, Medawar et al.), 'Popper_1' (the naive methodological falsificationist), and 'Popper_2' (the sophisticated methodological falsificationist), and then agonized over which is the real Popper. It would have been much easier for the reader if he had discarded this proliferating apparatus in favour of unambiguous phrases like 'According to Ayer, Popper holds ...,' 'Popper holds ...,' 'In opposition to Popper I hold ...'.

Just what did Lakatos say about Popper and the problem of induction? After mentioning that Popper in his early philosophy had suggested a purely *negative* solution, he added that Popper's "later philosophy (based on the idea of truth-content and verisimilitude) involved a shift of the problem and also a *positive* solution of the shifted problem; but, to my mind, he has not yet realized the *full* implications of his own achievement" (1978, p. 140). It sounds as though Popper, with his theory of verisimilitude, had in his hands a positive solution though one he had not yet fully articulated. Lakatos also spoke of three prongs in Popper's anti-inductivist campaign, declaring himself in full agreement with Popper concerning the first two. Concerning the third he reported:

> I had long discussions with Popper in 1966–7 about these issues; I profited immensely from them. But I was left with the impression that on what I called the 'third prong of his anti-inductivist campaign' we may never see eye to eye. *The reason is not that our disagreement is too big; but that it is so very small* (p. 164, his italics).

What was this minuscule disagreement? It was nothing less than whether Popper, the 'scourge of induction' as Lakatos called him (1978, p. 161), should introduce a synthetic inductive postulate to link corroboration-appraisals to verisimilitude-appraisals. I suggest that Lakatos was using a kind of Doublespeak, flattering on the surface but less flattering underneath. I would decode the above sentences of Lakatos as follows:

> Popper's negative solution didn't work. He later discovered the idea of verisimilitude. Around 1966–7 I suggested to him that he could obtain a positive solution by linking corroboration and verisimilitude with the help of a synthetic postulate. He opposed this suggestion.

Lakatos' idea was that without such a link the corroboration a theory gains when it passes a searching test remains no more than an excellent move in the 'game of science.' Such a link is needed to turn it into an advance in our understanding of the world.

In Popper's case the problem of getting at the propositional content of his sentences is different. He was not in the habit of writing sarcastically or with his tongue in his cheek (though he did describe Hegel as a "master logician" employing "powerful dialectical methods"). Where an individual sentence of his should not be taken quite literally this is usually because of exaggeration. I quoted him earlier saying that if a certain thesis of Lakatos were true his philosophy of science would be completely mistaken; but it is not logically possible for any body of propositions to be *completely* mistaken. Or take another sentence of his: "A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history" (1963, p. 35): on *every* page, including the sports pages? More serious difficulties are sometimes raised by the ways he deployed sentences. It is an interesting exercise to ask whether Popper ever explicitly addressed Lakatos on the question of induction. His Replies in the Schilpp volume are in five sections; section II is entitled 'The Problem of Demarcation' and section III 'The Problem of Induction.' Lakatos was accorded fifteen pages at the end of section II. His name does not appear, apart from two incidental mentions, in section III, whose opening sub-section is entitled 'My solution of Hume's problem of Induction.' (Soon afterwards this was expanded into Chapter I of *Objective Knowledge* which opens with the words "I think that I have solved a major philosophical problem: the problem of induction.") And Popper did not refer to Lakatos in any subsequent publication. So the answer appears to be that Popper did not explicitly address Lakatos on the question of induction.

But that turns out to be incorrect. We found that the gist of the propositional content of the passages from Lakatos quoted earlier was: corroboration needs to be linked to verisimilitude. Now a corroboration appraisal as understood by Popper sums up how a theory has performed so far and says nothing about future performance; by contrast, verisimilitude appraisals have no temporal restriction. So a principle linking corroboration to verisimilitude in the required way would have an inductive character. And in the answer which Popper made to Lakatos under the heading 'The Problem of Demarcation' there occurs the following remark: "I *did* suggest in *Conjectures and Refutations*, Chapter 10, that the degree of corroboration may be taken as an indication of verisimilitude" (1974, p. 1011, his italics). That little sentence means that the propositional content of Popper's earlier publications already contained the inductive postulate called for by Lakatos. As I put it elsewhere (1984, p. 283), the whiff of induction which Lakatos invited Popper to introduce into his philosophy was already there.

Lakatos generated at least two pseudo-disputes with Popper as a result of what I call his Research-Programme imperialism. Let p be a proposition held by Popper and q be a proposition held by Lakatos, where p and q are, from a logical point of view, mutually compatible. A pseudo-dispute arises if Lakatos nevertheless asserts that p must be supplanted by q, although he makes no case for this and his reason for it is only that it is good propaganda for his Research-Programme methodology. Let p be the proposition that in science we appraise competing *theories* while q is the proposition that in science we appraise competing *research-programmes*. It might likewise be held that in architecture we appraise individual buildings and that in architecture we appraise streets. Neither kind of appraisal need exclude the other. But now imagine that within the architectural profession a militant Street Party starts up whose main plank is that street-appraisals must supplant building-appraisals since *streets* are the basic unit of appraisal in architecture. The main plank of Lakatos' Research Programme Party was, in his words, "The basic unit of appraisal must not be an isolated theory or conjunction of theories but rather a *research programme*."[3] I accept that scientific research programmes, about which he said interesting and important things, do constitute genuine units of appraisal. But what argument did he offer for his conclusion that theories, which he usually spoke of as 'isolated,' do not also constitute genuine units of appraisal? After saying, rightly, that a scientific theory is judged in relation to its predecessors, he immediately added: "Then, of course, what we appraise is a *series of theories*" (1978, p. 33). That is rather as if, after saying that a building has to be appraised in relation to neighbouring buildings, he had added: "Then, of course, what we appraise is *streets*." And in any case a series of theories is not yet a research programme: the series terminates with a last theory, whereas a

