Dogma and Doubt

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The theory of natural selection has been re-examined in recent years by a number of critics concerned with the possibility of tautological formulation (Himmelfarb, 1962; Smart, 1963; Manser, 1965; Flew, 1967; Barker, 1969; Macbeth, 1971; Lewontin, 1972; Grene, 1974; Bethell, 1976; Peters, 1976). These criticisms have been dismissed, sometimes with impatience, by more authors than it is practical to cite here. Yet upon reading both critics and defenders it is easy to detect a deficiency in the exchange for communication is far from complete. The defenders have answered this ineffective complaint whenever they found it (and sometimes imputed it where they did not), but have spent little or no effort finding out what the critics might actually have in mind. Perhaps such problems are to be expected.

When a theory becomes part of the common working knowledge of an entire community it becomes the context within which that community understands the world. Doubt comes to be regarded as something less than legitimate, and critics find themselves talking only to each other. The critic is, in a certain sense, self-exiled, for he or she is trying to question what the common language of the field takes for granted, and this linguistic hurdle is a difficult one to overcome. Yet for the critic, the task is merely one of clarification. The other side, however, must deal with a condition which may turn out, in the end, to be far more debilitating, i.e. belief.

Specifically, I mean the belief that random variation can, when subjected to selective pressure for long periods of time, culminate in new forms, and that it therefore provides an explanation for the origins of morphological diversity, adaptation, and when extended as far as Darwin proposed, speciation. The principle of natural selection when understood in this sense may be equated with the Spencerian "survival of the fittest," as Darwin himself (1876) recognized in his later editions: "I have often called this principle...by the term natural selection. But the expression often used by Mr Herbert Spencer, of the Survival of the Fittest, is more accurate, and is sometimes equally convenient." While there is little doubt that such a theory provides the logical prerequisites of an explanation for the items listed above, the critics seem to be mainly concerned about whether the theory is supported by empirical evidence — i.e. whether the ubiquitous belief in evolution by natural selection is an empirical one. For their part, the defenders evidence some difficulty in understanding how the critics can propose that the 'fit' could fail to survive, at least more numerously than the 'less fit,' and how a continuous accumulation of such survivals could fail to lead to anything (i.e. fail to achieve the breeding of new types). In all candour, one must admit that the explanation is so logical and lucidly transparent that it is difficult to doubt. Yet if this is true of the mere argument, without recourse to empirical testing, are we not in danger of being seduced *prior* to such testing? And if this actually happened, would it not be the case that *inconclusive* tests would do nothing to shake our belief? Short of actual contradictory evidence, beliefs have a certain invincibility.

I shall argue, in the following discussion, that the real difficulty behind the recent criticisms is the inconclusive empirical status of the theory, and that the belief with which the theory is embraced by the defenders makes that empirical inconclusiveness all but invisible to them. The critics are tacitly aiming at a brand of science rather than a specific theory, and for this reason their concern may turn out to be of great importance in the near future.

Because the charge of tautology has been so central, I shall begin by examining the possible problems behind this complaint and then proceed to the arguments of critics and defenders within the present debate.

Tautology and Related Failures

Tautology always indicates some form of repetition, but whether or not that repetition is pejorative depends upon the particular context in which it is made. When the repetition is *useless*, as is the case in a bingo advertisement that invites ‘all husbands and married men,’ the repetition is simply a mistake. On the other hand, if we explain, to someone who really does not know, that ‘husbands *are* married men,’ we have used the repetition to explain the meaning of an unknown term, and no misuse of language is implied.

The context of statements in empirical science is usually causal explanation: this happens *because* that happened. Causal statements of this form are sometimes termed *synthetic* because the second half of the statement, which follows the *because*, must add something new, something not already contained in the first half. *Analytic* statements, by contrast, affirm some form of identity, and therefore repeat the first part in the second: i.e. ‘husbands are married men,’ or ‘a deafness is an impairment of the hearing.’ But when this definition strategy is used with causal intent, language breaks down. The statement that ‘your deafness is caused by an impairment of your hearing’ means only that your deafness is caused by deafness — and the intention to add something more than the fact of deafness is not carried through by the formulation. A scientific theory is a causal explanation and brings distinct elements into dependent relation: the thunder is caused by the lightning; your deafness is caused by a torn eardrum. Since cause and effect are not the same, the two sides of a causal proposition cannot be identical, and the repetition inherent in tautological formulation would be pejorative.

The pure *form* of causal explanation, therefore, forbids the use of analytic statement as non-explanatory, and the accusation of tautological (which implies analytic) formulation is a serious charge to make against a scientific theory. But there are related problems that must be investigated here before leaving our preparatory discussion, for the failure of tautology to add anything new to the known facts can be approximated by linguistic strategies which, although not tautological, still fail to add the requisite second element.

The general form of causal statement has the form *if this then that*, in which the *this* and the *that* are quite distinct elements. But the distinction between the two, which tautology fails to provide, is not the only necessary characteristic. Since scientific theories are empirical, they must refer to elements that are specifiable through experience. Thus, a second characteristic necessary to any properly formed theory is that it provide a means of specifying two independent sets of observations (for the *this* and the *that*, respectively). Any failure to provide such observations would preclude both application, for we would not know what was specified by the theoretical claim, and test, for we would not have observations *that could be compared* with theoretical prediction.

Take, for example, the satirical German folk-saying which claims that: ‘If the cock crows on the manure pile, it will rain — or it won’t.’ What does this really say about experience? Will anything be different if the cock crows? The two sides of the proposition are distinct, but the second part does not specify any observations, for it is simply too broad. One can neither apply nor test the statement, for it fails to specify any particular result. A like failure can be constructed for the other side of causal statement, as in the claim: 'if the right conditions obtain the harvest will be good.' Without some observational criteria for the meaning of 'right conditions' we cannot assign any empirical meaning to the statement, and thus, like the folk-saying, it remains empirically empty. Formulations far more subtle than my simplistic examples might be found, of course, and in such cases it might prove very difficult to detect the failure of the theory to have empiric content upon first reading. But the process of *testing*, through which we identify the two sides of the causal relation in terms of two sets of observations, in order to *compare* these with the prediction of the theory, puts the entire matter into high relief. We cannot check the prediction that if (X) obtains, (Y) will follow, unless we can identify (X) and (Y) in the world, and thus during the process of testing we should expect any failure of specificity to emerge.

