



*History and  
Philosophy of Science:  
a Phylogenetic  
approach*

**História e filosofia  
da ciência: uma  
abordagem  
filogenética**

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*In the aftermath of Thomas Kuhn's The structure of scientific revolutions, there was a great deal of discussion about the relationship between the History of Science and the Philosophy of Science. A wider issue was at stake in these discussions: 'normativism' versus 'naturalism' in Epistemology. If the History of Science, at best, gives us reliable information about what actually occurred historically, how can it inform debates about such things as confirmation or explanation in Philosophy of Science?*

*This essay makes a case for the centrality of historical investigation in the Philosophy of Science. I will defend what I term the 'Phylogenetic' approach to the Philosophy of Science. I will argue that since the foundations and dominant methods of a particular scientific field are shaped by its history, studying that History can give us considerable insight into conceptual and methodological problems in a particular Science. The case will be made both on general, philosophical grounds, and by compelling instantiation.*

*KEYWORDS: History and Philosophy of Science; Phylogenetic, normativism, naturalism.*

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A publicação de *A estrutura das revoluções científicas* de Thomas Kuhn resultou em uma grande discussão sobre a relação entre a história da ciência e a filosofia da ciência. Nessa discussão, o que estava em jogo era algo bem mais abrangente, isto é, o 'normativismo' versus o 'naturalismo' em epistemologia. Se a história da ciência, na melhor das hipóteses, nos dá informações confiáveis quanto ao que realmente ocorreu historicamente, como é que ela pode auxiliar os debates da filosofia da ciência sobre aspectos tais como confirmação e explicação? O presente artigo defende a centralização da investigação histórica para a filosofia da ciência. O autor defende o que ele chama de abordagem 'filogenética' à filosofia da ciência, argumentando que, uma vez que as bases e métodos que prevalecem em uma área científica são moldados pela sua história, estudar esta história pode esclarecer consideravelmente os problemas conceituais e metodológicos de uma determinada ciência. A argumentação se faz em bases filosóficas gerais e através de exemplificações determinantes.

**PALAVRAS-CHAVE:** história e filosofia da ciência; filogenética, normativismo, naturalismo.

## Introduction

About twenty-five years ago, in the aftermath of Thomas Kuhn's *The Structure of Scientific Revolutions*, there was a flurry of articles and book chapters dealing with the vexed question of the relationship between the history of science and the philosophy of science. (Laudan, 1977; Burian, 1977, pp. 1-42; Giere, 1973, pp. 282-97; Lakatos, 1971; Toulmin, 1972). The wider issue at stake was 'normativism' versus 'naturalism' in epistemology. If the history of science, at best, gives us reliable information about what actually occurred during the development of the sciences, how is it to support the inevitably normative conclusions of the philosophy of science? On the other hand, historians must make prior judgments about what counts as science in order to delimit their subject, and philosophers have to use similar standards in deciding what counts as the 'historical data base' for discussions of scientific laws, theories, explanations, confirmation, and so on.

Kuhn closed the Introduction to *The Structure of Scientific Revolutions* with what was clearly intended as a rhetorical question, one which presents the problem in stark terms:

How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply? (Kuhn, 1970, p.9)

One response to this problem — Giere's, in fact (1988) — was to bite the naturalistic bullet: philosophy of science (or better, 'science studies') is just one more empirical inquiry into a human activity, drawing its standards from a careful examination of the historical record or of present day science, including its standards for what counts as science. Whatever people at different times took to be science is considered to be science. The philosopher is not in a position to legislate such matters. Another response — also Giere's — was the cynic's: the connection between the history and philosophy of science is a 'marriage of convenience'. When historians threw the 'internalist' historians of science out of the history departments, they needed a home. Tom Kuhn created one for them, in philosophy departments.