DONALD GILLIES

LAKATOS' CRITICISMS OF POPPER

1. INTRODUCTION

Imre Lakatos' thought is very fascinating, but it is also fascinating to study the development of his thought. This development is not at all linear and uniform. On the contrary it is full of great upheavals, and dramatic changes of direction. The works of philosophers who often change their opinion, usually give the impression of a lack of any coherent position. Strange to say, the works of Lakatos, despite his frequent changes of opinion, do not give this impression. The reason for this paradoxical situation is perhaps that at the centre of Imre Lakatos' philosophy there is a dialectical concept of continual change. His own changes of opinion are, consequently, compatible with his philosophical vision of the world.

In this paper, I want to discuss only one of Lakatos' great changes. In the years 1963–4 when he published *Proofs and Refutations*, he was a follower of Popper and a defender of Popperian philosophy. He wrote in the introduction:[1]

> The purpose of these essays is to approach some problems of the *methodology of mathematics*. I use the word "methodology" in a sense akin to Pólya's and Bernays' "heuristic" and Popper's "logic of discovery" or "situational logic."

It is obvious, moreover, that the full title of the work: *Proofs and Refutations. The Logic of Mathematical Discovery* refers to two of Popper's most famous books: *Conjectures and Refutations*, and *The Logic of Scientific Discovery*. Lakatos has the same attitude towards Popper in his 1968 article: *Changes in the Problem of Inductive Logic*. In this article with a title so characteristic of Lakatos, he defends Popper's theory of corroboration against Carnap's theory of confirmation. In 1973, only 5 years later, the situation was very different. In that year Lakatos gave his last lectures on method at the London School of Economics. These lectures were published for the first time in Italian translation in 1995 in *Sull'Orlo della Scienza*, edited by Matteo Motterlini. In them, Lakatos attacks Popper in a ruthless fashion. Lakatos still sees some merit in Popper's political philosophy, but says that there is nothing of value in Popper's philosophy of science. To quote Lakatos himself:[2]

G. Kampis, L. Kvasz and M. Stöltzner (eds.), Appraising Lakatos: Mathematics, Methodology and the Man, 13–22.

Allegedly, Popper's three major contributions to philosophy were: (1) his falsifiability 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" ... what is it called? *The Open Society and its Enemies*. ... *The Open Society* is frankly a literary masterpiece: not being a political philosopher I cannot comment on its contents, but I certainly think it is a marvelous book. So, in conclusion, two-thirds of Popper's philosophical fame is based mis-judgement.

To this Lakatos adds a little later:[3]

I think that the fact Popper's philosophy survived for so long is a sociological mystery. Popper's immortality is secured by this idiotic result.

How did such an enormous change in Lakatos' attitude to Popper take place? In fact I was working on my PhD under Lakatos' supervision when this change occurred. So I hope that it will be useful if I give, in the next section, some personal reminiscences of Lakatos at that time.

2. SOME REMINISCENCES OF LAKATOS

My undergraduate degree was from Cambridge, where I studied mathematics for two years, and then philosophy for another two years. In my last academic year as an undergraduate (1965–6), I formed the plan of doing a PhD on the philosophy of mathematics. Consequently I read all the recent articles on this subject in the hope of discovering some interesting research line to follow. After this survey of the literature, I had no doubt that the most interesting recent article on the philosophy of mathematics was *Proofs and Refutations*, published two years previously in the British Journal for the Philosophy of Science. I decided to do for my PhD a historical/philosophical study of some branch of mathematics on the model of *Proofs and Refutations*. So I wrote to the author Imre Lakatos in the summer of 1966 to ask if he was willing to take me on as a PhD student. Lakatos replied suggesting that I should come to meet him at the London School of Economics.

My first meeting with Lakatos was certainly an occasion to remember. After a little philosophical discussion, I uttered the name of Wittgenstein. Lakatos replied: "Wittgenstein was the biggest philosophical fraud of the twentieth century." This statement really came as a surprise to me, since in those days the cult of Wittgenstein was still very strong at Cambridge, where I had been studying philosophy. Indeed my first year of philosophy at Cambridge consisted, for three quarters of the time, of reading Wittgenstein with care and attention. So I replied: "Dr Lakatos, what you say is truly surprising for me because I have just finished writing an essay in which I maintain that there are close links between your concept of mathematical proof and Wittgenstein's." The next time I met Lakatos, he said to me: "Regarding Wittgenstein, I looked through my copy of his *Remarks on the Foundations of Mathematics*, and I was surprised to find that I had written enthusiastic notes in the margins. But these notes were written in Hungarian which means that I must have been written them ten years ago, just after I had arrived in England." In fact Lakatos and I got on extremely well during the first few years of our acquaintance, although, unfortunately, we began to quarrel for various reasons later on. After I had known him for a few months, Lakatos said to me on one occasion: "Donald there is a problem about you. You are very difficult to understand because of your English accent." In fact in those days the philosophy department at the London School of Economics had a lively international atmosphere, and was full of Austrians, Americans, Hungarians, Italians, etc. So an English accent was really quite rare.

