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The Use and Abuse of Sir Karl Popper

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Abstract. Karl Popper has been one of the few philosophers of sciences who has influenced scientists. I evaluate Popper's influence on our understanding of evolutionary theory from his earliest publications to the present. Popper concluded that three sorts of statements in evolutionary biology are not genuine laws of nature. I take him to be right on this score. Popper's later distinction between evolutionary theory as a metaphysical research program and as a scientific theory led more than one scientist to misunderstand his position on evolutionary theory as a scientific theory. In his later work Popper also introduced what he took to be "improvements" of evolutionary theory. Thus far these improvements have had almost no influence on evolutionary biology. I conclude by examining the influence of Popper on the reception of cladistic analysis.

Key words: cladistics, evolutionary theory, Lamarckism, metaphysical research programs, operationalism, Popper, stage laws, tautology, testability

> Hasn't Popper argued that evolution is not really respectable science, that it's all some rather boring tautology and is not to be taken terribly seriously? – Lewis Wolpert.
>
> Well, I think Popper himself would (a) claim that he never said it, and (b) claim that if he did say it, he didn't mean it. – John Maynard Smith. In Wolpert and Richards (1988, p. 135).

Philosophers commonly complain that they get insufficient attention, let alone respect. Karl Popper is a major exception to this complaint. Perhaps professional philosophers do not lavish the attention on Popper that Popper (1982), not to mention the Popperians, thinks he deserves (Bartley 1982; Skoyles 1992), but scientists and even the general public take certain parts of Popper's work very seriously indeed, in particular his demarcation principle for science. Scientific theories, in order to be genuinely scientific, must be falsifiable.[1] Popper has been influential. The question I address is whether this

influence has always been positive, especially with respect to evolutionary biology.

The common view is that Popper made some injudicious remarks about evolutionary theory early in his career, but when he himself adopted a sort of "evolutionary epistemology", he was forced to reevaluate his earlier criticisms or, as he put it, to "eat some humble pie" (Popper 1972, p. 241). After all, if Popper proposed to support his general views about science by reference to evolutionary theory, then this theory must not be in too bad a shape. And as luck would have it, Popper came to see the error of his ways just in time to combat the misuse of his views by Creationists. The actual story turns out to be a good deal more complicated.

Popper began by arguing that there are no "evolutionary laws." Given the three sorts of statements that he counts as evolutionary laws, he is right. They are not laws, and he never changed his mind on this subject. Not until Popper introduced his "evolutionary epistemology" did he raise his most serious objections to the Neo-Darwinian version of evolutionary theory. In particular, he resurrected the old complaint that the principle of the survival of the fittest is a tautology or, as he sometimes put the complaint, "almost a tautology." As a result, he argued, evolutionary theory is untestable and, hence, not really a scientific theory. Popper then noted that he was using such terms as "Darwinism" in two senses: sometimes as a metaphysical research program and sometimes as one instantiation of that program. Metaphysical research programs are not supposed to be testable, at least not in the direct way that scientific theories must be testable (Popper 1974b, p. 137). But Popper (1972, p. 137) continued to maintain that the Neo-Darwinian version of evolutionary theory as a scientific theory remained so "feeble" that there is "hardly any possibility of testing" it. To make matters worse, Neo-Darwinians seemed willfully blind to numerous weaknesses of their theory. Popper then suggested ways of remedying these deficiencies.

Then, in 1978, Popper published another recantation. This time he made some very strong statements in favor of evolutionary theory and retracted his claim that the principle of the survival of the fittest is a tautology and, hence, not testable. He still thought that the distinction between Darwinism as a metaphysical research program and Darwinism as a biological theory was worth making, but he also thought that the theory of natural selection is testable – difficult, yes; impossible; no. In fact, the theory of natural selection in its most sweeping form is not only refutable but actually refuted! However, in the face of this recantation, he still maintained that Neo-Darwinian versions of evolutionary theory have serious weaknesses and adds one more criticism to those that he had set out previously.

One result of all this maneuvering was that two groups (Creationists and pattern cladists) were able to use Popper's writings to downgrade evolutionary theory. People in general have social obligations. Scientists and philosophers are no exception. Critics of science (and philosophy) argue that all intellectuals are obligated to be aware of the most likely ways in which their views can be misused and to couch their ideas in ways that make such abuse more difficult. I am not sure about how socially responsible intellectuals really need to be, but if this moral prescription applies at all, it applies in the case of Popper and his views about evolutionary theory. Popper (1978, p. 342) argued that the "search for truth presupposes ethics." The issue is the nature of these ethical obligations.

Stage laws and evolutionary philosophies

Early in his career, Popper did say some "contemptuous" things about what he termed "evolutionary philosophies." In his *The Poverty of Historicism*, Popper (1957, pp. 106–107) noted that the assumption of the "common ancestry of related forms" is a hypothesis, "not a universal law." It has the "character of a particular (singular or specific) historical statement." Certain universal laws of nature, like the "laws of heredity, segregation, and mutation", enter into the explanation of this hypothesis, but it itself is not a universal law of nature. Nor are statements such as "All vertebrates have one common pair of ancestors" universal laws of nature because they refer only to organisms living on Earth, "rather than to all organisms at any place and time which have the constitution which we consider as characteristic of vertebrates."

Five years later, when Popper published his *Conjectures and Refutations* (1962, p. 340), his opinion of evolutionary laws had not changed:

> There exists no law of evolution, only the historical fact that plants and animals change, or more precisely, that they have changed. The idea of a law which determines the direction and the character of evolution is a typical nineteenth-century mistake . . . .

Popper is certainly right that in the nineteenth century such stage theories of directional change were common (see Blute 1979). Some of Darwin's fellow Darwinians hoped to find comparable "laws" leading from monads to man. Popper cites T. H. Huxley as holding such a view. According to Huxley (1880, p. 214), all who are "competent to express an opinion on the subject" agree that all plants and animals came into existence in a "definite order, the statement of which order is what men of science term a natural law." Thus, if Huxley is right about the terminology of his day, a description of the phylogenetic tree counts as a "natural law."

