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ARTICLE

What did Popper learn from Lakatos?

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ABSTRACT
The canonical version of the history of twentieth century philosophy of science tells us that Lakatos was Popper's disciple, but it is rarely mentioned that Popper would have learned anything from Lakatos. The aim of this paper is to examine Lakatos' influence on Popper's philosophical system and to argue that Lakatos did have an important, yet somewhat unexpected, impact on Popper's thinking: he influenced Popper's evolutionary model for 'progress' in science. And Lakatos' influence sheds new light on why and how Popper continually revised one of the central claims of his philosophy of science: the evolutionary account of scientific theory change.

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I.

The title of this paper may seem surprising. The canonical version of the history of twentieth century philosophy of science tells us that Lakatos was deeply influenced by Popper, but it is rarely mentioned that Lakatos would have influenced Popper in any way. The consensus is that while Lakatos was Popper's disciple, Popper considered Lakatos' (early) work at most an application of his (in the domain of philosophy of mathematics). The aim of this paper is to argue that there is evidence that Lakatos had a profound influence on Popper's later thinking.

Popper wrote a letter to Isaiah Berlin about Lakatos in 1964, in which he writes: 'I can say what I think about him in five words: He has revolutionized my thinking'.[1] But what could he possibly mean? He did not start talking about progressive and degenerating scientific research programmes and protective

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[1]Popper to Isaiah Berlin on 16 February 1964, Lakatos Archive 13/736/65.

belts ever after. If Lakatos did have an influence on Popper's thinking, it must have been a less obvious one.[2]

It seems that Lakatos himself also thought that Popper learnt a lot from him. He wrote repeatedly (at least a dozen times) on the margins of various manuscripts by Popper that, 'you are stealing from me'.[3] It is also likely that Lakatos' perception that Popper did not acknowledge his influence was the main cause of the serious fallout between the two philosophers in the early seventies.[4]

The aim of this paper is to examine in what way Lakatos influenced Popper's philosophical system and to argue that he did so in an important, yet somewhat unexpected, way: he influenced Popper's evolutionary model for 'progress' in science.

II.

My proposal is that Lakatos influenced the way Popper thought about the analogy between natural selection and selection in the domain of scientific theories: in short, he influenced the way Popper's evolutionary epistemology was formulated.

This proposal may seem extremely surprising for two reasons. First, Lakatos was not too enthusiastic about the whole project of giving a selectionist explanation for progress in science. Second, Popper started talking about the analogy between natural selection and the selection among scientific theories in the thirties, decades before he met Lakatos. I will consider these two reasons to doubt Lakatos' influence on Popper in turn.

II. a.

Lakatos never seemed to understand why Popper was so interested in evolutionary explanations. But as he never directly criticized Popper's evolutionary arguments, it is not easy to see why he had doubts about this project.

However, Lakatos did write extensively on Stephen Toulmin's evolutionary model for scientific progress. As Toulmin's evolutionary arguments are, in important respects, similar to Popper's, we may be able to understand what it was that Lakatos disliked about Popper's evolutionary explanations by focusing on his criticism of Toulmin's writings.

[2]Note that Popper often said that Lakatos has revolutionized philosophy of mathematics (Lakatos confronts Popper with this in a letter in 1969, Lakatos archive, file number 13/736). But what he wrote to Berlin is something much stronger: that Lakatos revolutionized *his* thinking (not a subdiscipline).

[3]Lakatos archive, file number 10/2, Lakatos archive, file number 15/90.

[4]This fallout is well-documented (Lakatos archive, file number 13/736, Popper archive file number 318.4-10). Given the intellectual and personal closeness of the two philosophers, this turn of events is even more surprising – and the nominal reasons given (Lakatos used Popper's typist without Popper's authorization, Lakatos failed to give back the proofs of one of Popper's articles) clearly do not explain it.