I began working on my PhD with Lakatos in October 1966. The first thing Lakatos told me was that I should begin by reading the entire works of Popper, because they were essential. Lakatos was truly enthusiastic about the philosophy of Popper in those days. Initially, however, I was reluctant to follow these instructions, because up to that time Popper had written little on the philosophy of mathematics. However, being a well-behaved student, I began to read Popper carefully, and, within a short while, there was no need for Lakatos to give me any further encouragement, because I was finding Popper's writings ever more interesting. At that time Lakatos was working on the article already mentioned: *Changes in the Problem of Inductive Logic.* As I said, this article is a defence of Popper's theory of corroboration against Carnap's theory of confirmation. For this reason, I also read Carnap's *Logical Foundations of Probability,* and so became more and more interested in the foundations of probability. As a result of all this I ended up by writing my PhD not on the history and philosophy of mathematics, but on the foundations of probability. A problem which particularly interested me in that area was one already discussed by Popper concerning the falsifiability of probability statements. In fact my original plan was only carried out in 1992, not in the form of a PhD, but in that of a collection of essays by various authors. I was the editor of this collection on *Revolutions in Mathematics,* and in the preface I made an acknowledgement to Imre Lakatos as the inspirer of this type of research.

Returning to 1966, the reason why Lakatos was engaged in a defence of Popper against Carnap at that time was the following. In the previous year (1965), Lakatos had organised a conference on the philosophy of science and mathematics in London. Many famous philosophers of the time attended, including Carnap, Kuhn and Quine. Lakatos was the editor of the conference proceedings, and decided to add to these proceedings two essays, both defending Popper, but against two different opponents, namely Carnap and Kuhn. All went well while Lakatos was writing against Carnap. But the defence of Popper against Kuhn turned into a critique of Popper, and the development of a new approach to scientific method – Lakatos' methodology of scientific research programmes. In the next section I will consider Lakatos' criticisms of Popper.

3. Lakatos' Criticisms of Popper

In this context the key work of Lakatos is *Falsification and the Methodology of Scientific Research Programmes*, published in 1970. His criticisms of Popper are based fundamentally on the Duhem thesis. It is true that in the appendix of the article just cited, Lakatos speaks of the Duhem-Quine thesis, but in fact Quine's philosophy had little impact on Lakatos, while he used to study Duhem with the very greatest attention. It is not by chance that Lakatos, in one of the passages already quoted from his last lectures on method, speaks of[4] "his falsifiability criterion – I think is a step back from Duhem ...".

Duhem expounds his thesis as follows:[5]

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.

If we accept the Duhem thesis, it would seem to be impossible to falsify an isolated hypothesis, and hence that the falsifiability criterion is unsatisfactory. The Duhem thesis poses the following further problem. If experience disagrees with a group of hypotheses, how can we know which of the hypotheses should be changed? Lakatos proposes his methodology of scientific research programmes as a solution to this problem of Duhem's. According to Lakatos, a scientist always works in the context of a research programme, which has a *hard core* or *negative heuristic*. Lakatos claims that[6]

> This "core" is "irrefutable" by the methodological decision of its protagonists: anomalies must lead to changes only in the "protective" belt of auxiliary, "observational" hypotheses and initial conditions.

So if experience disagrees with a group of hypotheses, the scientists do not change those hypotheses which constitute the hard core of their programme. This reduces the difficulty of choice, and, moreover, each scientific research programme has a *positive heuristic* which gives advice on the way in which the programme should be developed.

These are the fundamental ideas of the methodology of scientific research programmes. Here I will not give further details, because this methodology is now well-known. Instead I would like to present the reasons why I did not, and still do not, accept this Lakatosian account of scientific methodology.

4. Some Criticisms of the Methodology of Scientific Research Programmes

When Lakatos began to develop his new methodology, I had, following his instructions, just finished reading the works of Popper. I must say that I had found Popper's philosophy very much to my taste. I do not have Lakatos' capacity for making sudden changes in my intellectual opinions, and I was, therefore, reluctant to give up the new faith which I had just acquired. Moreover, in my researches on the problem of the falsifiability of probability statements, I had discovered that the concept of falsification gives a reasonable foundation to the theory of statistical testing developed by Fisher and used by the majority of statisticians. Falsificationism therefore succeeds in providing a simple and satisfying explanation of probability which accords well with the most widely diffused statistical practice. This situation convinced me that there was a certain validity in the concept of falsification, despite the criticisms based on the Duhem thesis, and that Lakatos was therefore wrong to abandon falsificationism completely.

When Lakatos began to develop the methodology of scientific research programmes, the philosophy department at the London School of Economics was divided into two groups: those who accepted Lakatos' new ideas, and the 'old guard,' who tried to defend Popper's philosophy and criticize the new methodology. After Lakatos' death in 1974, I continued for many years to think about these problems. I finally reached the conclusion that Lakatos was correct in thinking that Popper's philosophy needs to be modified in the light of the Duhem thesis. However, I have come to accept the methodology of scientific research programmes only in part and with modifications, and so think that the problem created by the Duhem thesis needs to solved in a way different from that suggested by Lakatos. In the rest of this section, I will explain the reasons why I do not accept Lakatos' position, or, rather, why I accept it only in part. In the next and final section, I will give a sketch of my own position regarding falsificationism and the Duhem thesis.

At the most abstract level, the difference between my position and that of Lakatos can be explained as follows. I accept the distinction between the *discovery* of scientific hypotheses and their *justification*. Lakatos, by contrast, tried to reduce the problem of the *appraisal* of knowledge to that of the *growth* of knowledge. My criticism of Lakatos is that this reduction does not work, and that, we need some notion of the empirical confirmation of theories in addition to those of the progress and degeneration of research programmes. Of course this notion of empirical confirmation need not be the same as Carnap's – indeed I think it will be closer to Popper's concept of corroboration. This then is my general position. I will begin to expound it in more detail by giving a quotation from Lakatos where he explains his idea of the reduction of *appraisal* to *growth.*[7]

> But then two new problems arose. The *first* problem was *the appraisal of conjectural knowledge*. ... The *second* problem was *the growth of conjectural knowledge*. ...
>
> In this situation *two schools of thought emerged*. One school – *neoclassical empiricism* – started with the first problem and never arrived at the second. The other school – *critical empiricism* – started by solving the second problem and went on to show that this solution solves the most important aspects of the first too.