In the preceding discussion, three quite different sorts of statements are being run together:

(a) Descriptions of particular phylogenetic sequences, such as birds evolving from dinosaurs.
(b) The statement that all terrestrial organisms come from a single common ancestor.
(c) More abstract statements about general trends in evolution.

Apparently Popper was unaware of how radical his first view actually was. For Popper, statements such as the placoderms gave rise to the ostracoderms was akin to the claim that Charles Darwin gave rise to Leonard Darwin. Both sorts of statements are *singular* in form. As radical as this view may be, I think that Popper is right. If taxa must be *monophyletic* and defined in terms of *evolutionary homologies*, then all taxa, including species, are necessarily spatiotemporally restricted – and one thing that Popper thinks laws must be is spatiotemporally *un*restricted (Popper 1957, p. 103; 1959, p. 252; 1972, p. 193). That "*Dodo ineptus* is extinct" is as much a singular statement as is "Darwin is dead." Anyone who doubts how radical this view was at the time need only turn to the extensive literature that resulted when Michael Ghiselin (1974) and I (Hull 1976) argued precisely this same point.

But not all "evolutionary laws" mention particular taxa. For example, the claim that all life evolved from a single progenitor does not refer to any particular taxa. It may look universal in form. It is, if "all life" refers to any and all living creatures throughout the universe; it is not, if it refers only to terrestrial life. Because all terrestrial organisms use the same genetic code, they are likely to have evolved from a single terrestrial progenitor. Hence, the statement that all *terrestrial* life evolved from a single progenitor is probably true, but it is not sufficiently general to count as a law of nature, at least not as the term was used at the time by Popper and other philosophers of science.

Life in general is quite another story. Living creatures certainly have evolved hundreds of times in various planets throughout the universe. "Life" and "living creatures" do not refer to spatiotemporally localized groups of entities. The development of living from nonliving matter is the sort of thing that can happen over and over again and no doubt has. The problem with the statement that all life evolved from a single progenitor is that it is surely false.[2]

However, the real targets of Popper's criticisms were quite different sorts of statements – stage laws. Popper was certainly right that the nineteenth theory was rife with claims that first *A* happens, and then *B*, and then *C* – over and over again. Such claims had considerable support in embryology. Many organisms do repeat the same developmental sequences, but nineteenth

century scientists generalized from embryology to all nature. For example, Spencer (1857) attempted to claim priority over Darwin because of his "law" that life evolves from the homogeneous to the heterogeneous. What Popper did not like about such claims in biology is that they gave indirect support to comparable claims about human societies, in particular Marx's doctrine that human societies proceed from the primitive to the feudal, from the feudal to the capitalist, and from this stage finally to the ultimate socialist society. According to Popper (1972, p. 241) such claims were not "inexorable laws" but at most trend statements, and statements of such trends do not count as scientific laws.

That Popper was contemptuous of such "evolutionary laws" may be a bit whiggish, but it is understandable. On the view of laws of nature common among philosophers in 1972, none of the statements to which Popper refers count as laws of nature. Exactly why Popper thought he was eating humble pie on this score is puzzling, since he continued to reject the lawful character of such statements throughout his life, as did the Darwinians at the time. Perhaps Rensch (1971) could publish a list of an even hundred "evolutionary laws", but Simpson (1964) and Mayr (1963) did not think that the evolutionary process could be characterized in these terms. In rejecting such formulations as laws, Popper was joining in the Neo-Darwinian consensus. It should also be noted that in his early writings, Popper never complained about evolutionary theory as a process theory. His objections concerned statements about the products of this process.

What is really wrong with evolutionary theory?

After rejecting views that had been widely accepted in the nineteenth century but largely rejected by the time that Popper came along, Popper (1972, pp. 241–242) turned to what he found wrong with the evolutionary theory of his own day – not claims about phylogeny but the theory itself:

> Quite apart from evolutionary *philosophies*, the trouble about evolutionary *theory* is its tautological, or almost tautological, character: the difficulty is that Darwinism and natural selection, though extremely important, explain evolution by 'the survival of the fittest' (a term due to Herbert Spencer). Yet there does not seem to be much difference, if any, between the assertion 'those that survive are the fittest' and the tautology 'those that survive are those that survive'. For we have, I am afraid, no other criterion of fitness than actual survival, so that we conclude from the fact that some organisms have survived that they were the fittest, or those best adapted to the conditions of life.

One does not have to be a pedant to find problems with the claim that a statement is "almost" a tautology. In matters such as these, a miss is as good as a mile or, to mix trite metaphors, it is like being almost pregnant. A statement is either a tautology or it is not. Elsewhere, Popper (1972, p. 69) makes himself clearer. Evolutionary theory in its entirety is not tautological; only a "considerable part of Darwinism is not of the nature of an empirical theory, but is a *logical truism*."

Assuming that the principle of the survival of the fittest is a tautology, it follows that what purports to be the main mechanism for driving the evolutionary process has no empirical content. Tautologies are not falsifiable. Hence, if falsifiability is necessary for a theory being genuinely scientific, then this part of evolutionary theory is not genuinely scientific. But if evolutionary theory is not a scientific theory, then what is it? A couple of years later, Popper (1974b, p. 134) answered this question. It is a metaphysical research programme:

> From this point of view the question of the scientific status of Darwinian theory – in the widest sense, the theory of trial and error-elimination – becomes an interesting one. I have come to the conclusion that Darwinism is not a testable scientific theory, but a *metaphysical research programme* – a possible framework for testable scientific hypotheses.