Because the problem of tautological formulation is brought about by the failure to specify logically distinct elements in the causal statement, it can be confused with the failure to make those elements empirically distinct — i.e. to specify two sets of observations. We shall find, in the subsequent discussion, that several critics have made this mistake, and that worries about tautological reasoning are usually worries about the ability of the theory to specify enough, in the realm of observation, to produce a good test.

Differential Reproduction — The Tautological Formulation

Actual tautological formulation of Darwin's theory is found, to my knowledge, in only one form — that of differential reproduction. I find the practice of explaining any causal interaction with a tautology somewhat mysterious, given the obvious inability of analytic statement to serve in this capacity, but nevertheless several biologists have made the attempt. Waddington, for example, wrote in 1960:

> Natural selection, which was at first considered as though it were a hypothesis that was in need of experimental or observational confirmation, turns out on closer inspection to be a tautology, a statement of an inevitable although previously unrecognized relation. It states that the fittest individuals in a population (defined as those which leave the most offspring) will leave the most offspring. Once the statement is made, its truth is apparent. This fact in no way reduces the magnitude of Darwin's achievement; only after it was clearly formulated, could biologists realize the enormous power of the principle as a weapon of explanation.

Macbeth (1971) found this passage 'staggering.' It is even more astonishing to reflect that Macbeth's reaction was not the common one.

Waddington's argument suggests that his tautology explains something, and that is the oddity. What could possibly be explained in this manner? Observation of natural populations reveals that some individuals leave more offspring than others. If we care to assign a *name* to those prolific individuals, that is our own convention. But if we now go further and we say that the name explains *why* they leave more offspring, we have forgotten the factual basis of our identification of these individuals. The name we give them (fittest) means *only* that they leave the most offspring (by definition, as Waddington claims above). So our 'explanatory' statement becomes: the individuals who leave the most offspring leave the most offspring (writing 'leave the most offspring' for 'fittest' as we are entitled to do by definition).

One can only assume that Waddington must have forgotten that the term ‘fittest’ in his formulation, *could mean nothing else* than leaving the most offspring. It does not explain *why* they do so, for it adds no new element to the originally observed fact. It provides no cause for the effect. Upon reflection it is extremely difficult to believe Waddington’s claim that the idea that individuals who leave the most offspring do, in fact, leave the most offspring, was ever an ‘unrecognized relation’ — but it is very clear why biologists might be slow in recognizing ‘the enormous power of the principle as a weapon of explanation.’

All very obvious, or so one would expect. But J. B. S. Haldane (1935) did not think so:

> the phrase ‘survival of the fittest’ is something of a tautology. So are most mathematical theorems. There is no harm in stating the truth in two different ways.

Well, mathematical theorems are analytic in nature, as Haldane suggests, but that is just why he should have seen that natural selection should not be tautologically formulated. Causal theory differs from mathematics in that it must be synthetic rather than analytic. How Haldane missed the difference is hard to understand, but he was clearly oblivious to it when he wrote the passage above.

I have been belabouring a simple point because I want to be well rid of it before we get to the more serious problem of testability. The fact that the argument from tautology survived in the the literature from 1935 (Haldane) to 1960 (Waddington), however, may suggest something about the difficulty inherent in the attempt to identify fitness in nature, independent of offspring. Waddington actually implies, above, that this attempt has been a failure, since it is clear that if the requisite observations existed he would never have suggested the tautology. But I am getting ahead of the argument — let us look at the problem that the tautology was meant to address.

The Hand of Nature

Darwin’s original intent was clearly to designate a causal agency *behind* the differentials of reproduction. Such differentials exist — some organisms have more offspring than others of the same population — but were this effect undirected the differential would never lead to any particular result. On the other hand, if we speculate that some causal agency provides the differential with a direction, we have a hypothesis of the origin of diversity. Darwin provided a causal factor to do just this in his principle of natural selection which, when *added* to the naturally existing differences in a population, ‘selected’ some for advancement and others for retardation and eventual extinction. The scheme was advanced as an analogy to the selective activity of human breeders.

The argument begins with Darwin’s claim (1859) that present breeds of domestic stock are manifestly the work of human hands:

> We cannot suppose that all the breeds were suddenly produced as perfect and as useful as we now see them: indeed, in several cases we know that this has not been their history. The key is man’s power of accumulative selection; nature gives successive variations; man adds them up in certain directions useful to him. In this sense he may be said to make for himself useful breeds.

The claim is advanced in order to make the *analogy* at the base of the theory visible. Darwin continued his argument by hypothesizing a 'natural' selection which would act in a manner parallel to human selection. The organisms of any population, he said, were engaged in a competitive struggle:

> Owing to this struggle for life, any variation, however slight and from whatever cause proceeding, if it be in any degree profitable to an individual of any species in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of the individual, and will be inherited by its offspring. The offspring also will thus have a better chance of surviving, for, of the many individuals of any species which are periodically born, but a small number can survive. I have called this principle, by which each slight variation, if useful, is preserved, by the term Natural Selection, in order to mark its relation to man's power of selection.

Darwin is very clear, therefore, that the fact of reproductive differentials is central to his theory, but the crucial aspect is the *direction* of the differential, and it was this direction that natural selection was advanced to explain, even as human selection explained the differential directions that had, in the past, culminated in domestic breeds. The effect to be explained is the hypothesized differentials of the past, which culminated in present organisms. The cause advanced is the selective power of environmental pressure (counting other organisms as part of the environment of any particular individual), which acts in a manner analogous to the hand of man.