MATTEO MOTTERLINI

PROFESSOR LAKATOS BETWEEN THE HEGELIAN DEVIL AND THE POPPERIAN DEEP BLUE SEA

1. INTRODUCTION: A POP–HEGELIAN PHILOSOPHER

When Lakatos unexpectedly died in February 1974, Paul Feyerabend was invited to write an appreciation of his friend for the *British Journal for the Philosophy of Science.* He portrayed Lakatos as "a fascinating person, an outstanding thinker and the best philosopher of science of this strange and uncomfortable century"; as "a *rationalist,* for he thought that man had the duty of using reason in his private affairs as well as in any enquiry concerning the relation between himself, nature, and his fellow men"; and as "an *optimist,* for he thought that reason was capable of solving most of the problems arising in the course of such an inquiry" (Feyerabend, 1975b, p. 1).

Just a few years before Feyerabend had claimed, in a rather different style, that his fellow was a "*big bastard* – a Pop–Hegelian philosopher born from a Popperian father and an Hegelian mother" ("Lakatos–Feyerabend Correspondence," fothcoming in Motterlini, ed., 1999).[1] In fact, some of Lakatos' 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. I shall argue, however, that for the same reason, an tension in Lakatos' 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,[2] Lakatos too was caught between the devil of Hegelian historicism and the deep blue sea of Popperian fallibilism.

I shall provide an analysis of the roots and objectives of Lakatos' philosophical programme especially in the light of 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).[3] Instead of giving a complete description of the available material, I shall emphasise those items which illustrate most clearly Lakatos' 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.

G. Kampis, L. Kvasz and M. Stöltzner (eds.), Appraising Lakatos: Mathematics, Methodology and the Man, 23–52.

2. A CRITICAL MARXIST POLEMICIST

The contributions written by Lakatos for Hungarian literary and academic journals in the early Fifties show how Lakatos' 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 Petőfi Circle pedagogy meeting the same year, he argued for encouragement of a critical attitude, absence of censorship and science as a guide to the party instead of the other way round (Archive, 1.9).[4]

These writings possess the sharpness, originality, forcefulness, clarity and, at the same time, ambiguity, which was to distinguish all of Lakatos' 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 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. In libraries the pile of strictly confidential stock was getting higher and higher. [...] It is the basis of scientific education to train students and research students to respect facts, to demand exact thinking and proofs. Stalinism, on the other hand, branded these very demands as "bourgeois objectivism." Under the banner of party-minded science, a large (even, we could say, world-scale) attempt has been made to create fact-free and proof-free scholarship or science. (For example, Lysenko's and Lepichinskaia's biology.) The extermination of facts was often carried out under the pretext of a "Marxist" fight

against empiricism – an invisible and frequently non-existent "salient feature" was given first importance over the miserable and mostly unpalatable "phenomena." The victims of this fervid fight against formalism included logic; and many branches of applied mathematics (biometrics, econometrics) were anathematised. Dialectics was corrupted into scholastic sophism. 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 Nevelésről – "On Rearing Scholars," English translation by Ninon Leader, Archive, 1. 9).[5]

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?." It is clear that, instead of preaching the dogmas of communist orthodoxy, Lakatos claims an active role for dialectics as an instrument of criticism, rather than as mere rhetoric for empty scholasticism. In his defence of "dialectical rationality" as opposed to "irrationalistic mystification" Lakatos was probably influenced by György Lukács.[6] 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.[7] 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 estabilished in the history of science").

Leaving his country for Cambridge, after the Uprising in late 1956, Lakatos would not entirely give up the outlook in which he was 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' 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.[8] 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. Lakatos had certainly studied Marxism at the time he took part in Szabó's seminar on Plato at Debrecen University (Szabó for example recalls that: "Lakatos was more interested in Marxism than in philosophy"), and he later attended Lukács' 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' 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.[9]

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.,[10] although ultimately the reliability of results is guaranteed by a rigorous (Euclidean) proof.[11] Lakatos progressively 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,[12] 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, 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.[13]

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:

Martin Carrier

Explaining Scientific Progress: Lakatos' Methodological Account of Kuhnian Patterns of Theory Change

Theory choice decisions were at the focus of the debate on theory change that dominated the philosophy of science in the 1960's and 1970's. Comparative evaluations of competing theoretical approaches were supposed to form the nucleus of scientific progress and, at the same time, the nucleus of scientific rationality. A theory choice decision singles out the methodologically most qualified alternative from among a set of rival theories. A sequence of such decisions is supposed to generate a series of theories with increasing methodological virtue; and a series of this kind constitutes scientific progress. Moreover, scientific rationality is tied to the nature and justification of the criteria brought to bear on theory choice decisions. Rational theory choices are made relying on objective and epistemically significant criteria. The intertwinement of rationality, theory change and theory choice is among the outstanding commitments underlying the entire debate on "theory dynamics"; in particular, it is constitutive of the methodological approaches of Larry Laudan and, above all, Imre Lakatos. The backdrop, against which this debate unfolded, is provided by the methodological challenge involved in Thomas Kuhn's philosophy of science. My objective is to elucidate more clearly Lakatos' attempt to neutralize methodological threats he assumed to be inherent in some of Kuhn's claims on theory change.