Toulmin insisted that evolution should be more than a mere metaphor when we describe the progress of science (Toulmin, 'From Logical Systems', 560–4, see also Toulmin, *Human Understanding*). It is not enough to compare the trial and error method of science to the trial and error method of natural selection. The evolutionary model is indeed explanatory (Toulmin, 'The Evolutionary Development', 470): selection among scientific theories explains some of the features of these theories, most importantly, their survival.

Lakatos has a number of problems with Toulmin's project.[5] Perhaps the most important one is exactly about the explanatory power of evolutionary considerations (Lakatos: Toulmin's Wittgensteinian epicycles, ms, 12).[6] For Toulmin, the survival of a scientific theory is explained by its fitness. Lakatos strongly opposes this idea as, according to him, whether a scientific research programme survives depends on whether it is progressive or degenerative. And whether a research programme is progressive or degenerative is not a matter of some kind of momentary fit between the research programme and the data (which would supposedly be the equivalent of biological fitness in this context).[7]

Whether a research programme is progressive or degenerative can be determined if we look at how it has changed: if its predictive/explanatory power has increased in response to changes, it is progressive. If it has not, it is degenerative (Lakatos, 'Falsification and the Methodology', 'Science and Pseudoscience').[8] In contrast, the 'fitness' of a scientific theory, whatever it is exactly, is unlikely to be a function of the ways it has changed in response to objections.

Of course, what is in the background of this criticism is that while Toulmin (and Popper) tries to explain the survival of scientific *theory*, Lakatos' concern is with the explanation of the survival of a scientific *research programme*. As scientific research programmes do not get eliminated as a result of objections or conflicting data, it is not clear how the evolutionary analogy could be used in their case.

[5]The most detailed account of Lakatos' problems with Toulmin's evolutionary explanation is in Lakatos: Toulmin's Wittgensteinian epicycles (manuscript in the Lakatos archive, file number 8/4). For a shorter summary, see Lakatos, 'Understanding Toulmin', 137–8 (see also Worrall and Currie, *Mathematics, Science, and Epistemology*, 236–7; Nanay, 'Rational Reconstruction').

[6]Lakatos' other main problem is that this selectionist explanation bypasses scientists and philosophers, very much like the cunning of Hegelian reason. See also his letter to Jon Cohen, who reviewed Toulmin's book in the *British Journal for the Philosophy of Science* (which Lakatos edited) in 1972. Lakatos here explicitly agrees with Cohen's criticism of Toulmin's Darwinism. Lakatos to Jon Cohen, 22 October 1972, Lakatos archive, file number 13/166.

[7]The concept of fitness taken for granted in the late sixties and early seventies is somewhat different from what philosophers of biology mean by this concept now. The recently influential 'propensity interpretation of fitness', according to which fitness depends on the future course of events, was only introduced in the late seventies (Mills and Beatty, 'The Propensity Interpretation').

[8]Lakatos repeatedly insists, especially towards the end of his life, that this is the way progressive and degenerative research programmes can be distinguished, see, for example, his letter to Alfred Schramm, 12 October 1973, Lakatos archive, file number 13/829. See also Lakatos, 'Science and Pseudoscience', 101.

Although Lakatos' objections were aimed at Toulmin, they apply equally well in the case of Popper's evolutionary explanatory scheme for progress in science (especially as Toulmin is explicit that many aspects of his theory are based on Popper). Popper has also attributed explanatory power to the 'fitness' of a scientific hypothesis, already in the *Logic of Scientific Discovery*:

> We should try to assess how far [a hypothesis] has been able to prove its fitness to survive by standing up to tests.
>
> (Popper, *The Logic of Scientific Discovery*, 248)

And more explicitly:

> How and why do we accept one theory in preference to others? [...] We choose the theory which best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive. This will be the one which not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way.
>
> (Popper, *The Logic of Scientific Discovery*, 91)

Thus, when criticizing Toulmin, Lakatos also implicitly criticized Popper. As a result, it may seem odd that Lakatos, who, for the reasons mentioned here mistrusted the evolutionary explanation for progress in science, could influence Popper's evolutionary epistemology. But I will argue in Section III that this is exactly what happened.