Cause and effect are logically distinct in this formulation, and offered clearly for empirical specification — that is, for research. In actual application the researcher will attempt to observe these relations in nature, and once the requisite sets of observations are identified, to test the relations. But here even Darwin sounded a warning. It may not be a simple thing to specify the observations. After all, nature may be 'infinitely complex,' and although it is not difficult to see what the breeder is doing, the observation of natural causes is a more subtle thing. Darwin remarked, as part of the same argument:

> Man can act only on external and visible characters: nature cares nothing for appearances, except in so far as they may be useful to any being. She can act on every internal organ, on every shade of constitutional difference, on the whole machinery of life....under nature, the slightest difference in structure or constitution may well turn the nicely-balanced scale in the struggle for life, and so be preserved....Can we wonder, then, that nature's productions should be far "truer" in character than man's productions; that they should be infinitely better adapted to the most complex conditions of life, and should plainly bear the stamp of far higher workmanship?

By actually stating that man "can act only on external and visible characters" Darwin has suggested a very definite limitation on human knowledge. The breeder can work only with what he can detect. The researcher will be in the same boat. To begin with, therefore, we must expect great difficulties in identifying the *reasons* for fitness in nature. But that was in 1859. Why should Waddington imply that no progress had been made by 1960?

The Modern Debate

The observations that Waddington was at least not sanguine about are those which Darwin originally placed beyond the breeder's art — the detection of causal parameters which, however fine, might "turn the nicely-balanced scale in the struggle for life." Others have expressed like reservations. Simpson, in 1953 wrote that "The fallibility of personal judgment as to the adaptive value of particular characters, most especially when these occur in animals quite unlike any now living, is notorious." Dobzhansky (1975) went further, concluding that *no* biologist "can judge reliably which characters are neutral, useful, or harmful in a given species." With such comments in the literature, it can surprise no one that even 'outsiders' began to wonder if adaptive changes could be identified, especially since those who are not included in the biological community are also not included in the dogma. We find Macbeth (1971), a retired lawyer, complaining about tautological reasoning and imaginative narratives (some call them "just-so stories"), and Bethell (1976), a journalist, writing that Darwin never advanced any technique by which to identify his hypothesized adaptations. Bethells' piece was answered by Gould (1976), and since both articles appeared in non-technical journals (Bethell in *Harpers* and Gould in *Natural History*) the public at large had an opportunity to review the disagreement. Unfortunately, an amount of miscommunication obscured the ground.

Bethell had argued that since researchers had no way to identify, in the total organism, what traits conferred what advantages, they were forced into the tautological formulation of identifying fitness through survival. Gould did not agree, writing that criteria of fitness independent of survival were indeed available:

> Now the key point: certain morphological, physiological, and behavioral traits should be superior *a priori* as designs for living in these environments. These traits confer fitness by an engineer's criterion of good design, not by the empirical fact of their survival and spread. It got colder before the woolly mammoth evolved its shaggy coat.

Gould is proposing not merely the advantage of single traits, but their integration in good overall design. He is correct in his interpretation of Darwin, and his logic is beyond question. If an animal were well designed for an environment, it would do well in that environment, since that is what 'well designed' means. The remark about the mammoth is merely hypothetical, however, since the evidence for the sequence he suggests is inconclusive. Gould gives the mammoth example as an imaginary representation of the relation postulated by Darwin's theory.

Bethell also offered an imaginary scenario, using his story to comment on exactly the point that Gould seems to be making. He imagines that a wolf has been born with more powerful legs than the rest of the pack. He then asks, is this trait adaptive?

> A mutation that allows a wolf to run faster than the pack only allows the wolf to survive better if it it does, in fact, survive better. But such a mutation could also result in the wolf outrunning the pack a couple of times and getting first crack at the food, then abruptly dropping dead of a heart attack because the extra power in its legs placed an extra strain upon its heart. Fitness must be identified with survival, because it is the overall animal that survives, or does not survive, not the individual parts of it.

The contribution of individual traits must be summed in the whole before we know how useful any one actually is. Yet since this summing is beyond the knowledge of the investigator, claims Bethell, the investigator does not predict survival from his knowledge of animal engineering — rather he observes the fact of survival and then attempts to explain this by reference to design. No matter then, how we explain survival after the fact, the *detection* of fitness is always through survival.

The same problems and response were described by Hull (1974) who saw the dynamic very well:

> If one only knew enough about the genetic makeup, the embryological development, and the physiology of the organisms concerned, as well as the vagaries of the environment, one could assign a certain degree of fitness to each of these organisms and hence be able to make reasonable predictions about their chance of survival. With this information, one could in turn predict subsequent changes in the population.

Unfortunately, we do not have this information, but even without it, Hull continues, we are compelled to explain:

> The evolutionary development of a particular species or populations as such cannot be predicted with any reasonable degree of certainty. Predictions are possible only to the extent that a population or species happens to fit one of the patterns of evolution that have currently been discovered.
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> One is tempted to rush by all such pragmatic consideration. Perhaps biologists do not know all the relevant variables and could not combine them meaningfully if they did, but surely nature does the summing for us. In principle, every organism that dies without leaving issue has a coefficient of fitness of zero. No matter that two individuals are identical twins with the same genotype — one could have an extremely high coefficient of fitness and the other a very low one. The appeal of this retrospective deterministic bias is difficult to resist. If one individual dies without reproducing itself and another succeeds in leaving numerous offspring, something must have been responsible for the difference.