A series of confusions and ambiguities make Popper's evaluation of Darwinian theory as a metaphysical research program sound more negative than it actually was. Popper introduced his principle of falsifiability to distinguish between science and *pseudoscience*. In order for a formulation to count as pseudoscience, it has to pretend to be scientific. Coleridge's views on poetry are no more pseudoscientific than is a recipe for fudge cake. Neither purports to be scientific. In its original usage, Popper's principle of falsifiability implied something negative about the pseudoscientific formulations it was designed to distinguish from genuine science, but one can expand Popper's principle of falsifiability to distinguish between science and *nonscience*. On this usage "nonscience" carries no negative connotations. Lots of what counts as "nonscience" is still extremely valuable. Included in this catchall category is metaphysics. Even before the positivists took out after metaphysics, it had a bad reputation in many quarters. "Metaphysical" in common parlance tends to have a negative connotation. It is just so much high-flown gobbledegook.

When Popper termed Darwinism a "metaphysical research programme", he did not mean anything negative by the appellation. After all, this was what *he* was engaged in, and he was certainly not a positivist.[3] In fact, Popper proposed to publish a *Postscript* to the German edition of his *Logik*

der Forschung (1934) at the same time that he published his *The Logic of Scientific Discovery* (1959), explaining his views on metaphysics, but eye surgery made it impossible. In any case, Popper's goal was to set out a general analysis of selection theories. Then by substituting classes of real entities, such as genes and species, this general formulation could be converted into a biological theory of evolution, or by substituting the relevant cultural kinds, it could be interpreted as a theory of cultural evolution. I for one am in no position to complain about such an activity because I have been engaged in this same research program for many years (Hull 1988).

Even if we go along with Popper's two senses of "Darwinism", he still is in trouble. Metaphysical research programs are not empirical in the direct way that scientific theories are, though Popper thinks that they can be tested (Popper 1974b, p. 137), but how about the original claim that Darwinian theory cannot be tested because the principle of the survival of the fittest is a tautology? We can begin with Darwin himself. In the *Origin of Species*, he specified several phenomena that, if they occurred, would force him to abandon his theory:

> If it could be demonstrated that any complex organ existed, which could not possibly have been formed by numerous, successive, slight modifications, my theory would absolutely break down (Darwin 1859, p. 189).
>
> [Some naturalists] believe that very many structures have been created for beauty in the eyes of man, or for mere variety. This doctrine, if true, would be absolutely fatal to my theory (Darwin 1859, p. 199).
>
> If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection (Darwin 1859, p. 201; see also p. 211).
>
> [I have discussed the adaptations of neuter insects] at some little but wholly insufficient length, in order to show the power of natural selection, and likewise because this is by far the most serious special difficulty, which my theory has encountered (Darwin 1859, p. 242; see also p. 239).
>
> The abrupt manner in which whole groups of species suddenly appear in certain formations, has been urged by several paleontologists .... as a fatal objection to the belief in the transmutation of species. If

> numerous species, belonging to the same genera or families, have really started into life all at once, the fact would be fatal to the theory of descent with slow modification through natural selection (Darwin 1859, p. 302).
>
> He who rejects [the imperfection of the geological record] will rightly reject my whole theory (Darwin 1859, p. 342).

Popper should have been very favorably impressed with the author of the *Origin of Species* because here he is specifying in advance which phenomena would be fatal to his theory (Popper 1972, pp. 70, 247). Of course, we should take Darwin's claims quoted above with a grain or two of salt. Time and again, scientists will say that such-and-such would be fatal to my theory, but once it becomes clear that such problem cases do occur, scientists rework their theory to incorporate these phenomena.

The first quotation from Darwin concerns both the tempo of evolution and the nature of the process. Darwin thought that evolution was gradual, not because gradualness was all that apparent in the fossil record (it was not) but because his mechanism implied that it must be. Organisms, especially multicellular organisms, are so finely organized that any major change in their makeup would surely cause their death. Although large changes might very rarely play a role in evolution, the vast majority of alterations must be small. As a result, evolution is gradual. Darwin also thought that cataclysmic, saltative change smacked of miracles.

The second phenomenon quoted above raised the issue of supernatural agency even more directly. Darwin opposed special creation primarily because it invoked supernatural causes, and if one miracle can be introduced, nothing prevents the introduction of countless other miracles as well. Darwin's third example addresses natural selection directly. According to his view, one organism can possess a structure that incidentally helps organisms belonging to other species, but this structure must have evolved *initially* for the good it does for the organisms that possess it. The adaptations exhibited by neuter insects posed an equally serious problem for natural selection. The hereditary transmission of characters is necessary on Darwin's theory for the gradual construction of adaptations, but neuter insects never reproduce themselves. Then how do such adaptations develop? Darwin's answer involved a mixture of what we now term group selection and kin selection.

Finally, what if whole groups of species sprang into existence simultaneously the way that Charles Lyell thought that they did? If the process was supernatural, then it would threaten the naturalistic character of Darwin's theory. If, to the contrary, a natural mechanism existed for such an occurrence, it would also threaten Darwin's theory. We would not need natural selection to explain the gradual and halting evolution of birds if birds simply sprang fully

developed from their ancestral species or from something else like gemmules lying dormant in the earth. The issue is not that these alternatives are highly implausible but that they are genuine alternatives to Darwin's theory. If the fossil record is reasonably complete, then whole groups of species *do* seem to emerge quite rapidly. Hence, Darwin has to argue that the fossil record is punctuated with significant gaps.[4]

Certainly Darwin seemed to treat the principle of the survival of the fittest as if it needed empirical support, but how about contemporary formulations? The issue with respect to the status of the principle of the survival of the fittest turns on how evolutionists operationalize such theoretical terms as "fitness". One quick way to facilitate mathematical manipulation is to define "fittest" in terms of "survival." No population geneticist thinks that populations of organisms are infinitely large, but for the purpose of increasing the ease of mathematical manipulation, they periodically will assume, for sake of argument, that they are. Similarly, population biologists will periodically treat the principle of the survival of the fittest as if it were a tautology in order to make the mathematics more tractable. Popper (1972, p. 271) notes this source of the problem, when he states that Darwin's central idea has suffered an "eclipse" because of the the "fashionable pursuit of mathematical exactness."