I begin by giving a brief sketch of Kuhn's model of scientific change and continue by highlighting the limitations it entails for any account of theory choice decisions appealing exclusively to objective and epistemically significant methodological criteria. Subsequently, I outline Lakatos' methodology and elaborate the criteria of theory choice involved in it. Finally, I develop the implications of Lakatos' model for scientific change and address, in particular, its bearing on those features of scientific change, which Kuhn thought defied the grip of systematic methodology. It is well-known that Lakatos attempted to provide a sort of rational reconstruction of some of Kuhn's allegedly descriptive generalizations about scientific change. My aim is to spell out the nature and import of this endeavor.

G. Kampis, L. Kvasz and M. Stöltzner (eds.),
Appraising Lakatos: Mathematics, Methodology and the Man, 53–71.

1. SOME BASICS OF KUHN'S "PARADIGM THEORY"

Kuhn introduced a distinction between two levels of scientific theorizing that was retained in all later conceptions; the distinction, namely, between an overarching theoretical framework, on the one hand, and its more specific elaboration, on the other. This framework he called "paradigm"; it was supposed to embrace theoretical principles, methodological or metaphysical commitments, and a collection of exemplary solutions to problems (whence derives the appellation "paradigm"). For example, the paradigm of 19th-century wave optics proceeded from the assumption that light is to be conceived as a state of elastic oscillation of a pervasive medium. Specific versions of the paradigm consisted in more elaborate explanations of optical phenomena such as refraction, diffraction or interference. A scientific discipline that is dominated by one particular paradigm has entered the stage of "normal science." The shared commitment to an overarching framework relieves the scientists from the need to defend their basic orientation and thus allows them to focus on more productive, technical work. In normal science, a paradigm rules monopolistically and unquestioned. Its principles are not liable to empirical testing. If an application of a paradigm fails unexpectedly, i.e. if an anomaly emerges, the blame is not attributed to the paradigmatic principles themselves. Rather, additional unrecognized influences or lack of ingenuity on the part of the scientists are held responsible. That is, either the situation is considered more complex than anticipated, or the scientists' creativity and technical skill are found to be wanting. The paradigm is maintained, in spite of empirical counterinstances. In sum, Kuhnian normal science is characterized by *paradigm monopoly* and *paradigm immunity* (Kuhn, 1970a, pp. 77–80; Kuhn, 1970b, p. 6).

It is obvious that the central traits of Kuhn's normal science stand in marked contrast to Popper's methodological advice to heed counterinstances. Scientists informed by Popper's falsificationism must not ignore empirical problems. Rather, they are called upon to either modify the theory in a methodologically acceptable fashion or to drop it entirely. A theory is improved in an acceptable way if the anomaly is resolved and at the same time the theory's empirical content (i.e. the number of possible observations conflicting with the theory) is expanded (Popper, 1935, §§ 20, 31). Nothing of the kind is required from Kuhnian normal scientists. They are licensed to shelve unsolved problems and go ahead undauntedly. The difference between Popper and Kuhn cannot be traced back to the ubiquitous contrast between lofty normative principles and sloppy practice. Kuhn, namely, gives epistemological reasons for the nonchalant attitude toward anomalies he assumes to be characteristic of normal science. The chief reason is based on the historical observation that no theory ever gets rid of anomalies. This Kuhnian tenet of the "omnipresence of anomalies" rules out assessing each of them as a serious threat to the theory. Taking – in the Popperian spirit – each anomaly to be a potential refutation amounts to closing down the business of scientific theorizing altogether (Kuhn, 1970a, pp. 79–82). By contrast, the immunity conferred to paradigms in normal science provides a basis for the tenacious pursuit of theories, which is in turn a necessary precondition for overcoming recalcitrant difficulties.

However, when anomalies pile up and the paradigm proves incapable of guiding a successful tradition of normal science, commitment to the fundamental principles is increasingly weakened and finally lost. In the course of such a "crisis," alternative theoretical options are considered and pursued. The emergence of a crisis follows from the principles of Kuhn's account. As a result of the sophisticated and highly specialized work done in normal science, anomalies are bound to turn up (barring the extremely improbable eventuality that a theory gets everything right). As unsolved problems pile up, each tradition of normal science sooner or later slides into crisis. Such crises frequently result in a "paradigm shift" that is characteristic of a "scientific revolution." It is one of Kuhn's central historical claims that a theory is never given up, unless it can be replaced by an alternative approach. Abandoning a paradigm is tantamount to adopting a new one. In contrast to the smooth development of normal science, Kuhnian cataclysms amount to a wholesale substitution of the former conceptual framework. This means, in particular, that revolutions are non-accumulative, in that they involve taking back problem solutions that were formerly accepted as correct. What counted as trustworthy scientific knowledge before, is at least drastically reinterpreted and frequently rejected as misleading or false.

The non-accumulative character of scientific revolutions becomes manifest in four features, namely, in changes: of the relevant concepts, of the problem-situations, of the criteria for evaluating theoretical achievements and, finally, in the occurrence of so-called "Kuhn-losses." The assumed conceptual change constitutes the notorious doctrine of meaning variance, which grows out of the assumption that meaning is determined by the pertinent theoretical context. Drastic alterations of this context lead to significant changes in the meaning of the concepts involved, which in turn may vitiate the translatability of concepts from different theories. This result constitutes the "incommensurability thesis," which denies that the substantive claims of disparate theories can be translated into one another (Kuhn, 1983). I won't go into this matter here. The reason is, first, that addressing this contentious issue deserves a full-scale treatise in itself, and that, second, it is of no relevance for the methodological problems I wish to discuss. The latter problems arise irrespective of any potential further aggravation generated by the breakdown of translation. Thus, I proceed from the assumption – as Lakatos does – that the substantive content and the empirical consequences of rival theoretical approaches can be compared.[1]