Whether one works out the retrospective case with regard to the fate of a single individual, or as many would have it, the *probable* fate of an individual or group possessing a particular adaptation, *something* presumably makes a difference. Gould suggests that the crucial aspect is an integrated adaptive scheme, a design which is superior *a priori* to those other individuals in the population examined that do not possess it (superior, of course, in terms of its probability of survival in the selected environment). But if we allow nature to do the summing for us, after Hull's example, *we* never calculate the probability of survival, but simply identify the superior designs *after the fact of survival*, and then speculate on what led to this survival. No test can be forthcoming if we act in this manner, and this is the basis of Bethell's complaint. Although Medewar (1978) saw fit to call Gould's reply, which correctly shows that Darwin's *theory* was not tautologically formulated, "a pretty accomplished hatchet job on an unlucky Mr Tom Bethell," I cannot see that Gould has answered Bethel at all. He has never even mentioned the problem of testing, and one cannot but wonder what he would have said if Bethell had not given him an opening by mistaking his terminology and remarking, at one point, that the theory was tautological when he really means untestable.

Perhaps the theory has not produced a good test. Answering Lewontin (1977) on the lack of a significant test, Maynard Smith (1978) admits that when attempts at prediction are examined:

> What these examples, and many others, have in common is that a model gives predictions that are in part confirmed by observation but that are contradicted in some important respect. I agree with Lewontin that such discrepancies are inevitable if a simple model is used, particularly a model

> that assumes each organ or behavior to serve only one function, I also agree that if the investigator adds assumptions to his model to meet each discrepancy, there is no way in which the hypothesis of adaptation can be refuted. But the hypothesis of adaptation is not under test.
>
> What is under test is the specific set of hypotheses in the particular model.

One is tempted to ask, 'well, but when *is* the hypothesis of adaptation under test?' Of course, that is what Bethel wanted to know. He was not answered.

Testing

Theories become empirical by seeking empirical support, but actual evidence always has a two-edged potential — it can confirm *or* deny. If evidence did not have this dual nature, we should not look for it, since it could tell us nothing we did not already know. This reflection would be too obvious to mention if it were not for the ability of theories to seem to have empirical support when they do not. Tautological statements, for example, may seem perfectly supported by the facts — is not all deafness a result of an impairment to the hearing? — until we realize that such statements could never be *questioned* by the facts. It then occurs to us that their support must be derived from some other source. If we cast the result of this argument as a general statement, we may write: 'what experience cannot question it cannot support,' and in this form the insight becomes recognizable as the basis of Karl Popper's criterion of falsifiability (1934), i.e. the rule that any theory that cannot be questioned by observation cannot be deemed scientific (empirical). The rule may be applied to research programs as well as the theories involved.

The research program concerned with the mechanism of adaptation began with Darwin's warning on the limits of human knowledge already in place. It has been very successful, despite that, in demonstrating that a hypothetical account of adaptation is possible in *every* case. The target of the critics, however, has not been the possibility of producing such hypothetical scenarios, but of submitting them, and the general theory behind them, to empirical test. The critics imply that we simply do not know enough about the organic realm to understand what would bring our hypotheses into question. Let us examine the ground to see why this might be so.

When we go about the business of casting predictions from theory, we cannot assume a simple relationship between the theory and the world. That is, when I say that the acceleration of gravity is constant, I am not refuted by showing that a feather falls slower than a parachutist, or a stone faster. Faced with such facts, I simply reply that my statement, being a general law, did not take air resistance into account. Upon inspection, the constant acceleration of gravity *does not predict* what may happen to any actual falling object — it only specifies the *contribution* of gravity to that event. Making this explicit, I may say that falling objects will all fall with the same constant of acceleration if *no other causal parameters* interfere, or in the jargon of the profession, if we assume *ceteris paribus* (that all other things are equal, i.e. without effect on the consequent). Any predictive statement contains, tacitly, a *ceteris paribus* clause, and therefore at least one qualification.

Closer examination shows another qualification. In all but the simplest predictions I will make use of some other theory or theories to construct my statement. If I say that the acceleration of gravity is constant neglecting air resistance, I have a theory of air resistance that allows me to speak in this manner. If I make a prediction about the next eclipse of the sun, I have, besides the theory of gravity, a great multiplicity of other theories behind my statement. We have then, the application of the basic theory, the *ceteris paribus* clause, and the assumed theoretical background (all other needed theories) — all contained in predictive statements.

Now, supposing a prediction should fail, what is brought into question? There are three candidates: (1) my prediction is wrong because my theory is wrong; (2) my prediction is wrong because the *ceteris paribus* assumption is wrong (i.e. there are interfering parameters); and (3) my prediction is wrong because one of the background theories is wrong. When we make predictions for the sake of conducting a test, all three possibilities must be considered when the prediction fails.

When Maynard Smith speaks of the *ad hoc* addition of assumptions to the model in order to meet each failure of prediction, these assumptions may add to the basic hypothesis, to the *ceteris paribus* clause (we may assume that certain interfering parameters do exist), or to the theoretical background (although this last possibility is unlikely). There is nothing illogical about such additions, and their use has always been basic to science. When the Newtonian theory of celestial mechanics could not account for the orbit of Mercury, astronomers assumed some other factors (asteroid, gas cloud, etc.) must be interfering, and set out in search of them. Unfortunately, they never found anything, and so when Einstein's predictions made a better showing than Newton's (coming closer not only to the orbit of Mercury but also to many other measurements that escaped the Newtonian predictions) his theory supplanted Newton's. There are now some discrepancies in the Einstein predictions, but these are, so far, treated like the aberrant orbit of Mercury was by the Newtonians. Time will tell, or so we suppose.

But remember that Maynard Smith was not even talking about the central theory, but only about local hypotheses. We test such hypotheses by placing the basic theory in the theoretical background — we assume that it is unproblematic. Yet if we wanted to make it problematic — to submit it to test — could we do so?