Evolutionary biologists are well aware that fitness must be operationalized in a more empirical way. This need parallels a comparable need in behavioral psychology. "Intelligence" cannot be defined ultimately in terms of scores on IQ tests without making claims about the correlation between IQ scores and intelligence vacuous. One problem with investigating the notion of how these terms are to be operationalized is that very little has been said by philosophers of science or scientists for that matter about how to operationalize concepts. Philosophers have shown that operationism as a general theory of meaning is bankrupt, but we have said precious little about how concepts are to be operationalized, in part because this process seems highly contingent and particularized, the very sort of phenomena that we philosophers shy away from. And scientists themselves just *do* it, they just operationalize their concepts without saying much about *how* they do it. If some of my fellow philosophers could be lured away from Twin Earth and incommensurability, they would find this topic rewarding.[5]

The philosophical literature on fitness is extensive, so extensive that philosophy of biology is sometimes characterized snidely as the philosophy of fitness. A hint that the survival of the fittest has to be more than a tautology can be gathered from how extensive this literature actually is. Hundreds of papers and dozens of books devoted to working out the consequences of a tautology seem unlikely. Either numerous very sophisticated philosophers and scientists are being duped, or else the survival of the fittest is somewhat

more than a tautology. To make matters even more pointed, probably the most popular analysis of "fitness" is in terms of propensities, a notion that Popper should find especially appealing (Mills and Beatty 1979). However, I have been unable to discover whether or not Popper read any of this literature.

In any case, Popper finally came around to the view that he had been mistaken about the survival of the fittest being a tautology. In a paper entitled "Natural Selection and the Emergence of Mind", Popper (1978) states unequivocally that natural selection, though difficult to test, is in principle testable. If in fact the principle is interpreted as claiming that *all* organisms have evolved solely under the action of natural selection, it is false because of sexual selection and drift. Sexual selection is not part of natural selection because the two can work at cross-purposes, and drift cannot possibly be subsumed under natural selection. How was Popper led to make such a mistake? He had been misled by such authorities as R. A. Fisher, J. B. S. Haldane, and G. G. Simpson (Popper 1978, p. 344).

Popper's improvements of evolutionary theory

Even though Popper eventually became reconciled with the principle of the survival of the fittest, throughout his career he maintained serious doubts about evolutionary theory. It was "in need of a restatement which makes it less vague" (Popper 1972, p. 242), and periodically he himself suggested "slight improvements" in the theory. Although Popper (1974b, p. 133) set out these improvements as tentatively and humbly as possible, biologists at the time were not all that enthusiastic about his suggestions. In fact, Ernst Mayr urged Popper not to publish his Spencer Lecture when he first delivered it in 1961 (Popper 1972, p. 281).

This lack of enthusiasm from biological experts may well have intensified the low opinion that Popper had of Neo-Darwinians as scientists. For example, early on Popper (1957, p. 106) noted the "emotional attitude" that so many evolutionists have towards evolution and their tendency to accuse anyone who does not share in this emotional attitude of "obscurantism." To deflect their wrath, Popper felt obligated to explicitly acknowledge that modern Darwinism provides the "most successful explanation of the relevant facts." Later, Popper (1972, p. 271) wondered how so many Neo-Darwinians remain "almost blind" to the "countless difficulties of Darwin's theory."

Scientists frequently are emotional and seemingly blind when it comes to criticisms of their most fundamental views, especially if these criticisms have been countered time and again and the latest author of these criticisms seems unaware of this literature. And I might add philosophers on occasion are just as emotional and blind as scientists when it comes to their own basic

principles, as critics of Popper and the Popperians have amply discovered. A bit of self-reference is always a good antidote to irate indignation.

Scientists are as territorial as any other professionals, but the silence that greeted Popper's excursions into biology had other sources as well. Popper was not all that interested in evolutionary theory as a biological theory. He was more interested in it as a metaphysical research programme that could be instantiated with respect to the evolution of science. He also was interested in the theory of natural selection because of the strong argument it provided for the "doctrine of *mutual interaction* between mind and body or, perhaps better, between mental states and physical states" (Popper 1978, p. 350).[6]

Popper has the right, of course, to his own interests. In my own work on evolutionary theory (Hull 1978, 1980, 1982, 1988a,b,c), I have always had one eye on possible implications for conceptual evolution. Like Popper I was attempting to formulate a general analysis of "selection" that would be equally applicable to a wide variety of selection processes, including those that occur in science (Hull, Langman and Glenn, forthcoming). However, differences in focus can explain the failure of biologists to take Popper's views seriously.

For example, the most fundamental distinction in Popper's discussion is between Lamarckian and Darwinian evolution. In general, evolutionary biologists are sensitive when it comes to Lamarck. "Lamarckian" can mean just about anything to anyone (Hull 1984). Time and again, newly discovered processes are termed "Lamarckian", and the discoverers of these processes claim that they have thus totally refuted Neo-Darwinism. Just as regularly such processes are shown either not to occur or to fit neatly into the Darwinian version of evolution (see review of Jablonski and Lamb 1995 by Cooke 1995).

By the time that Popper began publishing on biological evolution, Lamarckism had been firmly refuted, and Popper joined with the Neo-Darwinians in rejecting Lamarckian inheritance. What may have puzzled biologists was the prominence that Lamarckian versus Darwinian inheritance played in Popper's writings. From the perspective of *biological* evolution, this old chestnut did not deserve such a prominent place. But Popper was not interested in biological evolution as a biological phenomenon. He was more interested in *cultural* evolution – primarily the evolution of science – and in this context Lamarckism was alive and well. Supposedly one of the chief differences between biological and cultural evolution is that the latter but not the former is "Lamarckian."[7]

As all authors do, Popper approached his subject from *his* perspective, even if it differed from the concerns of the local experts. For Popper the chief distinction was between Lamarckian *instruction* and Darwinian *selection*.

Maybe Neo-Darwinian population geneticists were not interested in Hume's problem of induction, but Popper (1972, pp. 96–97) was:

> The assertion that we have an irrational inclination to be impressed by habit and repetition is something quite different from the assertion that we have a drive to try out bold hypotheses which we may have to correct if we are not to perish. The first describes a typically Lamarckian procedure of instruction; the second a Darwinian procedure of selection. The first one is, as Hume observed, irrational, while the second seems to have nothing irrational in it (see also Popper 1974c, p. 1023).