The second major shift occurring during a revolution concerns the change of problems. This is unsurprising at first sight. After all, it conforms well to the traditional picture of scientific progress that old problems are solved and new problems crop up. Kuhn does not deny that problem changes of this kind appear in the course of a revolution; on the contrary, the solution of long-standing anomalies within the new framework constitutes one of the chief reasons for the shift of allegiance. However, as Kuhn stresses, an additional pattern of problem change turns up which amounts to "dissolving" a problem rather than solving it. That is, the legitimacy of the problem is rejected by the alternative approach (Kuhn, 1970a, p. 103). Around 1770, for instance, one of the major challenges in chemistry was to explain the role of phlogiston in the release of hydrogen from the solution of metals in acids. The claim of the rival oxygen theory was that phlogiston doesn't exist at all, and that it consequently plays no role whatsoever in these processes. Accordingly, the question wasn't answered; it was rejected as misguided, instead.

The third important alteration refers to the criteria invoked for judging problem solutions. Such criteria are frequently tied up with – and specific to – a given paradigm, and thus change upon paradigm substitution. One of Kuhn's examples is again taken from the Chemical Revolution. Within the phlogistic framework, it was considered the chief task of chemical theories to account for the properties of chemical substances (such as hardness, combustibility, volatility and the like), along with their changes during chemical reactions. Consequently, chemical explanations are to be judged according to their capacity to afford such an account. In the course of the switch to the oxygen theory, these problems were shifted into the background, whereas the challenge of accommodating reaction weights was moved to center stage. Chemical theories are to be assessed according to their ability to meet this challenge. As a result of the paradigm shift, the standards for judging the adequacy of theoretical achievements are altered as well (Kuhn, 1970a, p. 107; Kuhn, 1977, pp. 335–336).

Fourth, scientific revolutions frequently go along with what is now called Kuhn-losses. A new paradigm may be accepted in spite of the fact that some of the former explanatory achievements are thereby lost. More specifically, some of the phenomena accounted for previously are re-transformed into open problems. Already explained data once again become anomalous. To be sure, Kuhn-losses are only tolerated as long as they do not exceed a low-level threshold. But the salient point is that they do occur, and that their mere existence vitiates any claim to the effect that the new paradigm reproduces all the explanatory achievements of the old one. One of Kuhn's favorite examples of a Kuhn loss again refers to the Chemical Revolution. In the phlogistic framework, a metal was regarded as a compound of a specific component (the "calx") and phlogiston. Since phlogiston was assumed to be present in all metals, the theory could explain why they resembled one another to a much greater extent than the corresponding calces (the oxides, in modern terminology). The oxygen theory, by contrast, considered metals to be elementary, and thus lacked any resources to account for their similarity. The adoption of phlogiston theory thus reopened an empirical problem that had been considered settled before (Kuhn, 1977, p. 323; Kuhn, 1970a, pp. 132, 157, 170; Kuhn, 1970b, p. 20).

On the whole, then, and due to these four features, revolutions are characterized by a fundamental theory change, which admits of no reconstruction to the effect that the earlier theory is approximately retained by the later. The contrast between theories separated by a revolution are far-reaching and unbridgeable (Kuhn, 1970a, pp. 5–6, 97–110).

2. KUHN'S ACCOUNT OF THEORY CHOICE DECISIONS

Within the framework of the paradigm theory, theory choice decisions are only made in the course of revolutions. Naturally enough, Kuhn's account of such decisions focuses on such cataclysmic periods. The central claim is that the best choice is not fixed by appeal to the available facts and to standards of evaluation traditionally deemed "rational." Rather, there is room left for subjective factors, and their supplementary influence is not detrimental to scientific progress, but constitutes a methodological virtue.

The first contention is that the evaluation of theories cannot be made by relying solely on the data. This claim follows directly from the basics of Kuhn's paradigm theory. On the one hand, the old paradigm is afflicted with a particularly large number of anomalies; otherwise a crisis wouldn't have occurred in the first place. On the other hand, the new paradigm has, due to its nascent state, not yet reached a level of elaboration and articulation comparable with the former monopolist. A freshly invented approach inevitably suffers from gaps and lacunae, most of which are unlikely to appear in the older competitor. The conclusion is that both rival approaches are anomaly-ridden, so that the evidential situation fails to give unique preference to one of them. Empirical adequacy cannot be the sole criterion for theory choice.

Second, this shaky empirical ground necessitates the invocation of additional, non-empirical standards. However, the catch is that these standards depend on – and vary with – the paradigm candidates at issue. As mentioned above, the contenders typically provide different standards for judging the appropriateness of problem solutions. As a result of these disparate criteria, each competitor appreciates its own assets and its rival's liabilities, drawing on its own specific measures of adequacy. Naturally enough, the adherents of the contrasting paradigms will fail to convince one another (Kuhn, 1970a, pp. 109–110).

Third, not all criteria of appraisal are tied to one of the rival candidates. There are shared methodological values, including explanatory power, precision, consistency or simplicity. The problem is, though, that these standards are imprecise, and can be made precise in disparate ways. If the simplicity of a given theory is to be assessed, different results are likely to turn up. Moreover, the application of more than one of these standards to a specific case may easily engender conflicts among them. One of the candidates may have a wider scope, while the other may furnish more precise

Gábor Forrai

Lakatos, Reason and History[1]

1. The Rationalist Program

Lakatos' philosophy of science is puzzling and can be interpreted in widely varying ways. According to the most widespread interpretation, his aim was to reconcile a basically Popperian outlook with the historical facts discovered by Kuhn and Feyerabend.[2] The purpose of this paper is to use his work to critically assess the project of which, according to this interpretation, he was an outstanding representative. This project conceives the task of philosophy of science *as showing the value of science by developing a normative theory of science, a methodology.* Its chief representatives were the logical positivists and Popper. Currently, Larry Laudan is the leading representative of this approach. The turn of the century conventionalists Poincaré and Duhem do not belong here: they were interested in the epistemological status of scientific theories, rather than in laying down universal norms for scientific practice.[3] It is for the same reason that later-day scientific realists like Richard Boyd or William Newton-Smith do not fit in here either. Their project is very much like that of the conventionalists, even though they reach practically the opposite conclusions. Kuhn and the other historically-minded authors also stand apart, for their approach is emphatically descriptive, not normative: they wish to study what happens in science rather than what should happen.