A test is of no value if it cannot call the theory being tested into question. When astronomers found that the orbit of Mercury did not fit the predictions, they knew that the theory had troubles and began an effort to solve them. They did not give up the theory until they had an alternative, but the point is, the troubles were made quite visible. But when we come to Darwin's theory the real point of his warning about the difficulties inherent in identifying fitness emerge. Darwin did not put forward a comprehensive theory of the organism. He had, in fact, no way of summing the effect of traits. Nor have we, until we have a theory of the total organism. In other words, the *ceteris paribus* clause of the Darwinian hypothesis is indeterminate, since the theoretical background is incomplete. We have no idea what *sort* of interfering parameters might exist because we have no theory capable of reducing the organism to a calculable whole. The astronomers who worried about Mercury could at least calculate, using the Newtonian theory, just how much of an interference they needed and how large an asteroid, or dust cloud, etc., would produce it. When we try to understand adaptation we are reduced to 'just so' stories because the possibilities are limited only by imagination.

It is this very indeterminacy that caused Lewontin (1977) to worry about the nature of the *ceteris paribus* clause in any engineering analysis:

> In order to make the argument that a trait is an optimal solution to a particular problem, it must be possible to view the trait and the problem in isolation, all other things being equal. If other things are not equal, if a change in a trait as a solution to one problem changes the organism's relation to other problems of the environment, it becomes impossible to carry out the analysis part by part, and we are left in the hopeless position of seeing the whole organism as being adapted to the whole environment.

This is exactly what Bethel said. But if this point is admitted, something more damaging follows:

This section began with the notion that 'what experience cannot question it cannot support.' I have argued within it that until the organism is reduced to a determinate system, we have not the knowledge to mount a good test of optimalization theory — that is, we cannot question it. If that is so, it follows that *the theory has no empirical support*. Its strength comes from its logical power to generate explanations for every manner of organic adaptation rather than from the evidence, which, as we have seen, contains no potential for falsification. The theory may be true, but whether it is or not, it cannot be said to have shown *evidence* of this truth, and the widespread acceptance of the theory must rest on some other grounds.

Indirect Defences

A theory may be accepted for more than one reason. It may be judged, for example, to be better supported than its competitors, and therefore be acceptable *if* no serious objections are known. It may, on the other hand, be *believed*, which is an entirely different matter. Theory is a device for investigating and presumably understanding the relation of phenomena. If the empiricist may be said to learn from experience then he or she does so by discovering which theoretical postulations are supported and to what degree. Belief, by which conviction is formed *in spite* of gaps in the evidence, is another species of thought. It is not well designed for investigation and stands in a certain opposition to hypothesis evaluation. I say that because such conviction, since it settles the matter while the evidence is inconclusive, is by definition no longer interested in learning from experience. Interest in this case has shifted from inquiry to application. Since it is quite possible to think a set of thoughts *without* believing them, it would be advantageous for the empiricist to avoid believing the theory under investigation. There is certainly little scientific profit in a form of thought that has lost interest in its relation to the the evidence.

The 'indirect defences' in the title of this section are all those suggesting that the central hypothesis of Darwinism may be defended by some other means than direct testing. They proceed, in actuality, *by an application of the theory*, which is already believed by the defender, to the evaluation of the same theory. This 'by our own bootstraps' approach is so popular that examples abound in the literature, and I will limit myself to the three major forms that the strategy takes. These are, in the order of discussion, the transference of empirical support from a testable part of the theory to the untested central thesis, a claim of confirmation that cannot be credited to the theory at all, and a general claim of 'fruitfulness.'

The first example is to me the most impressive, since it represents actual confirmation of an important prediction, and the empirical studies are justly admired for that reason. I have in mind the research into industrial melanism in populations of *Biston* moths (for a review, see Ford 1971 [and see more recently "Science as Process or Dogma? The Case of the Peppered Moth," by Craig Holdrege - ed.]), since that is so often put forward as a test of the action of natural selection, although other work would have a bearing here as well. The problem stems from the fact that the test does not apply to what I have been calling the central thesis, but rather to a necessary prelude.

Darwin proposed that selective pressure could approximate the regulative control of an intelligent breeder and supply a direction to otherwise random changes in a population. This directed progression of changes would then lead towards optimalization of adaptation within the context of the given environment. There are two parts to this proposal. The first is that selective pressure can approximate the breeder; the second, which I have termed 'central,' is that the differential so produced can culminate in new forms. The industrial melanism observations have confirmed prediction with regard to the first part, i.e. a specific change in allele frequency, parallel to that produced by a breeder, may be caused by one-sided predator pressure (one-sided in that the melanistic variant is better camouflaged). Can this confirmation be transferred to part two?

Not at all, since this is the more radical claim. There seems to be some confusion about this point, but the argument is simple enough. What if such differentials *do not* culminate in new forms? After all, they did not do so in experiments mentioned. The populations studied were essentially stable throughout the whole period, as evidenced by the fact that as the pollution diminished the allele distributions shifted back towards their original balance. If we suppose that this differential is really parallel to that produced by breeders (and that is the point of the confirmation), then it should be quite clear that we have yet a distance to go. Breeders do not habitually introduce new species. Short of sudden mutations, in fact, they do not seem to do it at all, a point which Darwin probably had in mind when he reminded his readers that "man can act only on external and visible characters" (like colour changes), but nature was presumed to act on "every shade of constitutional difference." Although Darwin *assumed* that human agency had led to new forms in the obscurity of the past, he saw the need of an excuse for the breeders of his day, who did not seem to be approximating evolution. But the excuse leads to doubt. The melanistic shift is external and perceptible, and picked out for survival through a predator pressure just as external as breeder selection. Is it really the right sort of differential? How could we tell?

Or going all the way, are we sure that the random changes in a population are really the building blocks of evolution? Perhaps some other process is really behind things — hopeful monster, neo-Lamarckian mechanisms, who knows what? Alternatives have been suggested; have we enough evidence to reject them? Hardly, if the preceding section is correct.

Those who put forth the *Biston* evidence as a test of Darwin's thesis are either very unclear about which part of the thesis they mean, or they evidently 'know' something the critics do not know — namely, that part two of the theory is beyond any need of support. That is, they have already been convinced that the accumulation of random changes *is* the proper mechanism, and they point to the approximation of breeder regulation by predator pressure as a confirmation of the only necessary prelude.

