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Adaptation Considered Harmful: Darwin's Problem Revisited

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Abstract

Evolutionary Epistemology—as formulated by D.T. Campbell and Karl Popper—can be seen as a more or less explicit and conscious attempt to *generalise* Darwinian theory, in an effort to solve the fundamental problem of epistemology: how can knowledge grow? I think we are entitled, therefore, to be at least a *little* concerned that Popper himself for a long time regarded Darwinian theory as tautological and/or metaphysical; and (worse?) that when he eventually recanted, he proceeded to declare that Darwinism has, in fact, been refuted!

In an attempt to make sense of this situation, I here revisit Darwin's original problem. I will suggest that once this *problem* is properly understood, then the more common confusions and misunderstandings associated with Darwin's *solution* can be satisfactorily resolved. In particular, I make the modest terminological proposal that the word "adaptation" be forthwith excised from the lexicon of evolutionary theory—it has done a good deal more harm than good!

Finally, I will briefly consider the relevance to Darwin's problem of some recent (purported) demonstrations of "artificial" Darwinian evolution, or *Artificial Life*.

Contents

1 Prologue

(1) The claim that Darwinism is *tautological* is an extremely hardy weed. It seems to sprout again, at irregular, but fairly frequent, intervals; this, despite even the most seemingly severe of critical commentary and dismissal (see, for example, Dawkins 1982, pp. 180–181, for a list).

(2) It is of course, a fundamentally Popperian notion that we are all fallible, and mistakes will occur, and to that extent perhaps we should not be surprised that the mistake of regarding Darwinism as tautological should *re*cur from time to time. Nonetheless, I suggest that there is at least a *prima facie* case for thinking that the issue must go a little deeper in this particular case. The very resilience of the doctrine suggests that it can hardly be *trivially*, or *manifestly* mistaken. Furthermore, those who have propounded this doctrine at various times seem, at the very least, to be otherwise sensible and intelligent people. I may mention Popper, of course, but he himself has pointed out that he was influenced in this by a number of other distinguished authorities (Popper 1978, p. 144).

(3) My problem then is to at least *explain* the obstinate durability of the thesis of Darwinian tautology; and perhaps, having explained and understood this phenomenon, to mount a definitive critical attack on the thesis.

(4) Thus, my objective here is not to persuade you that the tautological view of Darwinism is mistaken—though, if you are in doubt, that would be a beneficial side effect. Rather, my objective is to convince you that this thesis of Darwinian tautology is not *trivially* mistaken. That, in fact, the thesis exists in a remarkable variety of similar looking, but fundamentally distinct, forms. As it happens, these diverse forms require quite distinct criticisms; criticism that is effective against one may be irrelevant to others. My theory, if it might be dignified with that term, is that it is this very diversity that explains the historical difficulty of mounting a (single) decisive criticism; for in the face of each such individual challenge the doctrine can simply mutate into, or be displaced by, one of its alternative forms that is resistant to that specific criticism.

(5) In short, what I propose to present here is the analog of a "broad spectrum antibiotic", which will attempt to attack the thesis of Darwinian tautology—and some closely related theses—in as many of their variant forms as I possibly can. I will not pretend to you that this attempt at *exhaustive* criticism can possibly be stimulating or fascinating in itself; it is a tale of minute, intricate, and very confusing detail. Nor can I even hope to definitively or permanently dispatch these theses—as Popper has emphasised, even our criticisms are forever tentative and subject to correction. Nonetheless, I

will beg your indulgence for inflicting this essay upon you, in the hope that by presenting the issues and arguments as clearly and consistently as possible, I might at least offer the next generation of would-be Darwinian tautologists a clear target to practise against.