This project is often called "rationalism" both by its advocates and its critics. Lakatos uses a different term, "*demarcationism*." He describes it as one of the three possible answers to what he considers the central problem of philosophy of science: that of being the normative appraisal of theories. He summarizes it in this way:

> In the demarcationist tradition, philosophy of science is a scientific standards watchdog. Demarcationists reconstruct *universal* criteria which explain the appraisals which great scientists have made of *particular* theories or research programmes. But medieval 'science,' contemporary elementary particle physics, and environmentalist theories of intelligence might turn out not to meet these criteria. In such cases philosophy of science attempts to overrule the apologetic efforts of degenerating programmes.

G. Kampis, L. Kvasz and M. Stöltzner (eds.),
Appraising Lakatos: Mathematics, Methodology and the Man, 73–83.

Demarcationists differ over precisely what the universal criteria of scientific progress are, but they share several important characteristics. First, they all believe in the third world of Frege's and Popper's three worlds. The 'first world' is the physical world; the 'second world' is the world of consciousness, of mental states and, in particular, beliefs; the 'third world' is the Platonic world of objective spirit, the world of ideas. Demarcationists appraise the *products* of knowledge: propositions, theories, problems, research programmes, all of which live and grow in the 'third world' (whereas the producers of knowledge live in the first and second worlds). In line with this, demarcationists also share a critical respect for the articulated. They readily agree that articulated knowledge is only the tip of the iceberg: but it is exactly this small tip of the human enterprise in which rationality resides. Finally, demarcationists share a democratic respect for the layman. They lay down *statute law* of rational appraisal which can direct a lay jury in passing judgment. Of course, no statute law is either infallible or unequivocally interpretable. Both a particular ruling and the law itself can be contested. But a statute book – written by the 'demarcationist' philosopher of science – is there to guide the outsider's judgment. (1976, 226–227)

The other two answers are scepticism and elitism. *Scepticism*, as Lakatos understands it, denies that there is a distinction between good and bad theories. Every theory is as good as any other. Given his description (1976, 225; 1978c, 107–108), it would be better call this view relativism, but he likes to project contemporary positions into the past, and the label "scepticism" serves this purpose better. He does not seem to be bothered very much by this approach. He always describes it rather briefly and never takes the trouble to argue against it. This is somewhat surprising, since the only representative of this approach he mentions is Feyerabend, with whom he had been arguing for over ten years. Later on, I shall suggest that one reason for his not attacking it may be that he has no resources to deploy. This, in turn, may explain the label "scepticism": many people think that, when push comes to shove, one just cannot argue against scepticism.

His real enemy then, the enemy he does take on, is *elitism*. This is the view that even though there is a distinction between good and bad, the criteria cannot be articulated in terms of explicit rules. Some people, the scientific elite, just see what is good due to their exceptional talents or their long experience. So quality control is possible, but it is not amenable to public justification. If we want to find out the value of a theory, we cannot apply rules but have to turn to the scientific elite. Of course, we have to know who to turn to. So the elitist lays down sociological and psychological criteria for the identification of the elite.

The thinkers Lakatos identifies as elitists are those who are not relativists but do not traffic in universal rules: Kuhn, Polányi and Toulmin. They would probably object to being classified in this way, and they would have a point: their professed aim is not to solve the problem of normative appraisal but to analyse how science works in descriptive terms. However, Lakatos' regarding of them as elitists is not groundless. He could argue that, by rejecting the enterprise of rule-based normative appraisal, they imply that appraisal is to be left to the scientists, a group of people with certain privileges, i.e. an elite.

So their rejection of the problem amounts to an elitist answer. He could add that many things these thinkers say would very much fit in with the elitist position.

But why does Lakatos think that this approach is wrong? His first objection (1978c, 114) is that normative evaluation of products must precede the sociological or psychological identification of the producers. The whole point of identification is to find out whose opinions can be trusted in the future. Using purely descriptive means, it is possible to identify many groups – high energy physicists, astrologists, stamp collectors, etc. – but this alone will not answer the question of whom to trust. It seems that criteria are necessary in order to decide which opinions are trustworthy, after which one may then employ sociological criteria to identify the group whose members are likely to have trustworthy opinions. However, the criteria for selecting the trustworthy opinions are just the sort of explicit universal rules the demarcationists are looking for. So elitism either does not get us anywhere, or it is incoherent, for it builds on the results of the demarcationist program, which it rejects.[4] Alas, this objection is not particularly convincing. An elitist (and not just an elitist, for that matter!) may reply that one does not need explicit rules to start trusting some people. It is without rules that we trust parents and teachers. And these initial authorities may direct us to other authorities, say, scientists, and then we may start applying sociological techniques to identify that group.