What Popper has in mind by terming a Lamarckian procedure "instructive" is that the environment impresses beliefs upon us. After enough observations, we are forced to realize that planets travel in ellipses. Whereas, a Darwinian scientist sets out bold hypotheses on the basis of very little, if any, observational data.

I think that the distinction that Popper makes is clear enough, but I must admit that I cannot see much connection between this distinction and anything that Lamarck or Darwin may or may not have said. If the contrast between Lamarck and Darwin was intended to illuminate his distinction between instruction and selection, all I can say is that it had the opposite effect on me. Although Popper (1974a, p. 34) dismisses terminological choices as basically irrelevant, terminology can he helpful or misleading. In this instance, I find his terminology seriously misleading.

In biological evolution, the distinction between Lamarckian and Darwinian inheritance (and hence evolution) turns on the genotype-phenotype distinction. Because this distinction is not at all clear in the context of conceptual change, the issue gets extremely complicated. Anyone who is interested in these terminological and conceptual snarls can read Hull (1988a,b) and Kantorovich (1989). In any case, Popper agrees with biologists that biological evolution is not Lamarckian in a literal sense. He adds that the evolution of science is also not "Lamarckian" in his metaphorical sense. On this score, many scientists remain stubbornly "inductivists" in their pronouncements, if not in their practice (see later discussion of cladism).

Yet another reason that biologists tended to be put off by Popper's suggestions for improving evolutionary theory is that Popper had a decided penchant for the views of borderline and maverick scientists. J. M. Baldwin, R. B. Goldschmidt, and C. H. Waddington were well-established scientists, but they urged versions of evolutionary theory which were, at the time, nonstandard. Among evolutionary philosophers, the only one who impressed Popper (1975, p. 133) was Samuel Butler. Popper even cites positively a book by the Creationist, Norman MacBeth. It was MacBeth's *Darwin Retried* (1971) that led Popper to think that "it might be time to revive Goldschmidt's

'hopeful monsters' in a new form." Such references are not well calculated to engender confidence among evolutionary biologists as to Popper's judgment.

As far as substantive improvements to Neo-Darwinism are concerned, Popper (1974b, p. 138) makes two suggestions:

> (A) I distinguish external or environmental selection pressure from internal selection pressure. Internal selection pressure comes from the organism itself and, I conjecture, ultimately from its *preferences* (or "aims") though these may of course change in response to external changes.
>
> (B) I assume that there are different classes of genes: those which mainly control the *anatomy*, which I will call *a*-genes; those which mainly control *behaviour*, which I will call *b*-genes. Intermediate genes (including those with mixed functions) I will here leave out of account. The *b*-genes in their turn may be similarly subdivided into *p*-genes (controlling *preferences* or "aims") and *s*-genes (controlling *skills*).

In later writings Popper expands on improvement A. According to Popper (1984, p. viii; see also 1984, p. 13):

> Darwinism teaches that organisms become adapted to the environment through natural selection. And it teaches that they are passive throughout this process. But it seems to me far more important to stress that the organisms find, invent and reorganize new environments in the course of their search for a better world. They build nests, dams, little hills and mountains. But their most momentous creation has probably been the transformation of the atmosphere surrounding the earth by enriching it with oxygen . . .

Darwinians are as aware as anyone else that organisms are anything but passive. Darwin wrote an entire book about how earthworms transform soil. However, Popper is right that organisms as active agents tend to get lost in much of the literature in population genetics with its heavy emphasis on genes and characters. This tendency is so marked that one of the most original and insightful population geneticists has been lead repeatedly to complain of it (Lewontin 1978, 1983). However, once again, I would like to note that Popper's concern was engendered by his interest in learning and conceptual change, especially as it pertains to science. Organisms are anything but passive when it comes to the learning process. Certainly scientists are far from passive.

Popper's second suggestion for improving evolutionary theory (improvement B) is more problematic. It has to do with what is termed the Baldwin Effect. At the end of the 19th century, Lamarckian inheritance was still

a reputable position. Baldwin (1896) introduced a mechanism to explain apparent Lamarckian inheritance. According to Baldwin, the phenotype of an organism is somewhat plastic. One and the same organism can develop differently, within limits, depending on the environment that it confronts – a capability that we now term its reaction norm (see Schlichting and Pigliucci 1998). Thus, if a particular environmental variable recurs through several generations that keeps a succession of organisms at one extreme of their reaction norms, time is bought so that, with luck, the genotype can catch up. Thereafter, the phenotypic reaction would be nearer the center of its reaction norm rather than at its periphery. If Baldwin had let the matter rest here, the response of later Darwinians might have been positive. However, he went on to insist that his mechanism was a "new factor" in evolution over and above natural selection. Later Darwinians could not see the novelty (Mayr 1963, p. 610).

In line with Baldwin, Popper (1974b, p. 138) proposes to enrich Darwinism by distinguishing between two sorts of selection – selection that is internal to the organism and selection that is external to it. Internal selection comes ultimately from an organism's preferences or aims. Popper's hypothesis is that "organisms, under external selection pressures, have developed genes, especially b-genes, which allow the organism a certain variability." As a result, organisms are capable of some variation in their behavioral reactions to their environments:

> We can now say that certain environmental changes may lead to new preferences or aims . . . The new preferences or aims may at first appear in the form of new tentative behavior (permitted but not fixed by *b*-genes). In this way the animal may tentatively adjust itself to the new situation without genetic change. But this *purely behavioural* and tentative change, if successful, will amount to the adoption, or discovery, of a new ecological niche. Thus, it will favour individuals whose *genetic p*-structure . . . more or less anticipates or fixes the new behavioural pattern of preferences. This step will prove decisive; for now those changes in the skill structure (*s*-structure) will be favoured which conform to the new preferences . . .
>
> I now suggest that *only after the s-structure has been changed will certain changes in the a-structure be favoured; that is, those changes in the anatomical structures which favour the new skills* (Popper 1974b, p. 139).