2 Immutability and Common Descent

> In considering the Origin of Species, it is quite conceivable that a naturalist, reflecting on the mutual affinities of organic beings, on their embryological relations, their geographical distribution, geological succession, and other such facts, might come to the conclusion that each species had not been independently created, but had descended, like varieties, from other species...
>
> Darwin (1859, Introduction)

(6) Here, right at the start of *The Origin* Darwin concisely introduced, and immediately solved (or, at least, proposed a solution to) a difficult biological problem—namely, why does the biological world exhibit such hierarchical patterns of similarity, and geological sequence, between distinct species of organisms? There is, of course, an historical context to this problem: the assumption that species are immutable. Given this assumption, then these many relations of similarity, at multiple hierarchical levels of classification, seem arbitrary and thus problematic. Conversely, if immutability is discarded, then the problem situation is crucially changed, and we can conjecture that most, or perhaps even all, organisms are bound by relationships of common descent—and this very easily and naturally explains the hierarchical pattern of similarity.

(7) It is worth emphasizing that Darwin did not *originate* either this problem or its solution—and nor did he claim to. As Burrow put it, in his editor's introduction to a modern reprinting of *The Origin*:

> The theory of evolution [i.e. of common descent] was already an old, even a discredited one. Darwin, in later editions of *The Origin*, listed over thirty predecessors and was still accused of lack of generosity.
>
> Burrow (1968, p. 27)

(8) The theory of common descent can, of course, be challenged. For example, its most ardent critics are the proponents of "scientific creationism".

However, for the purposes of this *particular* article, I am going to (tentatively) adopt the theory of common descent as *true*. I make this point explicitly at the outset, so that it will clear in what follows that the problem I am dealing with is *not* the problem of hierarchical biological *similarity*, and should not be confused with it.

3 Darwin's Problem

> ... Nevertheless, such a conclusion [common descent], even if well founded, would be unsatisfactory, until it could be shown how the innumerable species inhabiting this world have been modified, so as to acquire that perfection of structure and coadaptation which most justly excites our admiration.
>
> Darwin (1859, Introduction)

(9) With this continuation of the earlier quotation from Darwin the problem situation is now extended, in a crucial way. We arrive at what I shall term *Darwin's Problem*:

P_d **(Darwin):** Given that species descend from other species (i.e. given the theory of common descent), how is it that, in some cases at least, this descent has been accompanied by an increase in *adaptive complexity*?

(10) I introduce (albeit with some hesitation) the term *adaptive complexity* here, following Maynard Smith:

> The main task of any theory of evolution (*sic*) is to explain adaptive complexity, i.e. to explain the same set of facts which Paley used as evidence of a Creator. Thus if we look at an organism, we find that it is composed of organs which are at the same time of great complexity and of a kind which ensures the survival and/or reproduction of their possessor. Evolution theory must explain the origin of such adaptations.
>
> Maynard Smith (1969, p. 82)

(11) Popper (1978) has similarly formulated a version of P_d by reference to Paley's famous "argument from design"—i.e. that the appearance of "Design" in the biological world "proves" the pre-existence of a designer or creator (which is to say, *God*). Popper emphasizes that Darwin himself acknowledged a strong influence from Paley's formulation (and attempted solution) of this problem.[1]

(12) Dawkins has adopted the terminology of "adaptive complexity" from Maynard Smith (Dawkins 1983, p. 404), and I shall use this phrase freely in what follows, but I shall also synonymously (?), and sometimes preferentially, refer to "inate" or "inborn" *knowledge* in the sense introduced by Popper:

> I assert that every animal is born with expectations or anticipations, which could be framed as hypotheses; a kind of hypothetical knowledge. And I assert that we have, in this sense, some degree of inborn knowledge from which we may begin, even though it may be quite unreliable. This inborn knowledge, these inborn expectations, will, if disappointed, create *our first problems*; and the ensuing [somatic time] growth of our knowledge may therefore be described as consisting throughout of corrections and modifications of previous knowledge.
>
> Popper (1961, pp. 258–259)

(13) P_d refers to an increase or growth of adaptive complexity. I thus implicitly assume some notion of "degree" of adaptive complexity; but I require only that this can be defined *a posteriori*. That is, *given* two organisms, I suppose that I will, in general, be able to rank them (at least roughly) in terms of relative adaptive complexity. I specifically eschew any attempt to define an *a priori* measure of adaptive complexity.