II. b.

The second *prima facie* reason to doubt that Lakatos had a profound influence on Popper's thinking about the selectionist model of science is that Popper gave his first exposition of these ideas decades before he met Lakatos. In a letter to Donald Campbell, Popper says that the idea goes back at least to the early thirties.[9] And he had a fairly detailed account of it in his 'What is Dialectic?', a talk given in 1937 and published in 1940:[10]

> If we want to explain why human thought tends to try out every conceivable solution for any problem with which it is faced, then we can appeal to a highly general sort of regularity. The method by which a solution is approached is usually the same; it is the method of trial and error. Thus, fundamentally, is also the method used by living organisms in the process of adaptation.
>
> (Popper, *Conjectures and Refutations*, 312)

Note, however, that Popper's view on the status of the analogy between natural selection and the trial and error method in science changed over

[9]Letters from Popper to Donald Campbell, February 3 and April 6 1964, Popper archive, file number 282/12.

[10]The paper was published in *Mind* in 1940 and reprinted as chapter fifteen of *Conjectures and Refutations*. The most explicit statement of the evolutionary analogy is on pp. 312–13 of *Conjectures and Refutations*.

the years. Popper rarely acknowledges this, but others did.[11] An important example is Ernst Mayr, one of the most important evolutionary biologists in the twentieth century. In 1979, he wrote to Popper commenting on one of his papers ('Natural Selection and the Emergence of the Mind'), emphasizing that it is a much better treatment of natural selection than Popper's earlier writings. As he says:

> To be very frank, I was not too happy with your treatment of natural selection in the essay you wrote in Objective Knowledge.[12]

Mayr emphasizes (twice) how 'delighted' he is that Popper changed his views about natural selection. But what is this change in Popper's treatment of natural selection and when did it happen? The early essay Mayr was referring to is 'Evolution and the Tree of Knowledge', which was published as Chapter Seven in *Objective Knowledge*. It was originally delivered as the Herbert Spencer lecture in Oxford in 1961 and hardly any of the original text was changed before the publication in the volume in 1972 (when Popper also wrote an addendum to the paper).

The later paper Mayr referred to as expressing an improvement on Popper's earlier thinking about natural selection is 'Natural Selection and the Emergence of the Mind', which was delivered as the first Darwin Lecture in Cambridge in 1977. It was published a year later in *Dialectica*. Another important paper where Popper expresses similar thoughts as in the Darwin Lecture is 'The Rationality of Scientific Revolutions', which was originally delivered as another Herbert Spencer lecture in 1973 and was published in the same year. So sometime between 1961 and 1973 he changed his mind significantly about natural selection.

And Popper was apparently quite preoccupied with the question of the analogy between natural selection and the trial and error method of science (see Nanay, 'Popper's Darwinian Analogy' for a summary). In 1963, he asks Campbell for all of his papers on evolutionary epistemology.[13] In 1965, Lakatos says in a letter to Marjorie Grene that 'nowadays, [Popper's] main interest is evolution'.[14]

And in 1967, he wrote to Lakatos: 'You will be thrilled to hear what I have to tell you about the connection of our common philosophy and the great Breakthrough in Biology'.[15] Popper did not elaborate on what this connection is

[11]Popper did acknowledge that he changed his mind on the falsifiability, and hence, on the scientific status, of evolutionary theory. See, for example, Popper, 'Natural Selection and the Emergence', 344–6.
[12]Ernst Mayr to Popper, 31 July 1979, Popper archive, file number 325/1
[13]Popper's letter to Campbell, 3 June 1963, Popper archive file number 282/12
[14]Lakatos to Marjorie Grene, 31 December 1965, Lakatos archive, file number 13/346. Lakatos thanks Grene for sending him her article 'Beyond Darwinism', and he writes: 'I wonder whether you sent a copy to Karl Popper who I am sure would be very interested to read it. Nowadays his main interest is evolution'.
[15]Popper to Lakatos, 9 January 1967, Popper archive file number 318.4-10, Lakatos archive, file number 13/736.

supposed to be, but from the collection of his clippings at that time, it seems likely that he was referring to the 'central dogma of molecular biology' literature on transcription from RNA to DNA that Popper took to question the framework of Weismannian (as opposed to Lamarckian) evolution.[16] More about this in Section III.