I do not know how to respond to Popper's postulation of various sorts of genes, and I am not alone in this quandary. I have not been able to find any biologist who has taken up Popper's suggestion. On one interpretation, all it amounts to is that "unprogrammed" behavior can precede genetic

changes that can come eventually to make it in a sense "programmed." Nor is this hypothesis especially novel. For example, Ospovat (1981, p. 51) has shown that Darwin in his second notebook held that "frequent repetition of habitual and instinctive behavior, in response to altered environmental conditions, causes changes in structure", or as Darwin put it, "instinct goes before structure." And finally, I must note that even though the Baldwin effect is supposed to apply to all characters, once again Popper is interested primarily in behavior (for further discussion, see Continenza 1986).

The popular response

So much for Popper's criticisms and suggested improvements of evolutionary theory. I now turn to the use of Popper as an authority in squabbles both within science and about science. Although Popper himself officially rejected the power of authority, he had it foisted on him, like it or not. Time again, scientists stated that Popper spoke to them in ways that no other philosopher did. As Sir Peter Medawar remarked, "Popper is incomparably the greatest philosopher of science that has ever been", and Hermann Bondi seconded Peter Medawar's high opinion: "There is no more to science than its method, and there is no more to its method than Popper has said" (see also Bondi 1992, 1994). And finally, John Eccles advises scientists "to read and meditate upon Popper's writings on the philosophy of science and to adopt them as the basis of operation of one's scientific career" (Halstead 1980a, p. 215). Is it any wonder that other philosophers of science got their backs up?

The most vociferous and protracted controversy that has ever graced the pages of *Nature* concerned the British Museum (Natural History), cladism, Marxism, Creationism, and the nature of evolutionary theory. It started innocently enough. A study had shown that visitors to the Natural History Museum were coming away having learned very little. As a result, the Public Services Department was commissioned to rework the exhibits. One problem at the start is that natural history museums have traditionally attempted to fulfill two functions: as locations for natural historians to do their work and as places of entertainment and edification for the general public. The two functions can easily come into conflict. Second, the British are not all that open to "modernization", especially when it comes to their national treasures. Any changes in the exhibits of this Victorian pile of terra cotta would have caused a ruckus.

To this mix was added a third element – the proper way to classify organisms. For most of its history, systematics was characterized by largely intuitive methods of classification, but with the rise of the New Systematics, systematists attempted to codify their practices. The first consensus was that

grades had to be balanced with clades; that is, degrees of similarity had to be combined somehow in classifications with order of speciation. However, in the 1970s a new school of systematics arose that advocated the strictly cladistic methods of Willi Hennig (1966). Not only were the exhibits under construction *new*, but also they were ordered along the *principles of cladistic analysis*. One important feature of cladistics is that cladistic methods cannot identify ancestral species as ancestral. Certainly common ancestors must have existed, but cladistic methods cannot tell ancestors from their descendants. According to the cladists, all groups have to be treated as collateral sister groups.

A paleontologist, L. B. Halstead, who sensed "sinister" doings at the Natural History Museum, sounded a warning call. For example, statements in the booklet that accompanied the new exhibits made it sound as if human beings had no ancestors. What would Creationists say to this! In addition, Halstead noted that opposition to the gradual character of evolution evinced by some cladists supported the Marxists view that all change is ultimately saltational; i.e., it occurs in discrete leaps. Henceforth, "Marxists will be able to call upon the scientific laws of history in its support" (Halstead 1980b, p. 208).

In general, Halstead's claims about Marxist motivations did not go down very well. One prominent paleontologist at the museum, Colin Patterson (1980, p. 430), objected that of the three original advocates of punctuational speciation, only one was a Marxist (Stephen Jay Gould), only one was a cladist (Niles Eldredge), and one was neither (Stephen Stanley). More importantly, "Cladistics is not about evolution, but about the pattern of character distribution in organisms." Patterson also objected to Halstead's seeming to argue from authority. Just because Mayr and Simpson are gradists, it does not follow that he cannot adopt a different position. And in the very next letter, Hughes-Games (1980, p. 430) invoked the authority of Popper. The emptiness of claims about Marxist laws of history have been shown by "Sir Karl Popper in *The Poverty of Historicism* and *The Open Society and its Enemies*."

Halstead (1981, p. 106) replied that his early claims concerned traditional cladists, not the newly emerging transformed cladists who were motivated by epistemic purity. Because transformed cladists had such high standards of knowledge, history was "unknowable." Hence, any attempt to reflect ancestor-descendant relations in classifications was illicit. In this connection, Malcom McKenna (1981, p. 627), a paleontologist at the American Museum of Natural History in New York City, congratulated the British Museum for "bringing epistemology into its exhibits and teaching visitors that science is a method, not a body of revealed knowledge." The virtue of cladistic analysis is that it "parsimoniously estimates relatedness and is therefore testable."

The editors of *Nature* objected to the standards of knowledge being employed in this argument. One sentence in the brochure accompanying the new exhibits began, "If the theory of evolution is true . . . " The editors responded, "But is the theory of evolution still an open question among serious biologists? And, if not, what purpose except general confusion can be served by these weasel words?" (*Nature* 1981, p. 735).[8]

When two-dozen biologists from the Natural History Museum (1981, p. 82) blasted the view of science implicit in the preceding editorial, the editors of *Nature* responded at some length, relying almost entirely on the views of Popper. As might be expected, a professional philosopher of science wrote to complain of the use that was being made of Popper's philosophy of science. Not only was it being misused, but also Sir Karl had hardly uttered the final word in philosophy (Caplan 1981, pp. 623–624). Even Creationists got their say, complaining about how close-minded so many atheists are (Darnbrough, Goddard and Stevely 1981).