You will notice that, despite their different responses to the problem, Giere and Kuhn see the problem, as do many others, in the same way. The history of science is a sort of 'inductive data base' to be used as confirmation for various philosophical views about science. This is a picture of the relationship between history and philosophy of science I completely reject. My primary goal in this essay is not to argue against this picture, however, but to present an alternative view of the relationship between history of science and philosophy of science. After many years of doing the history and philosophy of biology in a certain way, I spent some time reflecting on what it was I was doing. It was decidedly *not*

the enterprise of trying to support philosophical conclusions with historical facts. It was rather the activity of understanding foundational problems in biology through a study of the historical origins and development of those problems. I will label this approach the ‘phylogenetic’ approach to the philosophy of science. This label is intended to highlight the analogy between this approach to the philosophy of science and the way in which evolutionary biologists use phylogenetic reconstruction to understand current organisms. I will begin with a somewhat crude and impressionistic outline of what I have in mind by that phrase, turn to a detailed example, and close with a less crude and more realist account.

### The phylogeny of foundational problems

To begin with, we need an account of what sorts of things a philosopher of science *can* do. Here I am an unabashed ‘technicalist’. The philosopher of science focuses a particular kind of training and expertise on puzzles, paradoxes and confusions in the foundations of science generally, and of special sciences specifically. Whether it is puzzles about quantum non-locality, singularities in relativity theory, group selection and fitness in evolutionary theory or information theory in thermodynamics, there is a place for people trained to look for the hidden presuppositions of different approaches, or for their logical virtues and flaws, or to draw out imaginatively the consequences of different ways of conceptualizing or formulating a theory or problem. Similarly, there may be unusual or problematic approaches to testing, confirming and rejecting hypotheses in the sciences that may benefit from philosophical scrutiny. But for such scrutiny to be of value, philosophers must know what those problems are, and know them in the form they take in the actual sciences.

But so conceptualized, why would philosophy of science have any need for the history of science?<sup>1</sup> The answer lies, I believe, in the fact that the foundations of a particular scientific field are shaped by its history, and to a much greater degree than many of the practitioners of a science realize. There is more conceptual freedom in the way theories—even richly confirmed theories—may be formulated and revised than is usually realized. Studying the way they *actually* came to be formulated and revised historically can be of considerable philosophical value. My primary argument for this claim will be by means of compelling instantiation, but the idea can be given initial plausibility by considering two well-known episodes in biology’s history.

1. It is well known that Charles Darwin was forced to present a cobbled-together sketch of his theory to the Linnaean Society in 1858 because he had received a paper by mail from Alfred Russel Wallace which, the myth goes, presented ‘the same theory’ he had come up with 20 years earlier.<sup>2</sup> This is a twofold myth: first, because their theories

<sup>1</sup> The very question that Giere began with in his 1973 article, in fact.

<sup>2</sup> A fine survey of the actual history of the interactions between Darwin and Wallace before and after 1858 is available in Kottler (1985, pp. 327-366). However, even Kottler underestimates the differences in the theories presented side by side in 1858.

are, in fact, quite different; and second, because Darwin's theory circa 1858 was significantly different from his theory circa 1838. Focusing only on the first myth: Wallace's theory has no place for the concept of natural selection; in fact, unlike Darwin, Wallace thinks that what occurs in domestic breeding would lead you to deny evolution and is at great pains to indicate that natural populations are very different. Indeed, he and a number of other of Darwin's supporters regularly urged Darwin to cast the notion of natural selection, with its built in analogy to domestic selection aside.

Furthermore, Wallace's theory lacks a mechanism of speciation, Darwin's central concern. The presence within a species of a 'typical form' and occasional 'varieties' is taken for granted. The struggle for existence leads to the gradual replacement of the typical form by the variety. The following passage will give you the feel for Wallace's approach:

The variety would now have replaced the species, of which it would be a more perfectly developed and more highly organized form. It would be in all respects better adapted to secure its safety, and to prolong its individual existence and that of the race. Such a variety could not return to the original form; for that form is an inferior one, and could never compete with it for existence.<sup>3</sup>

<sup>3</sup> The two 1858 papers, along with the introductory remarks of Charles Lyell and Joseph Dalton Hooker, can be seen in Barrett (1977, 3-19). The quote can be found on p. 15.

It is not hard to imagine a scenario whereby Wallace's version of evolutionary theory prevails. Had that happened, the conceptual and logical foundations of evolutionary biology would have gotten off to a different start and moved along a different conceptual trajectory.