To sum up, sometime in the second half of the 1960s, Popper changed his mind about natural selection and Lakatos was in the middle of this transition.[17] Just how he changed his mind and how this change could be attributed to Lakatos' influence is the topic I now turn to.

III.

In order to understand the change in Popper's thoughts on natural selection, we need to be clear about how Popper conceived of the analogy between natural selection and the trial and error method of science before his contact with Lakatos.

In his 1961 lecture, Popper states that the main problem for evolutionary theory is to account for the way in which random mutations can explain the apparent teleology of the natural world.

> [The difficulty evolutionary theory faces is] the difficulty of understanding how a complicated organ, such as the eye, can ever result from the purely accidental co-operation of independent mutations.
>
> (Popper, *Objective Knowledge*, 273)

Note that he talks about purely accidental cooperation of independent mutations. He repeats these phrases over and over in the essay:

> The real difficulty of Darwinism is the well-known problem of explaining evolutions which are *apparently goal-directed* such as that of our eyes, by an incredibly large number of very small steps; for according to Darwinism, each of these steps is the result of a purely accidental mutation. That all these independent accidental mutations should have had survival value is difficult to explain.
>
> (Popper, *Objective Knowledge*, 269–70, emphasis original)

[16] Among his clippings from this period are three Encyclopedia entries on Weisman (one from the 1967 Encyclopedia Britannica) as well as lots of photocopies from the 'the central dogma of molecular biology' literature. Popper archive, file number 384/12.

[17] I do not mean to give a monocausal explanation here and attribute the entirety of these changes in Popper's thinking to Lakatos' influence. There were other philosophers and biologists who Popper was in contact with about these issues (notably, Campbell, Toulmin and Kuhn). It is remarkable though how reluctant Popper was to take their criticism seriously, with regards to the analogy he posited between biological evolution and the evolution of scientific theories – see the affair of his cruelly rejected submission to Nature 'Lamarckism and DNA' in 1973 (see Aronova, 'Karl Popper and Lamarckism') and his correspondence with Ernst Mayr. He did make an exception in the 1960s for Lakatos though, as evidenced by his frequent reference to 'our common philosophy' in Popper's correspondence (Popper to Lakatos, 9 January 1967, Popper archive file number 318.4-10, Lakatos archive, file number 13/736). Nevertheless, it is the direction of the change in Popper's thought that reveals Lakatos' influence, as I'm now hoping to show.

Popper took this to be a problem for biology as well as for an evolutionary explanation for scientific progress. But his attempts to address this problem are far from convincing. In the addendum to the 1961 lecture that was published in 1972, as a rather desperate attempt, he appeals to the idea of 'hopeful monsters' to explain how a series of independent mutations can lead to adaptation.

What is striking is that he did not take the cumulative character of natural selection into consideration: that changes accumulate over the generation and as a result mutations are not at all 'independent'.[18] Here is a very simplified example (where I ignore sexual reproduction and limit the traits relevant to selection to only one). The height of giraffe *x* is 12 feet. She has three offspring, *a*, *b* and *c*. Giraffe *a*'s height is 10 feet, *b*'s is 12 and *c*'s is 14 feet. If the branches are very high up, then *c* is more likely to survive than *a* and *b*. Thus, *c* makes it to the next generation and she has three offspring, *d*, *e* and *f*. As *c*'s height was 14 feet, this will be the trait that gets transmitted to her offspring, who will have the height of 12, 14 and 16 feet respectively. Again, *f*, who has the longest neck is the most likely to survive. And so on. What we have here is a cumulative selection process: changes accumulate. The selected traits in one generation will be different from the ones in the previous generation: *c*'s neck is longer than *x*'s and *f*'s is longer than *c*'s. In the first generation, *x*'s height was 12 feet, in the *n*th generation, the height of the individuals in the population will be close to the height of the lowest branches of the trees in the environment; it will *adapt* to the environment. Thus, this selection process seems to explain why the height of giraffes is the way it is: because those giraffes whose neck was too short had less chance to reach the branches.