Finally, the editors of *Nature* brought the controversy to a close, announcing that they would not publish any more letters on these topics after July 1991. Appropriately, they concluded with a letter from the man who had started all the hubbub in the first place – Beverly Halstead. In his contribution, Halstead (1981, p. 404) included long quotations from Popper in which Popper clarified his attitude toward both the evolutionary process and phylogenetic reconstructions. A controversy over these same issues had been running apace in the *New Scientist*.[9] It was in this magazine that Popper (1981, p. 611) decided to explain his position on evolution and falsifiability:

> . . . some people think that I have denied scientific character to the historical sciences, such as palaeontology, or the history of evolution of life on Earth; or to say, the history of literature, or of technology, or of science.
>
> This is a mistake, and I here wish to affirm that these and other historical sciences have in my opinion scientific character: their hypotheses can in many cases can be tested.
>
> It appears as if some people would think that the historical sciences are untestable because they describe unique events. However, the description of unique events can very often be tested by deriving from them testable predictions or retrodictions.

Popper is very careful in the preceding quotation to defend historical reconstruction. He says nothing about the evolutionary process. And he chooses his terms carefully. Testable predictions or retrodictions can be derived from unique descriptions. He says nothing about falsifiability. Popper's care is appropriate. Popper introduced his criterion of falsifiability as an antidote to the positivist criterion of verifiability. With respect to statements that are universal in form, the contrast is striking. No statement of unres-

tricted universality can be verified because it applies to indefinitely many instances. However, in principle, a single counter-instance to a universal statement can falsify it. (As it turns out, universal claims are not all that easy to falsify once one acknowledges, as Popper does, that such statements do not exist in isolation but in more inclusive complexes.) Singular statements in a sense are in a stronger position than are universal statements because they can be shown to true or false with equal ease, not to mention difficulty.

The problem with singular statements is they refer to events that occur at a particular time, and because the specious present is so fleeting, most singular statements refer to events that occurred in the past. The question then becomes how certain we can be of past events. Queen Elizabeth II is a woman. The truth-status of the preceding singular statement is about as certain as we can hope to get. She certainly looks like a woman. Everyone in a position to know claims that she is a woman. It was entered on her birth certificate. She had several children. What more can anyone want? But Queen Elizabeth I was also a woman. How certain can we be of this singular statement? Disagreements on the answer to questions such as these are what motivated the argument among cladists (especially transformed cladists) and noncladists about phylogeny.

Scientists have a strong predilection for observations. Without observational data, science is impossible. But sometimes in their enthusiasm for observational data, scientists are led to argue that all scientific investigations must begin with observational data and nothing else. Some scientists even go so far as to claim that science should stop with data and never proceed past it to theoretical speculation. Transformed cladists are widely interpreted as holding at least some version of this inductivist position. They are not alone among scientists in their predilection. Lots of scientists can be found saying that scientists should stick with the facts and nothing but the facts.

One irony in the debate over cladism is that inductivists such as the transformed cladists should have picked Popper as one of their patron saints (Nelson and Platnick1981, p. ix). *Systematic Zoology* has to be the only journal in which all of Popper's major works in the philosophy of science have been reviewed (Platnick and Gaffney 1977, 1978a,b). If Popper is famous for anything in addition to his principle of falsifiability, it is for his thesis that all concepts are theory laden. There is no such thing as the facts and nothing but the facts. Even if there were, Popper (1972, p. 259) insists that scientists cannot and should not attempt to begin their investigations with observations:

> I cannot, of course, hope to convince you of the truth of my thesis that observation comes after experience or hypothesis. But I do hope that I

> have been able to show you that there may exist an alternative to the venerable doctrine that knowledge, and especially scientific knowledge, always starts from observation.

In the preceding quotation, Popper can be found agreeing with many scientists that scientific practice must *begin* at a particular place. The only difference is that this place is experience and hypothesis rather than observation. On my view, there is no one place that scientific investigations must begin. Scientists begin wherever they happen to be.

Although evolutionary theory as a process theory is difficult to falsify, it is still *falsifiable* in Popper's sense, but hypotheses about past unique events cannot be *falsified* in the way that general laws can. They are, nevertheless, *testable*. In this connection, cladists argued that their classifications were genuinely scientific because they were "falsifiable" in Popper's sense. However, according to Popper, statements about the evolution of life on Earth are *singular*, not *general* in form. Hence, strictly speaking, "falsifiability" does not apply to phylogenetic reconstructions at all (Hull 1983), and we have returned by a very circuitous route to the topic with which this paper began. Can protracted controversies in science turn on issues as transparent as these? Unfortunately, they can (for a more recent discussion of the influence of Popper on cladistics, see Faith and Cranston 1992).

Conclusion

Karl Popper developed a philosophy of science that has seemed to many scientists to reflect the nature of science. Scientists have used Popper's authority in their internecine battles. So have Creationists in their battles with scientists. In both cases, scientists and Creationists alike have not understood Popper all that thoroughly, and those who did were not all that successful in setting things straight. Although Creationism at the time was not the major force in the British Isles that it was in the former colonies, Popper still might have been at least dimly aware that terming evolutionary theory a "metaphysical" research program might aid Creationists in getting Bible stories taught in biology classes. He was aware of a very large literature in which "metaphysical" is a bad word.

Popper had no way of anticipating the use that cladists would make of his work, but when he did become aware of it, he did little to rectify these misunderstandings – and they lay at the center of his own professional writings. In response to the editorial in *Nature* entitled "Darwin's Death in South Kensington", almost two dozen working biologists at the British Museum (Natural History) complained:

> How is it that a journal such as yours that is devoted to science and its practice can advocate that theory be presented as fact? This is the stuff of prejudice, not science, and as scientists our basic concern is to keep an open mind on the unknowable. Surely it should not be otherwise?
>
> You suggest that most of us would rather lose our right hands than begin a sentence with the phrase "If the theory of evolution is true . . . " Are we to take it that evolution is a fact, proven to the limits of scientific rigour? If that is the inference then we must disagree most strongly. We have no absolute proof of the theory of evolution. What we do have is overwhelming circumstantial evidence in favour of it . . . .