2. It is likewise well-known that Gregor Mendel published the lectures presenting his experiments with hybrid pea-plants in a new Society Proceedings (1865, pp. 3-47) that was then fairly widely distributed around Europe; and that his work received very little notice, being occasionally reported without fanfare in surveys of hybridization research, but otherwise ignored. In 1900s, the story goes, three researchers — C. Correns, H. De Vries (both in Stern and Sherwood 1966, pp. 107-132), and E. Tschermak (1950, pp. 42-7) — published the results of their research with hybrids from different genera, each of them noting the same F2 3:1 (or 1:2:1) ratios and all citing Mendel's earlier work.

But again the myth covers up the different theoretical and methodological approaches of these 're-discoverers', as well as their differences with Mendel—different experimental methods, different mathematical techniques, different theoretical pre-suppositions and conclusions drawn. In this case the history is remarkably complicated, and the methodological, mathematical, and conceptual foundations of modern genetics owes a great deal to its radically contingent history.

This by itself does not make the history of science of *central* importance to the philosophy of science. However, among the 'historical

contingencies' built into the theories, methods, and explanatory techniques of a science there are those that lead to theoretical and methodological problems for that science. Under such circumstances, a careful examination of that science's historical trajectory is crucial in providing various sorts of help toward philosophical understanding. Let me say a bit more about two sorts of help historical investigation can provide.

### **Alternatives**

A reasonably mature science is the result of a number of decisions made, at various historical nodes, among a variety of possible options that ought to be taken.

As one traces back through the history of a current theory, one finds various alternatives. This historical research opens up a space of theoretical possibilities that were earlier rejected, or not considered, but in the light of current problems, may seem interesting. Stephen J. Gould often mines the history of science in search of alternatives to neo-Darwinism, for example. His claims about 'the hardening of the Neodarwinian synthesis' are claims that a variety of theoretical options available for exploration in the early work of people like Sewall Wright and George G. Simpson were simply not pursued. Why weren't they? Should they have been? Would those options help us with some of the foundational problems in evolutionary biology today? These are historical questions with philosophical pay-offs.

### **Locating the source of the problem**

Certain problems in the philosophy of biology, as I will demonstrate, have a historical origin. Go back to, say 1875, and evolutionary theory lacked various problems it now faces—and had many it now lacks! By moving forward in time, it may be possible to focus on the theoretical developments that set the scene for the problems that now concern us. Prior to doing such historical work, the problem may seem intractable, hard to understand, even paradoxical. Seeing the problem gradually emerge and become explicitly recognized *as* a problem helps theoreticians and philosophers, I believe, to understand it.

I have called this a phylogenetic approach to the philosophy of science. I would now like to exploit the evolutionary metaphor from which that name comes to further explicate the basic idea here.

Some of the most compelling evidence of the evolutionary history of an organism comes from features it possesses that are [i] widely shared with organisms in very different environments and/or [ii] of little or no adaptive value in that organism's current environment. We easily think of 'vestigial' structures such as the subcutaneous 'eyes' of moles or hind limb skeletal remnants of cetaceans in this way. But organisms are

mostly their deep history, rather than their recent adaptations. There are no doubt endless numbers of better designed skeletons for upright locomotion than ours — ours reflects an endless variety of changing conditions imposed on us throughout our evolutionary history. The evolutionary trajectory of a particular contemporary species is inscribed in its genotype and displayed in its phenotype. Natural selection tinkers with what history provides, but it seldom does more. Local, highly contingent adaptations get built in to the genetic heritage of a species, and further adaptation is a modification of that heritage.

So with our current scientific theories, the modifications and revisions they constantly undergo are highly constrained by their historical baggage. But that history was not aiming toward the current version of the theory (thus this historiographic approach avoids the label, ‘Whig’), and it is not linear. Like evolutionary phylogenies, in the branching network of science, there are likely to have been a variety of developments going off in different directions from any particular node, many of which became dead ends, some of which did not, and perhaps one of which comes to be the ‘dominant’ or ‘received’ theoretical approach (as with the Neo-darwinian synthesis in the 1940s and 50s).

What constraints operate in such way that certain branches develop and others do not? First and foremost are the empirical constraints. A significant part of what makes one revised version of a theory last and come to dominate is its superior ability to resolve empirical anomalies, to suggest novel tests - ideally, tests that force choices among competitors - to account for evidence formerly not thought of as evidence for that theory at all, and so on. I would argue that, while this is not the only environmental factor shaping theory construction and revision, it is the most important one in the historical sequences I have studied.