There is a significant debate in contemporary philosophy of biology about the constraints under which selection can explain adaptation (Neander, 'Pruning the Tree of Life', 'Explaining Complex Adaptations'; Sober, 'Natural Selection and Distributive'; Nanay, 'Can Cumulative Selection'), but one point of consensus is that if selection is not cumulative, it has no chance of explaining adaptation (see, e.g. Bedau, 'Can Biological Teleology', 650–4; Walsh, 'Chasing Shadows', 142–3; Neander, 'Explaining Complex Adaptations'; Sober, 'Natural Selection and Distributive').

And the cumulative character of natural selection is indeed the most important change between Popper in 1961 and Popper in 1973. As we have seen, he completely ignored the cumulative character of selection in 1961: he talks about 'independent accidental mutations' and asks how these could possibly lead to adaptation (Popper, *Objective Knowledge*, 270). That he took mutations to be independent is an evidence that he did not

[18]The reason for this may be that Popper often contrasted 'cumulative' with 'revolutionary' as attributes of scientific change (see, e.g. Popper, 'The Rationality of Scientific', 12), and he took scientific change to be the latter. Note that what I mean by the cumulative character of selection is consistent with both 'cumulative' and 'revolutionary' scientific change – as Popper used the notion.

take selection to be cumulative. If a selection process is cumulative: changes in a lineage accumulate. In other words, a mutation influences how the lineage will change, and, thereby also influence later mutations in the same lineage.

Popper did not deny that the trial and error method is used repeatedly both in the biological case and in the case of the selection among scientific theories. As he writes back in 1937:

> It is clear that the success of [the trial and error] method depends very largely on the number and variety of the trials: the more we try, the more likely it is that one of our attempts will be successful.
>
> (Popper, *Conjectures and Refutations*, 312)

It is important to highlight, however, that repeating a one-step selection process does not make it a cumulative process (see Nanay, 'A More Pluralist Typology', see also Nanay, 'The Return of the Replicator'). The distinctive feature of a cumulative selection process is that the selective retention influences the next round of variation. If a one-step selection process is repeated from scratch again and again, this is not the case.

The big change in Popper's thinking about selection is that by the early 1970s, he took the cumulative nature of selection more seriously. In 1973, in 'The Rationality of Scientific', he writes:

> It is to be noted that in general no equilibrium state of adaptation is reached by any one application of the method of trial and the elimination of error, or by natural selection.
>
> (Popper, 'The Rationality of Scientific', 4)

So far, this is just a reiteration of the earlier point that a single step of trial and error is unlikely to get us to the adaptive equilibrium. But, importantly, he adds that the main reason why this is so is the following:

> New pressures, new challenges and new problems may arise as a result of the structural changes which have arisen from within the organism.
>
> (Popper, 'The Rationality of Scientific', 4)

This way of thinking about selection, in contrast with what Popper held in 1961, takes the cumulative nature of selection seriously. Not only does he hold that selection is a multi-step process, he also acknowledges that earlier steps in the selection process influence later ones.

In short, by 1975, Popper came to believe that if selection is to explain an adaptive equilibrium, it should be cumulative. Further, it needs to be noted that Popper seems to take the cumulative nature of selection among scientific theories more seriously than he does in the biological domain. When he elaborates on the cumulative character of selection in the biological case, he is less than convincing: he talks about 'the time span of a few generations – at the very least, say, one or two generations' (Popper, 'The Rationality of Scientific', 6). Needless to say that the time span of one or two generations is extremely unlikely to be enough for any gene-based cumulative selection process to reach adaptive equilibrium.