If our aim is empirical investigation, any belief that can set up shop as 'knowledge' is always a fatal possession, for it undermines the basic project. The biologist who 'knows' that any differential can lead to new types is admitting that no empirical support is sought or needed for that proposition, which thereby becomes an *a priori* truth. Critics who are not blessed with similar metaphysical insight may gain the distinct impression that they are not viewing the same world, and there is some value in the metaphor. Whatever we actually believe we take to be identical with reality, and therefore *not* part of hypothesis that stands in need of support. Those who believe the Darwinian theory apply it, or parts of it, to their observations as a known parameter. Their results are artifacts of their belief, but this fact can hardly be visible to them until they are willing to question what they have previously taken for granted.

All 'indirect defences' contain such artifacts, although few include such admirable work as the *Biston* data. An example of how little data one actually needs for this approach can be found in Maynard Smith's 1969 defence of neo-Darwinian theory against the charge of tautology. Here, without any confirmation that could be credited to the theory, the author manages to argue empirical support. Reasoning that if actual observation could falsify any prediction of the theory, it could not be tautological, which is correct, Maynard Smith proceeds to examine what sort of observation might do the job:

> If one invents counter-examples, they seem absurd. Thus, if someone discovers a deep-sea fish with varying numbers of luminous dots on its tail, the number at any one time having the properties of a prime number, I should regard this as very strong evidence against neo-

> Darwinism. And if the dots took up in turn the exact configuration of the heavenly constellations, I should regard it as adequate disproof. The apparent absurdity of these examples shows that what we know about existent organisms is consistent with neo-Darwinism.

Let me unpack this argument by examining a more naive example of the same structure. Imagine that someone were to remark — and sometimes one need not be limited here to imagination — that Newton's theory receives daily confirmation in the fact that apples do not fall up. It would not be so difficult to explain, in this case, that Newton could take very little credit for the prediction. When he wrote his account of gravity, he undertook to link, in a significant way, the known behavior of heavenly bodies to the known behavior of weighted objects (apples, stones, etc. ...). From that theoretical linkage many *new* predictions could be derived, and for these Newton gets the credit (or blame, since some did not pan out). But the motion of the unsupported apple was a known fact when he took up his work, and had been claimed by all previous theories. Newton, and a good many others, would have been wrong if apples occasionally fell up, but they cannot derive predictive credit from their falling down.

All theories *must* exhibit an agreement with the observed behavior of phenomena when first proposed, or they could never be taken seriously. But this *canonical agreement*, as it is sometimes called, is not part of the empirical support of a theory. Its empirical support begins to accrue only when predictions that it *adds* to the already known behavior of things begin to show confirmations. After all, since all competing theories must have canoncial agreement *in common* or they could never reach the level of competition, empirical support, by which we hope to *differentiate* between competing theories, must be added over and above canonical agreement.

Returning to the example at hand, we can now see that since the imaginative findings that Maynard Smith postulates would have been just as unexpected in 1850 as they would have in 1969 — in that they go against everything we have been led to expect of such organisms through actual observation rather than the predictions of any particular theory — neither Darwin nor neo-Darwinism can gain empirical support by reference to them. The agreement in question is one that many theories, including that of Lamarck, would pass.

Theories, *qua* theories, simply cannot be given predictive credit for everything they agree with. They can never be uniquely responsible for an entire canon (of known facts in a field) but share this with any number of possible contenders. We are speaking here, of course, of theories as postulations whose relation to the world is yet to be determined. Once we are convinced that we know that relation, however, and that the theory in question is *true*, we may indeed assign it complete responsibility for the canon, discarding all other explanations, for it is now identified with reality. Since Maynard Smith argues that to postulate counter-examples to his understanding of neo-Darwinism we should have to imagine the world quite otherwise than it is, he has evidently taken the step in question. Predictably, any audience sharing his belief will be unlikely to notice that no empirical support can result from the procedure.

Once we have become convinced by our theory, for whatever reason, artifacts of that belief are bound to emerge, for we see the world in the context of our belief. The most common of these artifacts, however, is the 'fruitfulness' with which the Darwinian theory is credited throughout the profession. I do not mean the *historical* fruitfulness, for this certainly is possessed by the theory to a very great extent, since it was largely responsible for placing biology on an evolutionary footing. I am thinking rather of the ongoing empirical work which is credited to the theory — work that takes place in the context of assumptions 'all but proven.'

In my review of the present debate I quoted Maynard Smith (1978) on adaptation predictions, to the effect that the "hypothesis of adaptation" is not under test — "What is under test is the specific set of hypotheses in the peculiar model." These predictions usually missed, but he considered the work fruitful because the researcher could improve the model through it. Since this remark was prompted by Lewontin's argument (1977) that the notion of optimalizing adaptation did not seem to be testable, it seems fair, if treasonous, to imagine what would happen if this central thesis were found to be incorrect. Would we then call such work fruitful?

In actuality, the sort of work Maynard Smith is defending (and Lewontin is questioning) proceeds by assuming the central thesis as a known parameter and then investigating what follows from application of that parameter. Technically, the central thesis then becomes part of the theoretical background of the investigation (like the theory of air resistance in the investigation of falling objects discussed above) and is assumed to be non-problematic. But in this case, the central thesis is the whole game and the local hypothesis is really encased *within* it, as an *example* of the general relation (a particular case of optimalization within a general theory of optimalization). If the general theory were incorrect, anything like confirmation on the local level would be an optical illusion, for it would confirm a relation that did not exist. The 'learning' involved in a failure of prediction would be a like artifact, for by it we 'learn' how to 'correct' our model, when in fact if the basic theory is untrue there is nothing to model.