Having said that, it would be hard to find an episode in the History of Science in which some features, even some important features, of the theory were not adopted for reasons other than judgments of empirical adequacy. As I will argue shortly, the so-called ‘tautology’ problem in Darwinian selection theory, which philosophers have played a central role in helping resolve, emerged as a consequence of a series of fundamental conceptual and methodological changes in the theory of evolution by natural selection. It is arguable that none of those changes was mandated by empirical considerations. I don’t want to claim that empirical considerations played no role — only that, whatever role they played, it was rather less determinative of the historical trajectory of the theory.

### **A case in point: fitness, adaptation and explanation**

One of the problematic features of the phylogenetic approach to Philosophy of Science is that it starts with a philosophical problem

at a certain point in a theory's development and looks back to history with this problem in mind. From a historian's point of view, nothing could be more suspect. I have denied that the method I am advocating is 'Whiggish', but can one use such an approach and truly avoid that label? Using a detailed case study, I hope to show that one can.

The problem I shall focus on is one that has been badly misunderstood, by Karl Popper and others, misunderstandings exploited by 'scientific creationists' and their fellow travelers. But it is a real problem, and we need to begin by formulating it.

In contemporary population genetics, the 'mechanics' of evolutionary theory as Richard Lewontin has called it, the concepts of 'mean fitness' and 'selection coefficient' play a key role. Both claim to be represented by mathematical variables in the mathematical models of the theory. Applying the models - i.e. solving the equations - requires supplying values for these variables based on different possible gene combinations at a given locus. Those values are derived from statistical samplings of populations over a number of generations.

The wrongly labeled 'tautology' problem arises from the fact that these relative fitness values are apparently determined by sampling actual populations to determine the actual relative reproductive rates of the different phenotypes. Judgments of relative fitness are based on the actual relative increases and decreases in the frequencies of the allelic combinations under consideration. But it is these changes in relative frequencies that the models are supposed to explain. And they can only do this if the fitness of a genotype represents something about it that explains these changes in its relative frequency. If it does not, then these models are explanatorily sterile.

Now there is a quick answer to this problem that unfortunately does not work. One simply says that the way the theory works, one 'predicts' certain changes in relative frequencies based on 'engineering', 'optimal design' or 'life history' considerations, and then 'tests' it by doing population studies — for the moment whether tightly controlled laboratory studies or random sampling of wild populations is not at issue (Burian, 1985, pp. 287-314; Mills and Beatty, 1979, pp. 263-286; Brandon, 1990). There is a more and a less fundamental problem with this quick answer. The less fundamental problem is that it is completely unclear how one uses this sort of analysis to derive specific, quantifiable fitness values. If one is simply using guess work, this approach quickly degenerates into the aforementioned sterility - in practice one just keeps adjusting the values until they come within tolerable limits of the values actually found.

The more fundamental problem is that we no longer have a single theory, but a potentially infinite number of *ad hoc* models. After all, the gene combinations that make a horseshoe crab, a scarab beetle, a Caribbean guppy and an African bonobo well-

adapted to their environments are utterly different, but the fitness value of some allele relevant to their adaptability may be exactly the same.

A variety of solutions to this worry have been proposed, and it is not (fortunately) my task today to adjudicate between them. Some have suggested that fitness be conceived as a “reproductive propensity”, which given that the mathematical notion of fitness is clearly probabilistic, makes sense.<sup>4</sup> Others have suggested that it is a property that supervenes on an endless variety of different adaptive arrays (Rosenberg, 1978, pp. 368-86). Still others have suggested that it be considered as an “uninterpreted term of the theory”, which takes on empirical content only in its explanatory applications (Rosenberg, 1993, pp. 118-28). Finally Lindley Darden and Joe Cain (1989, pp. 106-29), and independently Bradley Wilson and I (1994, pp. 65-80), have suggested that it be viewed as a middle-range abstraction.

<sup>4</sup> See note 2.

What I want to outline today is the way in which studying the history of this subject provides one with a space of philosophical alternatives to the theoretical approach that generates the problem and with a deeper understanding of it.