He even explicitly acknowledged this asymmetry in his thinking about the cumulative nature of biological selection and the selection among scientific theories. In the case of biology, mutations are 'blind': 'the survival of a mutation cannot influence the further mutations' (Popper, 'The Rationality of Scientific', 5). He contrasts this with mutations of scientific theories that are not 'blind' in this sense.

In short, Popper changed his mind about the cumulative character of selection. And this change is unlikely to have originated from the thorough analysis of gene-based natural selection – as it appears that he still did not fully appreciate what cumulative selection would imply in the biological case. And, as a result, he helped himself to dubious considerations like the idea of 'hopeful monsters' and Lamarckian inheritance when trying to explain how random mutations can explain the apparent teleology of the biological domain.

My claim is that this change is a consequence of Lakatos' influence. Lakatos is very explicit that scientific research programmes are not sets of theories but 'a temporal chain of sets of theories'.[19] If a scientific research programme faces an objection, it lives on (maybe acquiring a bit of protective belt). In other words, scientific research programmes can and do change. The set of theories in a scientific research programme at time t is different from the set of theories in it at some later time, t^*. And what set of theories we get in a research programme at time t^* depends on what happens to the research programme at time t – what objections it faces and how it can handle them.

If we conceive of selection as a process that operates on scientific research programmes, this would lead to a cumulative selection process. Research programmes can and do change and their changes accumulate. If a research programme faces an objection at time t, this leads to theories in this research programme at time t^* that would no longer face this objection. And this process goes on (until the research programme is superseded by another one).

According to Popper, unsuccessful theories are eliminated by objections. According to Lakatos, scientific research programmes (or at least their protective belts) are changed by objections (but they still live on). For Popper the trial and error method consists of one step only: the successful theory lives on, the unsuccessful gets eliminated (of course, then the process can be repeated). For Lakatos, the life span of a scientific research programme consists of constant cycles of objections and responses to objections, and these responses are built into their protective belts.

[19]Lakatos ('Falsification and the Methodology', 'Science and Pseudoscience'). Lakatos makes this especially clear in a letter to Yehuda Elkana in 1973: Lakatos to Yehuda Elkana, 29 March 1973. Lakatos archive, file number 13/250.

Thus, Lakatos' scientific research programme can be gradually fine-tuned: the set of theories that comprise the research programme at time *t** can deal with more objections than the set of theories at time *t*, which precedes *t**. The 'mutations' of the research programme are not completely 'blind' in the sense Popper thinks genetic mutations are blind: it is not the case that 'the survival of a mutation cannot influence the further mutations' (Popper, 'The Rationality of Scientific', 5). In the case of a scientific research programme, the way it can deal with objections does influence what directions it develops into in the future.

Lakatos' insistence on the temporal dimension of research programmes makes it possible for selection among scientific theories to be cumulative. And if selection is cumulative, it is not a 'purely accidental co-operation of independent mutations' as the early Popper suggested (Popper, *Objective Knowledge*, 273). It is the co-operation of sequences of mutations whereby the earlier ones influence the latter ones: a 'mutation' influences how the research programme will change and the next 'mutation' will depend on how the research programme has changed.

In short, what Popper learned from Lakatos is that if we take the temporal dimension of selection among scientific theories seriously, we can account for the cumulative nature of this selection process, which thereby will allow us to explain the 'equilibrium state of adaptation' in this domain (Popper, 'The Rationality of Scientific', 5).[20] In other words, he learned from Lakatos that in the case of the selection among scientific theories, change is not the 'purely accidental co-operation of independent mutations' (Popper, *Objective Knowledge*, 273).