If we are in the position of saying "Since we now know the theory is correct, what follows?", the item under investigation here is not the world of experience, *but the theory*, for experience no longer has the power to question that belief. The addition of empirical evidence at this point changes nothing, because whatever evidence we include will be interpreted by our theory, producing such artifacts as the illusory 'confirmation' and 'correction' above. There should be no confusion about this. A firm conviction precludes any possibility of learning from experience — learning, that is, about the relation of the idea to evidence, or to empirical support. By treating the theory as a known parameter we approximate tautology by a means I did not specifically describe in the first section, although it is a failure of empirical specification. The theory is unbeatable because it is allowed to *interpret* our observations while they are being made or being recorded. Once this has been done, it is only logical that the data so collected cannot be used to question the interpretation, being a product of it. 'Fruitful,' in such a context, can only mean something like 'extending the applications of an interpretation' rather than 'discovering the relation of said interpretation to evidence.'

Because the artifacts of a belief presuppose rather than produce that belief, the 'indirect defence' cannot be the source of the belief it elaborates. We shall have to look further for the ground of the widespread acceptance of the theory, and just because it is so ubiquitous in our culture, it might be illuminating to speculate upon cultural origins.

Intellectual Backgrounds

Re-thinking Darwinian assumptions, the reader will find, seems to lead us a good distance beyond Darwin. We have only to examine the central argument of Darwin's text to find out how far. Fortunately, Darwin set forth this argument with great clarity and his reasoning is quite transparent. The two-syllogism argument leading to the natural selection of the fittest runs as follows:

<u>First syllogism</u>

Premises taken from observation:

1. Rapid potential increase of organisms
2. Constant population size

Conclusion:

A struggle for existence

<u>Second syllogism</u>

Premises taken from observation:

1. Struggle for existence (conclusion, above)
2. Inherited variation (observation)

Conclusion:

Survival of the fittest

Darwin did not claim that these were syllogisms, but he did claim, as did Wallace and many Darwinians since, that the conclusions followed of necessity from these premises, and thus the syllogistic form is implied. But of course, these arguments are not syllogisms at all but some form of sorites (a combination of several syllogisms leaving a number of premises unmentioned but tacitly included).

If we grant, for the purposes of argument, Darwin's first two premises (they can be questioned today, but that is not my interest), we must recognize that the conclusion does not follow without further information. Assuming that many more are born than survive long enough to reproduce, we may deduce *only a high attrition rate* that takes its toll prior to reproduction. This is a purely mathematical argument, it says nothing about the dynamic within such populations which might accompany such attrition. We may add that ourselves, but then we are admitting another premise. We are adding information to what Darwin said was already complete, as, in fact, Darwin must have done also without noticing it.

"A struggle for existence," wrote Darwin (1859), "inevitably follows from the high rate at which all organic beings tend to increase."

> Hence, as more individuals are produced than can possibly survive, there must be in every case a struggle for existence, either one individual with another of the same species, or with individuals of distinct species, or with the physical conditions of life. It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms.

But why? How did he know that organisms were competitive? Why not attrition by voluntary bachelorhood, or suicide, or some other form of cooperation? Why not attrition by mere accidental death without any comparative aspect? The part that Darwin added is the premise of competition, which is not derivable from the mere fact of high attrition. But Darwin also had his artifacts.

Notice that the second syllogism will not work without the qualifications of *competitive* struggle (not just exertion), and variation *among traits that decide the result of competition* (those traits that have proven so hard to identify). The whole argument is called an application of Malthus, but Malthus himself was writing within the context of British economic thought, as summarized, if any one man can summarize it, by Adam Smith. Darwin thinks within the same context, and his debt to the theory of free market competition is immeasurably greater than his debt to Malthus. Without such a context none of these arguments makes sense.

It was Adam Smith who argued that while other cultures regulated their economic activity through religion or tradition, the 'free' market needed no such extra-economic regulation because economic forces alone could perform that task. Regulation of some sort was, Smith admitted, a necessity, in order that price reflect value. Yet, when individuals entered the market with nothing but their own greed to guide them, the conflict of interest arising between those individuals led to a competitive interchange which approximated the act of an intelligent regulator. When price rose above value, some competitor was bound to undercut it to move in on his profits, thus starting the downward trend. When it fell below, producers would be forced out of the market and the drop in supply would bring it up. No human regulator could do any better, or even as well, for the market mechanism was very sensitive.

Smith added, however, that a further benefit accrued from competitive regulation that was largely unknown in other markets. Because any increase in efficiency of production allowed the producer the competitive advantage of lower price, such competition continually benefited the efficient to the disadvantage, or even extinction, of the inefficient (efficient or inefficient, that is, within the given market). The cumulative result of this dynamic over long periods was the evolution of the economic system toward greater and greater productive capacity for less and less human labor. And of course on the local level, the system tended toward the elimination of the inept and the rise to economic prominence of the 'fit.'

As Cannon (1961) has demonstrated, Darwin needed a mechanical approximation of intelligent regulation to answer the Protestant theologians (especially Paley). He got it from the free market mechanism of Smith, who had already revealed how the thing could be done in *The Wealth of Nations* (1776). The fact that he did not give credit to Smith as well as Malthus indicates how fully immersed in the world view of the free market he was. It never occurred to him that such obvious truths needed a citation! I am not sure that we moderns are very different.

If we are candid, we should be able to see that Darwin's original argument is very convincing. This should lead us to reflect on how dependent we are of the same economic context for our outlook. After all, the argument follows *only* by the grace of our tacit assumption of the free market model of competitive dynamic. But we need only look to current political issues to see how compelling that model is.

Francis Bacon, in the *Novum Organum* (1620), wrote that a true empiricism could only be furthered in a mental atmosphere cleared of the various 'idols' (contextual assumptions) of one's culture. Among these he included the "idols of the marketplace." His model of induction was not correct, but his warning with regard to the substitution of cultural artifacts for empirical evidence seems more timely today than it did then. Scientists are only human, after all. They begin their adult lives, like everyone else, armed with all sorts of *a priori* 'knowledge.' But since the time of Bacon it is usually understood that the task of the empiricist includes a purging of such material.