If we return to Charles Darwin’s Darwinism, we can see that the theory of evolution by natural selection is free of this problem, but for suspect reasons. Darwin made no attempt at all to investigate populations empirically to see whether the mechanisms described in the first four chapters of *On the origin of species* actually produce differential changes in the frequencies of ‘small heritable variations’, as he claims they will (Lennox, 1991, pp. 223-46). It is unclear why he does not do this, but two reasons are suggested by other aspects of his theoretical perspective.

Darwin seemed to think that selection-driven evolution moves with unimaginable slowness in nature - he may thus have assumed that direct evidence would never be available. It is sometimes (wrongly) claimed that he thought that evidence of domestic or artificial selection was sufficient to support his theory. It is clear from the following remark, *concluding* the chapters that presented his theory, that he did not think that.

Whether natural selection has really thus acted in nature, in modifying and adapting the various forms of life to their several conditions and stations, must be judged of by the general tenour and balance of evidence given in the following chapters (Darwin, 1859, p.127).

Another possible reason for his not attempting to investigate selection in natural populations is suggested by his theory’s most obvious shortcoming, its lack of an account of the origins of variation and of the

mechanisms of inheritance. Without a method for disentangling the heritable and non-heritable components of variation in a population, such an investigation would have been futile — and Darwin was acutely aware that he had no method for doing this.

At any rate, Darwin and his followers were well aware that the theory was untestable without a well-verified mechanism of inheritance, since it was by the differential passing on of traits from one generation to the next that evolutionary change was alleged, on his theory, to take place. One central project for biology between 1880 and 1920 was to nail down such a mechanism, and to figure out how to apply information learned in highly controlled experimental settings (such as hybrid breeding experiments) to natural, uncontrolled populations. The form of genetics that ‘caught on’ was Mendelian, which used simple statistics and probability theory to analyze the results of experiments involving hybrid crosses, self-fertilization of hybrids and ‘back-crosses’ of hybrids with pure lines, and to make inferences about the cellular mechanisms producing the resulting ratios of observed traits. This, combined with the increasing power of the light microscope to observe meiosis and gametogenesis at the cellular level — including the behavior of chromosomes — provided biology with a powerful theory of the mechanisms of inheritance which included a clear method of experimentally testing theories and a clear connection between the phenotypic ratios to be explained and the genetic mechanism used in the explanation.

<sup>5</sup> The founding document is an apparently modest attempt on the part of a mathematician to correct an error made by a biologist about the implications of Mendel’s laws for ‘mixed’ populations. Cf. G. H. Hardy (1908, pp. 49-50).

But how to apply this theory to nature? Well, as it turned out, a German Doctor named Weinberg and an English mathematician and cricket lover named Hardy provided a solution, which is incredibly simple. In response to a casual question put to him during a cricket match by the experimentalist R.C. Punnett, the mathematician G.H. Hardy pointed out that Mendel’s laws, derived from the crossing of pure lines followed by repeated self-fertilization of the resulting hybrids, could be generalized to apply to large randomly breeding populations.<sup>5</sup> Mendel’s insights into ‘the laws governing the distributions of characters in hybrids’ can be transformed into a formula representing the *ratio* of different allelic combinations (termed genotypes) in a population formed by the random mating of individuals with different forms of the same gene. If we represent the different forms of the gene at the same locus<sup>6</sup> (known as different alleles) by A and a respectively, that formula will look like this:

$$AA: 2Aa: aa$$

The frequencies of the different genotypes can then be represented as follows:

$$p^2+2pq+q^2=1$$

where p=the frequency of A, q=the frequency of a, and p+q=1.

<sup>6</sup> For simplicity, I assume a locus with two alleles.

The Hardy-Weinberg Law thus gives us a ‘base line’ with which we can compare actual changes in frequencies of alleles across generations of reproductive communities. Deviations from this base line indicate a disruption of this equilibrium of genotypic frequencies across generations. There are a number of factors that may lead to such disruptions: a variety of forms of alteration of the genetic material (mutation), the migration of new genes into the population (which will change the initial frequencies), random changes in frequencies arising from sampling error (known as genetic drift) and selection favoring one genotype over another. Assuming other disruptive forces have been corrected for or ruled out, population genetics builds into its models the notion that a change in the frequency of a particular genotype is a measure of its relative fitness. This was a fundamental assumption of the ‘genetical theory of natural selection’ developed by Ronald Fisher (1958) in a book by that name.