We can sum up the change in Popper's evolutionary thinking in the following manner. The early (pre-1961) Popper postulated a symmetry between natural (gene-based) selection and selection among scientific theories. As he perceived gene-based selection to be the 'purely accidental co-operation of independent mutations', he assumed that the same is true of the selection among scientific theories. But this posed the problem about how selection, conceived this way, could possibly explain complex adaptation – both in the biological domain and in the domain of scientific theories.

Lakatos' influence was to persuade Popper that selection, at least selection in the domain of scientific theories, is cumulative: it is not a 'purely accidental co-operation of independent mutations'. But if this is so, then Popper faces a dilemma. He should either give up on the analogy between gene-based

[20]It is clear from Lakatos' correspondence that his theory of the methodology of scientific research programmes goes back at least to 1964 (see, e.g. Lakatos archive, file number 13/736), which is exactly when Popper's interest in biology is revived. Given that Lakatos and Popper at this time were working together very closely, it is difficult to believe that Popper would not have heard about the idea of research programmes from Lakatos as early as anyone else. See also footnote 21 below for more wrinkles.

selection and selection in the domain of scientific theories, or he should find some reason to believe that gene-based selection is not a 'purely accidental co-operation of independent mutations' either. He seemed to vacillate between these two options in 'The Rationality of Scientific', where he explicitly follows the former strategy some of the time (Popper, 'The Rationality of Scientific', 5) but makes gestures to the latter one at other times (Popper, 'The Rationality of Scientific', 4).

Further, his enthusiasm for 'the Break-through in Biology',[21] which he could not wait to share with Lakatos, should be interpreted in the light of this dilemma. His clippings about Weismann and the 'central dogma of molecular biology' literature suggest that what he took to be 'the Break-through in Biology' was that the flow of information is not unidirectional from the DNA to the RNA, which he presumably interpreted as going against the Weissmannian orthodoxy that the germ-line is never influenced by whatever happens to the organism during its life. If this were so, then gene-based selection would be more similar to the quasi-Lamarckian process of selection in the domain of scientific theories – the analogy between gene-based and scientific selection would be restored.

Popper never, not even in 'Natural Selection and the Emergence of Mind' (where he carefully avoided the issue), seemed to give the right analysis for the cumulative character of gene-based selection – namely, that it is not the case that 'the survival of a mutation cannot influence the further mutations' (Popper, 'The Rationality of Scientific', 5). It can influence the further mutations *of the next generation* – and one does not need to deny Weismannian inheritance in order to account for this.

But Popper's analysis of gene-based selection is a secondary issue here. The main point is that he changed his account of selection in the domain of scientific theories, even though he had to pay the price of giving up the analogy between gene-based selection and selection in the domain of scientific theories. And, as I hoped to show, he could change his account of selection in the domain of scientific theories in such a way that allows for cumulative selection only because he took Lakatos' insistence on the temporal dimension of scientific research programmes seriously. This is what Popper learned from Lakatos.[22]

[21]Popper to Lakatos, 9 January 1967, Popper archive file number 318.4-10, Lakatos archive, file number 13/736.

[22]This also explains the somewhat passive aggressive first footnote of Popper, 'Darwinism as a Metaphysical Research', where he is clearly trying to assert his intellectual copyright for the very idea of research programmes: 'The term "metaphysical research programme" was used in my lectures from about 1949 on, if not earlier; but it did not get into print until 1958, though clearly in evidence in the last chapter of the Postscript (in galley proofs since 1957). I made the Postscript available to my colleagues, and Professor Lakatos acknowledges that what he calls "scientific research programmes" are in the tradition of what I described as "metaphysical research programmes"' (Popper, 'Darwinism as a Metaphysical Research', 133).

A couple of important questions need to be raised about this interpretation of Lakatos' influence on Popper.

First, if appeal to Lakatos' scientific research programmes did change Popper's evolutionary epistemology, why didn't Lakatos himself notice these implications of his account? The short answer is that he probably did.