Do scientists really think that they are striving for absolute proof? Can they demand it in the work of other scientists? If scientists should have gotten any message from Popper's writings, it is that absolute proof in science is impossible. But Popper's terminology can be misleading. The statement that Elizabeth I is a woman is, technically speaking, *unfalsifiable*. So is the statement that the ostracoderms evolved from the placoderms. But both are *testable*. Popper might have explained to the biologists concerned how these two terms differed in his technical vocabulary. Professional philosophy of science rarely plays a significant role in scientific controversies. In the case of cladism, Creationism and evolutionary theory, it did – to the detriment of all concerned.

Notes

[1] Lindahl and Welljams (1992, p. 204) present summary data from the *Science Citation Index* and the *Social Sciences Citation Index* showing that the personal, anecdotal estimations of Popper (1982), Bartley (1982) and Skoyles (1992) about Popper's influence are mistaken. In fact, Popper's annual average citation rate in the philosophy journals included in the *SSCI* is 39.6. Of course, if one were actually to read these citations in philosophy journals, one is likely to find that most are negative, but that is the nature of philosophy. We spend most of our time criticizing the work of others, Popper included. Skoyles (1992, p. 100) even makes the amazing claim that there is "no school of Popper: the only popperians are scientists, like Bondi, not philosophers or those studying society." For further discussion of the influence of Popper, see Mulkay and Gilbert (1981).

[2] Popper (1974b, p. 135) thinks that the origin of life was extremely improbably, so improbable that nothing can explain why it originated. If the time that it takes for something to develop is any indication of how improbable it is, then the transition from life to nonlife is not all that improbable because the first signs of life occur quite early in the fossil record. If we take the universe to be about 17 billion years old, the earth was formed about 4.5 billion years ago. Within another billion years, the first anaerobic bacteria evolved. It took at least two billion years more for multicellular organisms and sexual reproduction to occur. Given current evolutionary theory, meiosis is a good deal more improbable than life itself. Popper (1984, p. viii) also holds the biblical (and mistaken) view that plants evolved before animals.

Certainly blue-green algae evolved quite early, but nothing that might be termed a "plant" evolved until long after animals were firmly established.

[3] Popper tirelessly reiterates that he was not and had never been a positivist. In fact, in all due modesty, he was forced to admit that he was the one who did in positivism (Popper 1974a, p. 69, 1984, pp. 6, 89, 90, 176).

[4] Before the reader jumps to the conclusion the punctuational views of Eldredge and Gould (1972) refute the Darwinian theory of evolution, some care needs to be exercised. The chief differences between the two views is that Eldredge and Gould think that most change occurs over a relatively short period of time (possibly twenty of thirty generations) and that species do not change much thereafter. In addition, some non-selective mechanisms might influence these periods of rapid change significantly. How "Darwinian" this alternative is continues to be under negotiation. I do not think that facts are "negotiated" in the way that some social constructivists claim that they are, but terminology certainly is negotiated in a literal sense of this term, and intellectual justice is not the only concern in these negotiations. To some extent the ease with which Darwinians coopt positions that were presented to refute them looks suspicious. For example, early on Darwinians opposed Kimura's neutralist views , but as the data piled up indicating that most mutations are neutral and not negative, Darwinians simply modified their theory and termed the result "Darwinism" (Stebbins and Ayala 1981).

[5] I have my own problems with fitness. I do not think that a single measure of fitness, no matter how defined or operationalized, can be very significant because too many factors, many of them conflicting, contribute to an organism's fitness. The same objection is commonly raised with respect to IQ. One difference between fitness and IQ is that fitness functions in a much more powerful theoretical context than does IQ. Even so, the same objections apply to both concepts.

[6] In this paper I do not address Popper's solution to the mind-body problem.

[7] Popper (1972, p. 263) notes other disanalogies between biological and cultural evolution. For example, biological evolutionary trees are constantly diverging as evolution proceeds through time, while the tree of knowledge is characterized by lots of merger. "In other words, the evolutionary structure of the growth of pure knowledge is almost the opposite of that of the evolutionary tree of living organisms, or of human implements, or of applied knowledge." The difference which Popper points out is not as great as he thinks. Lots of merger occurs in biological evolution. It is quite common in plants. Plants that are closely related frequently produce hybrids that become established as separate species. But merger can occur across great distances in the phylogenetic tree as viruses and bacteria become incorporated as organelles in other organisms. Chlorophyll probably started out as a parasite. In addition, at least some of the merger that we see in the tree of pure knowledge results from our failure to distinguish conceptual homologies from conceptual homoplasies. If this distinction is ignored in biological evolution, merger becomes commonplace.

[8] The film loop that accompanied the exhibits contained even more misleading claims:

> The Survival of the Fittest is an empty phrase; it is a play on words. For this reason, many critics feel that not only is the idea of evolution unscientific, but the idea of natural selection also. There's no point in asking whether or not we should believe in the idea of natural selection, because it is the inevitable logical consequence of a set of premises .... The idea of evolution by natural selection is a matter of logic, not science, and it follows that the concept of evolution by natural selection is not, strictly speaking, scientific ... If we accept that evolution *has* taken place, though obviously we must keep an open mind on it .... We can't prove that the idea is true, only that it has not been proved false (Cox 1981, p. 373).

Miles (1981, p. 530) responded that his Department of Public Services was preparing a new version of the film loop because this one gave an impression other than that intended. If this controversy at the British Museum (Natural History) sounds anything like the Enola Gay fiasco at the Air and Space Museum in Washington, D.C., it should.

[9] The controversy in the *New Scientist* began again with a paper by Halstead (1980a), followed by lots of letters to the editor, additional papers by Little (1980), Sparkes (1981), Ruse (1981), Ridley (1981), etc. Ruse (1978) in a review of a book edited by Rom Harré (1975) discusses many of the same issues about the role of Popper's philosophy in science that I do in this paper. Still another sequence of papers and letters to the editor appeared in *Science* (211, pp. 35–36, 1331–1332; 212, pp. 281–283, 737–738, 873–875, 1446–1449).

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