Again, I don’t have time to tell even the outlines of the story, but that theory was quickly and vigorously attacked in a brilliant review by an American theoretician who had been developing an entirely different approach to the same problem — different models, different mathematics, different assumptions about typical natural populations. His name was Sewall Wright. One can think of the relationship between these two brilliant thinkers in the following way: they were studying the same problem, they accepted the theory of the gene, they both saw the problem as a mathematical one, and yet they rejected each others’ basic assumptions.

<b>Fisher</b>	<b>Wright</b>
1. Virtually infinite populations	1. Small relatively isolated populations
2. Random mating	2. Sortative Mating
3. Differential changes of four primary factors	3. Differential changes due to a balance due to selection
4. Pan-adaptationism seldom adaptational	4. Differences between populations

Partly because of the intense criticism and rivalry between these two men, both were keenly aware of their assumptions.<sup>7</sup> Fisher had been trained as a mathematician and physicist — in a recent history of evolutionary biology, he is described as “importing into evolutionary biology models taken from statistical mechanics and thermodynamics” and as “track[ing] the trajectories of genes in the same probabilistic spirit in which Maxwell, Boltzmann, and Gibbs tracked arrays of gas molecules” (Depew and Weber, 1995, p. 244). But Fisher was well aware of the dangers. After noting “some remarkable resemblances” between his fundamental theorem of natural selection and the second law of thermodynamics, he notes five “profound differences” between them, the second of which is worth quoting.

<sup>7</sup> A fine discussion of their differences and rivalry can be found in W. Provine, (1986, chs. 7-9).

Fitness, although measured by a uniform method, is qualitatively different for every different organism, whereas entropy, like temperature, is taken to have the same meaning for all physical systems (Fisher, 1958, pp. 39-40).

Alas, whether or not Fisher was the last population geneticist to make this point, it was quickly forgotten. For example, it is not uncommon today to see the general theory of natural selection stated in the following way (Wilson, 1980, p. 14):

Most people are familiar with the basic theory of natural selection. Organisms vary in a heritable fashion; some variants leave more offspring than others; their characteristics, therefore, are represented at a greater frequency in the next generation.

In this description of the theory of natural selection, the only explanation offered for the greater frequency of certain characteristics in the off-spring population than in the parent population is that the parents with those characteristics leave more off-spring. But that is a completely trivial result and leaves environment/organism interactions (i.e. actual selection pressures) completely out of the equation.

Darwinian Fitness often receives a similar treatment. Take, for example, the following glossary entry for 'fitness' in a highly regarded primer in population genetics. Fitness: the reproductive contribution of an organism or genotype to the following generations (Ayala, 1982, p. 240)

As an account of how one measures fitness differences in a population, this is completely innocuous. But, as Ronald Fisher said, 'fitness although measured by a uniform method, is qualitatively different for every different organism...'. Even then, as Sewall Wright would point out, that is only a measure of fitness if one assumes all the other factors that can affect reproductive rates can be ignored.

There is clearly a conceptual muddle here. The concepts at the core of Darwinian selection theory have been 'operationalized', without the scientists who are doing so being aware of it. But if one goes back to the debate between Fisher and Wright, at the point at which this problem begins to emerge, one can begin to see what happened.

The story of the emergence of this problem is remarkably complicated. There is clearly a political and value component to it, which Diane Paul, in her brief note on the history of the concept of fitness in *Keywords in Evolutionary Biology*, nicely illustrates by the following quote from the widely used textbook *Principles of Genetics*, co-authored by Sinnott, Dunn and Dobzhansky (1958, pp. 100-1):

These [struggle for existence, survival of the fittest] emotionally loaded phrases have been often misused for political propaganda purposes. A less spectacular but more accurate statement is that carriers of different genotypes transmit their genes to the succeeding generations at different rates... The 'fittest' is

nothing more remarkable than the producer of the greatest number of children and grandchildren.