On the margins of a reprint of Toulmin's 'From Logical Systems' he explicitly compares the 'temporal sequence of populations' to scientific research programmes (which, as we have seen, he considered to be a 'temporal chain of sets of theories').[23]

Another important indication that Lakatos may have seen the consequences of his theory to Popper's evolutionary epistemology is a note on the margins of a reprint: a reprint of Popper's 'Conjectural Knowledge' (1971). Referring to Warnock 1960's, 'Review of "The Logic of Scientific"' criticism, Popper writes:

> Some people thought that the phrase 'prove its fitness to survive' shows that I had here intended to speak of a fitness to survive in the *future*, to stand up to future tests. I am sorry if I have misled anybody, but I can only say that it was not I who mixed the Darwinian metaphor. Nobody expects that a species which has survived in the past will therefore survive in the future. […] It would be absurd to suggest that Darwinian survival involves, somehow, an expectation that every species that has so far survived will continue to survive.
> (*Objective Knowledge*, 19, emphasis original)

Lakatos' comment on the margin seems to suggest that he thought that although past survival does not guarantee future survival, it does make it more likely – at least in the case of scientific theories.[24] Discounting the shadows of probabilistic inductivism of this remark, what is important here is that Lakatos did take Popper's fitness analogy seriously. He also marks the entire paragraph with 'Has to be taken note of'.[25]

However, it is difficult to deny that Lakatos always stayed away from any serious discussion of evolutionary epistemology or even of evolutionary explanations in general (except for taking it as an example for a scientific theory). Although comparing the progress of scientific theories to natural selection was extremely widespread among Lakatos' contemporaries (even Kuhn could not resist it – Kuhn, *The Structure of Scientific*, 172), Lakatos never even mentioned such an analogy. Why?

The most plausible answer is that he, like the later Popper, saw some serious discrepancy between selection in biology and selection in the

[23]Lakatos notes on Toulmin, Lakatos archive, file number 15/102.

[24]Lakatos notes on Popper, Lakatos archive, file number 15/90. He wrote 'NO. More likely' next to the last sentence.

[25]It is important that Lakatos really hated this essay. In an unsent letter to Popper, he wrote that 'I also wish to congratulate you on *Objective Knowledge*. Apart from the moral and intellectual disaster of the first two chapters, it is a masterpiece'. Lakatos to Popper (unsent) 5 February 1973, Lakatos archive 13/736/172.

domain of science. But his way of responding to this discrepancy was to reject the analogy altogether. Popper was looking for models of biological selection that would restore the asymmetry, but Lakatos seems to have proceeded the other way and concluded that the evolutionary analogy is misguided. Scientific research programmes are not eliminated by conflicting data: they undergo change in response to conflicting data. But evolution by natural selection in the biological domain does not work this way.

Lakatos had misgivings about the analogy between gene-based selection and selection in the domain of scientific theories. Popper did too, as a result of Lakatos' influence, but he did not give up on this analogy altogether. He was looking for ways in which this analogy could be restored. His early thinking about selection and science was inspired by the prevailing model of selection in biology. But he spent the end of his life looking for biological models of selection in biology that would be consistent with the model of selection in the domain of scientific theories he learned from Lakatos.

It is worth noting that there may be a way of conceiving of gene-based natural selection on the analogy of the selection among scientific research programmes. The analogue of scientific research programmes (as units of selection) in the biological domain would be lineages: temporal sequences of individuals connected by means of inheritance. Individual *x*, *x*'s parent, *x*'s parent's parent, etc., for example, form a lineage. Lineages do not (always) get eliminated in response to interaction with the environment. They change and live on. They live for many many generations, changing and adapting to their environment. Just like Lakatos' scientific research programmes. Thus, if we take Popper's attempt to restore the symmetry between gene-based selection and selection in the domain of scientific theories seriously, we should interpret gene-based natural selection as lineage selection. This option was not really open to Popper and Lakatos, as the possibilities of taking lineages as the units of selection was unexplored in the seventies (it is also quite unexplored now, with some very isolated exceptions, e.g. Brandon, *Adaptation and Environment*).

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