# George Polya's reaction to Imre Lakatos' 'Proofs and Refutations'

Miklós Rédei
Department of Philosophy, Logic and Scientific Method,
London School of Economics and Political Science,
Houghton Street, London WC2A 2AE, UK, m.redei@lse.ac.uk.

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**Abstract**

Based on unpublished, archival material, some informal reactions by George Polya to Imre Lakatos' "Proofs and Refutations" are presented. The archival material is letters by Polya to Lakatos in the period between 1957 and 1965. The letters show that Polya admired Lakatos' work but he also voiced some criticism, especially when Lakatos deviates from heuristics.

## 1 The aim of this paper

The main purpose of this paper is to present, mainly on the basis of unpublished, archival material, some informal reactions by George Polya to Imre Lakatos' "Proofs and refutations" ($P\&R$ henceforth). By "archival material" is meant here letters written by Polya to Lakatos in the period between 1957 and 1965. These (typically hand-written) letters are held in the *Lakatos Papers* in the Lionel Robbins Library of the London School of Economics. Most of the letters were written in Hungarian (the native tongue of both Polya and Lakatos). English translations of some of these letters by *Ninon Leader* (born "Neményi Ninon") are also available in the *Lakatos Papers*. These English translations have been used in this paper – with occasional insignificant modifications which will not be indicated explicitly. Letters by Polya to Lakatos and from Lakatos to Polya can also be found in the Polya archive of Stanford University; these are from the period after 1971 and are not used in this paper.

The expression "Proofs and Refutations" needs some clarification when it comes to the issue of Polya's reaction to it. This clarification will be done in section 2, where some context of Polya's reaction will also be given. Section 3 contains excerpts from Polya's letters to Lakatos' about

Lakatos' plan to write about Euler's theorem in his PhD dissertation. Section 4 presents Polya's reaction to $P\&R$ as expressed in his letters, including his criticism of $P\&R$. In this section I also comment on Polya's criticism. Section 5 formulates some concluding thoughts.

## 2 The context of Polya's letters to Lakatos on "Proofs and Refutations"

Lakatos became familiar with Polya's heuristics already before emigrating from Hungary to England (for a review of the main events in Lakatos' life and the main themes in his philosophical work, see the Lakatos entry in the Stanford Encyclopedia of Philosophy [10] and the references there). While Lakatos worked in the Research Institute of Mathematics of the Hungarian Academy of Sciences from 1953, he translated Polya's book *How to Solve it: A new Aspect of Mathematical Method* [11] into Hungarian [12]. Polya thanks Lakatos for his effort to do so in a letter [13] written in 1957, the year in which the Hungarian translations of Polya's book was published. Polya and Lakatos remained in touch via correspondence even after the period of their exchange about the $P\&R$.

In the years 1956-1959 Lakatos was working on his PhD Thesis in King's College in Cambridge (under the supervision of R.B. Braithwaite). The PhD Thesis, with the title "Essays in the Logic of Mathematical Discovery" [1] was completed in 1961. A substantial part of the PhD Thesis was based on Lakatos' four part paper entitled *Proofs and Refutations* published in *The British Journal for the Philosophy of Science* (henceforth *BJPS*), received by the editors on October 3, 1960 [2, 3, 4, 5]. Lakatos planned to expand these works into a book and worked on this during 1961-1966; however, he never finished the manuscript. There are several reasons for this. One is that Lakatos' interests turned to philosophy of science while he was working on the $P\&R$:

> In 1966 Schilp asked me to contribute to the Popper volume. I took this as a great honour to be able to defend Karl Popper from his wicked enemies and sat down to read his works which for me then formed part of my unquestioned background knowledge. Thus I left the new version of my "Proofs and Refutations", extended to about 400 pages and nearly ready, and switched to philosophy of science for what I thought would be an excursion of a couple of months. As it turned out, very soon my unquestioned background knowledge became questioned background knowledge and I ended up, first by publishing two longish papers on the philosophy of science critical of Popper; a third one, almost book-long, is now in press; and I decided to tie up the lose ends and turn it into a book on the philosophy of science by the end of this year. [7]

But Lakatos intended to return to his work on $P\&R$ and publish it, as he writes to Polya in 1972:

> I plan to finish my book *On Scientific Research Programmes* [...] in 1973 and to finish *Proofs and Refutations and other Essays in the Philosophy of Mathematics* in 1974, I hope in time for your 85th birthday. [8]

But this never happened. The book [9] was finally published in 1976 only, after Lakatos' death in 1974: Lakatos' students, J. Worrall and E. Zahar edited the available manuscript for publication (see the Editors' Preface in [9] for more details about what the published work includes and how it is related to Lakatos' PhD dissertation and the papers [2, 3, 4, 5]).