Professor Paul (1992, p. 114) trenchantly comments, “Thus was born the famous tautology problem”, which has bedeviled the field ever since.”

Yet, as we have seen, there is another very different dimension to this history, which is highlighted by Fisher’s comment about the dis-analogy between his principle of natural selection and the second law of thermodynamics, above. This dimension of the history tracks the introduction of mathematical model into the domain of evolutionary population dynamics.

Suppose statistical studies of populations of morning glories and giraffes result in the discovery that the suite of genes for a particular coat pattern in the giraffes and for a certain color pattern in the flowers have exactly the same mean fitness (say 65). What can this mean other than that they have the same relative rate of reproductive success? Neither their genotypes nor their phenotypes are the same. Their environments are entirely different, and the environmental variables relevant to differential rates of reproductive success between different genotypes are almost certainly going to be different. Yet if one is seeking a completely general mathematical model of fitness of the sort Fisher dreamed of, these differences must somehow be suppressed. Viewed from this vantage-point, it is hard to see how to avoid this problem.

There is no doubt that, whether your *bête noire* was *laissez-faire* capitalism or fascism, if you imagined that ‘survival of the fittest’ was popularly associated with either one, you would happily embrace a way of talking about fitness that de-coupled in from Darwin’s ‘struggle for existence’. But the de-coupling was, I think, also driven — perhaps primarily driven — by two quite different philosophical goals. The first of these goals was to be able to formulate this part of evolutionary biology in a recognizable mathematical formalism. The second related goal was to operationally define the key terms in the theory in a way that made it a completely general biological theory. Fisher, at least, was aware of the problems created by attempting to achieve this goal. Fitness is not, as the mathematical models make it appear to be, a single variable, different values of which belong to different genotypes.

There is yet another problem with the approach initiated by Fisher. As Sewall Wright carefully described and illustrated (the third point of difference between him and Fisher noted above), there are a variety of factors that can be operative within a population leading to long term increases and decreases in the frequencies of genotypes in populations. Since this effect can be produced by mechanisms other than natural selection, and also can be absent because of a balance between selection and other forces (or even countervailing selection forces), it is unwise to assume that such an observed effect is the result of natural selection, or that its

absence signals an absence of selection. Looking through John Endler's (1986) *Natural selection in the wild*, many of the methods he identifies as methods for detecting the operation of selection cannot distinguish among the various possible causes of differential reproductive rates.

The theory of natural selection thus has two distinct philosophical problems. Abstractness: Formulated as a quasi-mathematical theory, its variables seem not to represent any single varying property in nature. The traits of organisms and conditions of environments that concretely interact to bring about differential reproductive rates within populations seem impossible to represent in a single, abstract theory.

Differentiation: Even if we could solve the problem of abstractness and formulate a concept of fitness or adaptability that would unambiguously refer across all organic populations, there is a problem of how one differentiates changes in populations due to selection from the same changes produced by other mechanisms.

### **The phylogenetic analysis of a philosophical problem**

In the last section I provided a small bit of a much more complex history, not as an end in itself, but as an example of how a phylogenetic analysis of a current problem in the foundations of Evolutionary Biology can help clarify the problem for philosophical purposes. The method I have used is to trace back historically to a point where the problem does not exist, and then work forward historically until one can see it beginning to emerge. As in this case, it is often true that at that point, those involved in the scientific debate will be quite self-conscious of problems that a couple of generations later are submerged as unquestioned, unanalyzed presuppositions of the field's common set of concepts and methods. People see the problems, but cannot see what it is about what they are doing that is producing the problems. Nor, while working with those concepts and methods, can they imagine any other way of approaching their subject that will avoid the problems they are facing.

What I hope is now clear is that foundational problems in the Sciences — both of a methodological and conceptual kind — arise in part because of a series of contingent choices (evolutionary novelties, if you like) about theory development and conceptualization made at various nodes of the phylogenetic History of that Science. A careful study of the historical genealogy of a philosophical problem can provide a deeper understanding of it, and a richer sense of the theoretical alternatives open to us in solving it. I do not mean to suggest that this is the only worthwhile method for working on problems in the Philosophy of Science. What I hope I have convinced you of is that virtually every problem we work on in the philosophy of science can be illuminated by a phylogenetic reconstruction of that problem.

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