At the moment, one might argue, with good cause, that the scientific community is somewhat indecisive about its allegiance. Both scientists and philosophers of science are unclear as to whether 'fruitfulness' should be measured by an empirical or a social standard. Is a theory more successful when it tests well or when it commands a wide consensus? We cannot have it both ways for the two are simply not the same.

Cannon, who is a historian, simply described what he saw, and the resulting analysis (1961) reveals the rather schizoid nature of our value system. Examining Darwin's procedures for characteristic traits, he writes:

> Here I think we will eventually find the secret of Darwin's greatness, in two traits not always praised in theories of 'how to conduct yourself scientifically.' One is Darwin's notorious habit of jumping to conclusions without adequate evidence. He developed his coral reef theory, we remember, before examining coral reefs. The other is that of stubbornly maintaining his theories regardless of the valid arguments and evidence that could be brought against them. [Cannon's note at this point refers the reader to the argument over the 'Parallel Roads of Glen Roy' paper.]
>
> These are the procedures to be recommended, of course, only to the great; and I come to the regrettable conclusion that science takes great strides forward not primarily from laborious research, but rather when some biased person maintains his intuitions in public, and when, thereafter, generations of scientists find that some of these intuitions do actually illuminate whatever work they are doing.

We should perhaps excuse the author for the sophistry of recommending procedures "only to the great," when the only logical thing he can mean is "only when you are right." What else could he say to an audience convinced of Darwin's greatness? The unfortunate thing about this argument is that it despairs of any objective criterion in the end and settles for consensus. The 'illumination' of the last lines is, I am afraid, little more than the 'fruitfulness' of the theory to those who believe it. This paper, of course, has very different things to say about "how to conduct yourself scientifically," and Cannon obviously knew that Darwin's 'virtues' were debatable. As a historian, he does not try to say what scientists should do, but rather what they *have* done. But if we let the matter rest with past practice, there will be a cost.

Conclusion

At the moment, the critics of Darwinian theory are turning in rather profitable work for those interested in empirical investigation. They are clearing the ground for new questions, new investigations — questions which could not be heard in a dogmatic context because their area of inquiry was preempted by the Darwinian answer. While that answer was believed, there was nothing to inquire about. The critics are on their way to moving that answer back into the realm of theory, where it belongs. Once this has been done it will result in a certain freeing of inquiry. (For a summary of *some* of the possibilities, see Rosen & Buth, 1980.) What is at stake is not the validity of the Darwinian theory itself, but of the approach to science that it has come to represent. The peculiar form of consensus the theory wields has produced a premature closure of inquiry in several branches of biology, and even if this is to be expected in 'normal science,' such a dogmatic approach does not appear healthy. Lewontin (1972) seems to agree:

> For what good is a theory that is guaranteed by its internal logical structure to agree with all conceivable observations, irrespective of the real structure of the world. If scientists are going to use logically unbeatable theories about the world, they might as well give up natural science and take up religion.

Of course, to make sense of the above complaint, one must agree that there is, or should be, a difference between these two institutions.

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References

Barker, A. D., 1969. An approach to the theory of Natural Selection. *Philosophy, 44:* 274.

Bethell, T., 1976. Darwin's mistake. *Harpers Magazine, 252:* 70-75.

Cannon, W., 1961. The bases of Darwin's achievement: a reevaluation. *Victorian Studies* 5: 109-134.

Darwin, C., 1859. On the Origin of Species, 1st ed. London:

Darwin, C., 1876. On the Origin of Species. London:

Dobzhansky, T., 1975. Book review. *Evolution, 29:* 376-378.

Flew, A., 1967. *Evolutionary Ethics*. London: Macmillan.

Ford, E. B., 1971. *Ecological Genetics*, 3rd ed. London: Chapman & Hall.

Gould, S. J., 1976. Darwin's untimely burial. *Natural History* 85: 24-30.

Grene, M., 1974. The Understanding of Nature. Essays in the Philosophy of Biology. Boston: Reidel.

Haldane, J. B. S., 1935. Darwinism under revision. *Rationalist Ann*. 19-29.

Himmelfarb, G., 1962. Darwin and the Darwinian Revolution. New York: Doubleday.

Hull, D., 1974. *Philosophy of Biological Science*. Englewood Cliffs, New Jersey: Prentice-Hall.

Lewontin, R. C., 1972. Testing the theory of natural selection. *Nature*, 236: 181-182.

Lewontin, R. C., 1977. Adaptation. In *The Encyclopedia Einaudi*. Torino Giulio Einaudi Edition.

Lewontin, R. C., 1978. Adaptation. *Scientific American*, 239-3: 212-230.

Macbeth, N., 1971. *Darwin Retried*. Boston: Gambit.

Manser, A. R., 1965. The concept of evolution. *Philosophy*, 40: 18-34.

Maynard Smith, J., 1969. The status of neo-Darwinism. In C. H. Waddington (Ed.), *Towards a Theoretical Biology*. Vol. 2. New York: Aldine.

Medewar, P., 1978. Book review. *The Sciences, 18-5:* 24-27.

Peters, R. H., 1976. Tautology in evolution and ecology. *American Naturalist, 110:* 1-12.

Popper, K., 1934. *Logik der Forschung*. [*Logic of Scientific Discovery*. London: Hutchinson. 1959. English translation.]

Rosen, D. E. & Buth, D. G., 1980. Empirical evolutionary research versus neo-Darwinian speculation. *Systematic Zoology*, 29: 300-308.

Simpson, G. G., 1953. *The Major Features of Evolution*. New York: Columbia University Press.

Smart, J. J. C., 1963. Philosophy and Scientific Realism. New York: Humanities Press.

Waddington, C. H., 1960. Evolutionary adaptations. In S. Yax (Ed.), *The Evolution of Life*, p. 385. Chicago: University of Chicago Press.

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