Thus, when it comes to Polya's reaction to $P\&R$, one should distinguish the following three periods and the related works by Lakatos:

1. The period 1957-1960, when Lakatos planned his dissertation and worked on it and on the papers published in *BJPS*.
2. Lakatos' PhD dissertation [1].
3. After 1963, when the papers in *BJPS* had already been published.

The date and content of the letters by Polya to be cited in the next sections make it clear which period and which work it is that Polya was commenting on.

# 3 Polya on Lakatos' dissertation plan

Lakatos explicitly states in his dissertation that the idea of considering Euler's theorem and its history was suggested to him by Polya, and he also mentions Polya's heuristics as one of the three major intellectual sources of the ideas developed in his dissertation:

> The three major – apparently quite incompatible – "ideological" sources of the thesis are Polya's mathematical heuristic, Hegel's dialectic and Popper's critical philosophy. [1][p. 5]

One finds a similar acknowledgment of Polya's influence in the first of the four papers in which Lakatos first published the ideas of $P\&R$: "The paper should be seen against the background of Pólya's revival of mathematical heuristic." [2][p. 1]

Lakatos and Polya were corresponding already before Lakatos started working on his dissertation, and Lakatos discussed the plan of his dissertation with Polya very early on: In his letter of June 12, 1957, Polya writes:

> I am very interested in the outline of your thesis – yes, indeed, one could, or rather one ought to probe into all directions (I myself have attempted to probe into it to some extent) but it is difficult to arrive at some lucid, "clear-cut" result. I wish and hope that you shall arrive at some lovely result. [13]

There are no copies of letters by Lakatos to Polya from the year 1957 in the *Lakatos Papers*; so it is unclear what precisely the plan was that Lakatos had sent to Polya for comments. But there is evidence that investigating Euler's theorem was part of the plan because Polya writes:

> I am very glad that you are studying with interest those things which you feel belong to your topic. One must follow one's feelings (with a certain degree of criticism and scepticism). You will return to Euler's theorem "when the spirit moves you" and this is how it should be. [14]

# 4 Polya's reaction to Lakatos' "Proofs and Refutations"

In view of the pre-history of Lakatos' PhD dissertation sketched in the previous section, it is not surprising then that Polya was among those who first received a copy of Lakatos' PhD Thesis. Polya's first reaction was the following:

> I received the first instalment of your thesis yesterday and by today I have already finished reading it – I cannot give you a greater compliment. The second compliment: I shall try it on my naive and ignorant teachers and will let you know if anything interesting turns out of it. [17]

It is not known what the outcome of Polya's experiment turned out to be – he never reported on this to Lakatos...

But Polya certainly liked what he read. Apparently he advised Lakatos to send his thesis for publication in *The American Mathematical Monthly* (henceforth "*Monthly*"). There is no explicit evidence in the correspondence for Polya having given this advice; however, in his letter of October 8, 1960, Polya writes that if he receives from *Monthly* the manuscript for refereeing, he would "warmly recommend it"; voicing at the same time the worry that Lakatos' work might be "[...] too 'sophisticated' for the readers of *Monthly*." [17]. Polya also thought that it would be a pity to simplify the work in order to get it published in the *Monthly*. This is formulated in a letter dated December 23, 1960, which is a bit confusing, since by October 3, 1960, Lakatos' paper had been submitted to *BJPS*; so one would think that publishing it in the *Monthly* was already off the agenda by December 1960. The possible explanation of the fact that it was not is that Polya had not yet been told by Lakatos by December 1960 that the paper had been submitted to *BJPS*. At any rate, Polya writes:

1. It would be a pity to trivialize the witty phrases – even for the "*Monthly*": the planned revisions (terminology, more forceful – and more detailed ! – phrasing of the central questions) will make the finer hints more understandable in general.
2. I would leave the quotations in their original form in a book written for adults (perhaps providing (??) their translation next to them – this would be better but this is a question of room and money). For the "*Monthly*" they have to be translated into English: infants do not speak languages.

> [...] The most important would be to publish the dissertation in book form, as a whole – from the perspective of impact and (let's not forget: prestige). [15]

Polya's worries about the suitability of Lakatos' paper for the *Monthly* turned out to be justified. Lakatos did not publish in the *Monthly*, and Polya regretted his advice to try to publish it there:

> I sincerely regret that I advised you to send your paper to the *Monthly* – it is much too sophisticated for the average reader of the *Monthly* – the best jokes are beyond his comprehension, and it would be a pity to rewrite it, leaving out the neatest points. [16]

Polya discussed other possible venues for publishing $P\&R$, apparently in reply to Lakatos' questions:

> I'm replying without delay to your just arrived letter (12.12.62). Unfortunately, I do not know enough, I do not have enough data, and I'm worried that my reply will not be very useful.
>
> I do *not* know the Van Nostrand series in question. Occasionally I receive undergraduate texts from them, but, on average, I read 2 pages in 10 books. I am not saying anything new when I say that, on the basis of its title, your book does not fit into this series.
>
> I know Kelly (John. L, I suppose, University of California, Berkeley) and Halmos, but not well. This information about Halmos is perhaps relevant: He wrote (a quite positive, not bad) critical review of "Mathematics and Plausible Reasoning" – Bull. Am. Math. Soc. v. 61 (1955) p. 243-245, unfortunately I do not have it at home, I am unable to quote verbatim, which he starts with the statement that he would completely disregard everything and would not even mention anything in the book that concerns heuristics and philosophy.
>
> The other publishers mentioned are all having good reputation. The best is of course Cambridge University Press – and this is the only one I know personally, Hardy-Littlewood-P. "Inequalities" was published by them – I can only say positive things about them. But if they are slow, this is definitely not a good sign. [18]

More generally, it seems Lakatos had difficulty in publishing the material on which his PhD Thesis was based: He tried to get some parts of it published in the journal *Mind*, but G. Ryle, editor of *Mind* rejected it, partly on the basis that the paper had a very large number (63) and long footnotes [20]...

Not only did Polya strongly and consistently urge Lakatos to publish his PhD Thesis in a book form – he advised Lakatos against any substantial modification of his Thesis. Reacting to the published version of $P\&R$ in *BJPS* Polya writes:

> My main impression: *Do not tinker with* $P\&R$, *trying to improve it*; incorporate it into your book as it is, apart from

> possible minimal corrections concerning details. It is true that $P\&R$ is, in many respects, confusing: if the reader tries to attach the different personalities to the various letters of the Greek alphabet which stand for them; if he tries, further, to distinguish the numerous new terms from each other (on almost every page up jumps a new term) – then he would get dazed. At least this is the experience of the reader who is writing this letter. But the paper is interesting as it is, it is entertaining, funny, and – most importantly – it is 'anregend' [*exciting – German*] and 'stimulating': the elimination of each secondary disadvantage could be done only at the cost of losing a value of first order – this is my strong impression. [19] (emphasis in original)

Similar thoughts about no-revision are formulated already in an earlier letter by Polya:

> If you do rewrite it – it would be a pity to rewrite it, I think –, do not forget the simple minded and put somewhere a resume in more generally understandable, if less "nuancé", terms. [16]

Polya's advice for Lakatos to provide a summary of the main points of $P\&R$ in an accessible manner hints at the difficulty that it is not easy to distill the philosophical-conceptual message of $P\&R$. This is also shown by the fact that when Polya tried to come up with some constructive criticism, he was struggling somewhat:

> At last I have had a relatively peaceful week. [...] I have read the *"Proofs and Refutations"* ($P\&R$) twice from beginning to end. Part IV. was new to me. I read it slowly with all the concentration I am capable of. Then I wanted to write a long letter to you. I realized however, in a fairly short time, that only a considerably shorter letter would have any chance of being written if I wanted to finish it in a finite time. I had written three pages of it. I could not continue it; afterwards I could not continue it either; now I have torn those three pages into pieces and shall be writing a very short letter, and an untidy one as well – otherwise I shall never finish it. [19]

Given that Polya was an eminent mathematician intimately familiar with the topic of $P\&R$, it seems justified to interpret the above words by saying that the difficulty Polya had when he was attempting the "long letter" was not just that he was short of time. The difficulty was related to the content of $P\&R$. This work represented a new genre. It was historical – but it was not history of mathematics in the traditional sense; it also was philosophical – but it was not philosophy of mathematics either in the traditional sense.

It seems that Polya also thought that, in spite of its virtues, the format of the work entailed some compromises and thus that it might be good to re-write the main points in a more systematic, less "theatrical" manner – more in a traditional format of history of mathematics:

> One could think of repeating the most important points of $P\&R$ in a subsequent chapter with fewer jokes and with greater calm; yes indeed, to think, to think it *through*. [19] (emphasis in original)

The above words amount to an implicit criticism of $P\&R$. But Polya also formulated some criticism explicitly. In his letter of October 24, 1960, he lists a number of typos and problematic terminology in Lakatos' manuscript (which is likely to have been the one that was submitted to the *BJPS*); but, more importantly, Polya had a substantial critical comment as well:

> I like very much everything, with very few exceptions. [...] As to the concrete case at hand: convex polyhedra have a solid "empirical content" and of the many proofs for Euler's th.[eorem] I prefer those which do deal with convex polyhedra – they are the least "analytic" or "trivial" – and Heinz Hopf, who is a good enough topologist, shares my taste in this point – he wrote an article "Die Zusammenhänge zwischen Topologie und Metrik im Rahmen der elementaren Geometrie" (Mathematisch-Physikalische Sommerberichte, Bd. III., p. 16-29, 1953) in which he restricts the consideration to convex solids – and I once discussed with him this point *in extenso*. In fact, this is a minor point *here* – but heuristically: to consider first convex polyhedra and dismiss other cases as "cura posterior" is, I think, *heuristically sound*. Similar: in considering an existence theorem for a boundary value problem it is heuristically sound to prove it first under narrow conditions (more continuity of boundary values, simple, perhaps convex domains, ...). Why? If you have a clear proof under narrow conditions, there is quite reasonable hope to extend it later: either by reducing the wider conditions to the narrower by a transformation, or by the study of the "narrow" proof: isolate ideas useful for the "wider" proof, or ...) [16]

This remark by Polya amounts to the criticism and suggestion that the simple and sound heuristic rule of starting from the typical and simple and ending with the exceptional and artificial could and should have been followed by Lakatos in presenting the material in $P\&R$. So, more or less tacitly, Lakatos is accused by Polya of breaking here with a main attitude represented by heuristics in mathematics.

But Polya's criticism cuts even deeper because Lakatos' breaking with a traditional presentation of mathematics in $P\&R$ is deliberate and essential: Lakatos could not have accepted Polya's advice. For if he had started with a (modern) definition of convex polyhedra he would have had to end the story right there because for convex polyhedra Euler's theorem does hold. Therefore no local or global counterexamples could have been given by the students discussing Euler's Theorem and the problem of the need to design different definitions motivated by counterexamples of (non-convex) polyhedra could not have arisen; consequently the discussion displaying the fluidity of mathematical concepts could not have started.

So, while Polya's critical remark is entirely rational and understandable from the perspective of a practicing, systematically thinking mathematician aiming at providing sharp definitions and proofs of increasingly general theorems, this viewpoint was against the whole idea of $P\&R$: it was in contradiction with the conceptual dramaturgy of $P\&R$. Lakatos could not have followed Polya's advice unless he was ready to sacrifice one of the leading ideas of $P\&R$.

But it seems Lakatos himself was aware that the dialog form of $P\&R$ was suboptimal when it came to portraying the role of rigor mathematics:

> My reluctance to publish it till now in English in book form was because I somehow thought that the dialogue did not bring out sufficiently the value of rigor. I hope that the new version will be an improvement in this line. [6]

But changing the dialog form and abandoning the idea of not starting with a precise definition of polyhedra would have meant a too radical deviation from the original work – Lakatos could not have done this without sacrificing the whole $P\&R$.

Polya also commented on the relation of Lakatos' work and his heuristics:

> I can see fairly clearly the relation of $P\&R$ to my own work. The basic difference is: I myself would hardly be able to say anything on "epistemology" that would deserve the attention of the public. Had been able to say anything about it, even then I would have refrained from it: it is difficult enough to have the public accept heuristics, and I would not have wanted to make this even more difficult by combining it with other controversial matters. The main point of $P\&R$ is, in my view at least, to call attention to the possible connection between heuristics and epistemology. It makes a number of points about pure heuristics as well, which I have not seen so clearly, and at any rate, have not said. [19]

# 5 Concluding comments

Given the significance of Polya's influence on Lakatos' $P\&R$, which was acknowledged by Lakatos himself, Polya's reaction to $P\&R$ is relevant historically. As we have seen, this reaction was overwhelmingly positive – with some important criticism. This criticism was typical in the sense that it arose from the practicing mathematician's attitude towards mathematics and its history. The typicality is further evidenced by another well-known mathematician's reaction: van der Waerden's. When Lakatos asked Polya for possible suggestions of mathematicians who would be interested in $P\&R$, Polya replied:

> Apart from myself, I know (or, rather: used to know) of only one mathematician who is seriously interested in heuristics: Fr. Krauss, a professor in Aachen before the war (at the end of the thirties), at the Technische Hochschule. But where he might

> be today, or whether he is still alive, I have no idea. (But I will find out and if he is still alive, I will let you know.) [...] van der Waerden in (Zürich) is also interested to some extent – you could learn a great deal from him, but perhaps not about heuristics. [13]

Lakatos followed Polya's advice and had sent his PhD dissertation to van der Waerden. van der Waerden replied [21]. In his reply van der Waerden agrees with a lot of Lakatos' claims but he also formulates a criticism that is very similar to Polya's described in the previous section: van der Waerden finds it absurd that Lakatos leaves the concept of polyhedron undefined, and he suggests a rewriting of $P\&R$ in the spirit and format of traditional history of science.

Lakatos also contacted other prominent mathematicians and philosophers of mathematics of his time (e.g. Hao Wang, Brouwer, Quine, Dummett), sending them his PhD dissertation or the *BJPS* paper. He had received replies in form of letters, these can be found in the *Lakatos Papers*. Discussing those reactions would be very interesting but this is beyond the scope of the present paper.

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