Lakatos' second objection (1978c, 115) is that the elitist position implies that every change, in the elite's view, constitutes progress. If we ask the elite whether their current view is better than the earlier one, they will certainly answer "yes." So every change is improvement, which is absurd. However, this objection works only if the elitist is not fallibilist. The fallibilist elitist may say this: "Look, this is what the elite believes. Your best shot is to believe the same. It may happen later that they realize that they were wrong, and that their change for their current view was a step backwards. So from the vantage point of their future opinion, the change for the current view was not progress." So the elitist may re-evaluate his authority-based judgment when the authority's view changes, but this falls short of acknowledging that every change is progress.

The third objection is that elitism does not help if the elite is divided. That is true. Under these circumstances that elitist will not know what opinion to adopt. But is this a fatal shortcoming? Why should the elitist be able to provide an assessment at any time? Why cannot he suspend his judgment until the elite sorts out the questions?

I did not raise these difficulties to defend elitism, but to illuminate Lakatos' position. The fact that he does not consider these – not particularly ingenious – counterarguments suggest that he believes they are beside the point. And the reason why they are beside the point is that he believes that normative appraisal should be *objective,* in the sense that it should not rely on anything particular, local or personal. It should be made from the standpoint of someone with no specific features or determinations at all (with the exception of the minimal rationality all sane adults possess). If one insists on this sort of objectivity, then the reply to his first objection is irrelevant. It is just a biographical matter what authorities one first comes to trust, and to what further authorities one is directed by them. These personal factors cannot lead to objective value judgments. So the reply simply does not speak to the point. The reply to Lakatos' second objection can be dismissed on similar grounds. Since the elitist's judgments vary together with the views of the elite, such judgments cannot be objective, since objectivity requires independence of accidental matters. If we have two theories and a body of relevant evidence, and these are kept constant, one should always arrive at the same judgment concerning their comparative value, and the judgment should not depend on local conditions such as the elite's opinion. The judgment can be overruled only if the theory or the evidence changes. A change in the elite's opinion is not an objective factor. Roughly the same reasoning can be applied against the reply to Lakatos' third objection. It might indeed happen that we cannot give higher marks to one competitor than to the other, i.e. we have to suspend the judgment. But the suspension of judgment cannot be based on the local condition of the elite's being divided.

This conception of objectivity makes it easy to understand some of the things Lakatos says about rationalism, i.e. demarcationism. The normative rules should be *universal*, since local rules, by definition, are not objective. They must be *explicit* rather than tacit, for the uninitiated cannot apply but explicit rules. Whether someone has been trained to pass judgment in a certain way or not is again a personal matter, so an objective judgment should not depend on it.[5] Finally, the objects of objective judgment should belong to the *third world*, since second world objects, like beliefs, are not available for everyone. Newton's thoughts were available only for a few people at best, but his theory is there for everyone.

2. Finding The Rules

Hopefully, this analysis of Lakatos' demarcationism results in a clearer picture of the aim of the rationalist project: to show the value of science in terms of rules that are objective in the above sense. But how should the rationalist go about finding such rules? The only possibility seems to be to take some theories whose scientific credentials are impeccable, and compare them with those which do not qualify as being scientific. Popper actually tells such a story about how he arrived at his rules (1963, 33–37), and Lakatos repeats it (1971a, 123–124). Around 1919, when Popper first started to think about these issues, there were four widely discussed theories: Einstein's theory of relativity, Marx's theory of history, Freud's psychoanalysis and Adler's individual psychology. He was impressed by the first, but has come to question the scientific status of the other three. He was thinking in this way:

Einstein alone took a serious risk – Eddington's 1919 measurements could have refuted his view. The advocates of the other three theories, however, had claimed to have won without taking risks. Everywhere they looked, they found evidence. From here, he arrived easily at the criterion of falsifiability and the ban on ad hoc adjustments.

Now this way of finding objective rules may raise two sorts of concern. First, suppose Popper had adored Marx, Freud and Adler, but despised Einstein. Would not have he come up with a completely different set of rules then? Surely, the rules must be based on something more solid than personal taste. I will come back to this point, but only later in the paper. Let us now accept that his choice of good and bad examples was legitimate. The second worry is that he had helped himself to some particular examples, which became available only at a particular point in history. Suppose Popper had been a contemporary of Plato. Could he have *then* come up with the same criteria? Probably not. One's conception of what science should be like is certainly heavily dependent on what science is at the moment. Since her place in history is a specific fact about the prospective rationalist, it cannot be relied on in the formulation of the rules for science, since the rules must be objective. Fortunately, this worry can be put to rest by invoking the distinction between *discovery* and *justification*. What warrants the objectivity of the rules is that they can be justified in an objective manner. Their discovery can – and actually must – depend on particular facts, but that does not matter.

Thus, the objectivity of the methodological rules must be grounded in their justification. So how are methodological rules to be justified? The obvious strategy for this is the following. Find something suitable to serve as the ultimate goal of science, something that does not stand in need of further justification, and show that the consistent application of the rules leads towards the realization of this ultimate goal. The best (the only?) candidate for serving as the ultimate goal is truth. So you have to show that the theories your rules of appraisal favour are those which have a better chance to be true. If the logical positivists had been asked to defend their methodology, they would have said that the reason why scientists should prefer highly confirmed theories is that the better a theory is confirmed, the more likely it is to be true. However, justifications of this sort can be subjected to epistemological criticism. An example of this is Popper's attack on logical positivism. He pointed out first that the logical positivists' preference for highly confirmed theories has the disastrous effect of leading to theories poor in content. The richer a theory is, the less likely it is to be highly confirmed. In effect, the high confirmation rule favours truisms. Second, he repeated Hume's criticism of induction in order to sever the link between confirmation and truth. If induction is ruled out, there is no reason to believe that high confirmation is a mark of truth.

Popper's own rules suffered similar criticism at Lakatos' hands. He pointed out that Popper's requirement of high content and high falsifiability does not