(14) von Neumann has described a similar idea as follows:

> There is a concept which will be quite useful here, of which we have a certain intuitive idea, but which is vague, unscientific, and imperfect ... I know no adequate name for it, but it is best described by calling it "complication." It is effectivity in complication, or the potentiality to do things. I am not thinking about how involved the object is, but how involved its *purposive operations*

[1] At one point Popper explicitly calls this "Paley's problem" (p. 345); but elsewhere in this same paper he refers to "Paley's and Darwin's problem" (p. 342), which is thus consistent with my designation of P_d as "Darwin's Problem". I shall consistently use the latter name; partly because I have used Darwin's formulation, rather than Paley's, to introduce it; but mainly because there is a subtle difference between the problem as *originally conceived* by Paley, and as *actually solved* by Darwin. In my construction (P_d), the (conjectured) truth of common descent is an explicit part of the problem situation; whereas, for Paley, the truth of common descent was neither a necessary part of (nor even particularly relevant to) the problem situation.

> are. In this sense, an object is of the highest degree of complexity if it can do very difficult and involved things.
>
> von Neumann
> (1966 Fifth Lecture, p. 78, emphasis added)

(15) Maynard Smith has similarly endorsed this somewhat loose approach:

> At the outset we are faced with a difficulty: we have no way of measuring the degree of complexity of a structure. Thus although most of us would readily agree that the organs of a man are more complex than those of an amoeba, and those of an amoeba more complex than those of a bacterium, we have no agreed criteria on which to base this decision, and no way of deciding by how much one organism is more complex than another.
>
> It may therefore seem odd to start formulating a theory of evolution by introducing a term which cannot be fully defined. However, I see no escape from doing so. *If organisms were not both complicated and adapted, living matter would not differ from dead matter, and evolution theory would have nothing to explain.*
>
> Maynard Smith
> (1969 pp. 82–83, emphasis added)

(16) I consider that P_d is the central, perhaps even the defining, problem of evolutionary biology, and is the problem Darwin hoped to solve with his theory of *Natural Selection*.[2]

(17) Before attempting to solve P_d we must be clear as to how P_d goes beyond the basic problems of the hierarchical relationships of similarity and geological ordering exhibited by biological organisms; that is, in what respect(s) the theory of common descent fails to address (much less solve) P_d.

(18) Briefly, while the theory of common descent asserts the existence of a unique phylogenetic tree, P_d goes on to ask: why does the phylogenetic tree has the particular structure that it has? Why, above all else, does it display at least some cases of increasing adaptive complexity?

[2] To this extent, the title of Darwin's book, *The Origin of Species* may be slightly misleading—it directs attention specifically at the phenomenon of *speciation*, rather than at the growth of adaptive complexity. While speciation is certainly an important issue we can say nonetheless that it is of secondary importance compared to the question of adaptive complexity. Diversity of species as such would be of relatively little interest if all species were equally rudimentary; conversely, even if there were only one species which displayed adaptive complexity, this one species would be of overwhelming interest. This is not, of course, to argue that speciation and the growth of adaptive complexity are *independent* phenomena, but merely that I shall concentrate on the latter.

(19) Note that the claim here is not that *all* evolutionary lineages have involved increasing adaptive complexity, but merely that at least *some* have. Similarly, even for those evolutionary lineages where there has been a net increase of adaptive complexity, there is no claim that this increase has occurred at a steady rate, or that it has been monotonic, or (most especially) that it will continue into the future.

4 A Solution: Organismic Darwinism

(20) It should not, perhaps, be necessary to burden the world with yet another presentation of Darwinian theory. Yet, insofar as the substance of this article is concerned with alleged misunderstandings of this theory it will be as well for me to present my own version—not least because it may transpire, through criticism, that it is actually *my* version that is mistaken!

(21) Perhaps more importantly, I have spent some years struggling with Darwinian theory, trying to ensure that I have grasped it fully. I am still not at all sure that I have succeeded; but I am satisfied that it is a more complex, and subtle, theory that is generally admitted, especially in the popular literature. This, in itself, persuades me to attempt a fresh presentation.

(22) I take the theory of *Organismic*[3] *Darwinism*, denoted T_d, to involve a number of separate theses, which I shall now describe in detail.

4.1 Reproduction

(23) Individual organisms are typically capable of reproduction, and in this way give rise to organism *lineages*.

4.2 Inheritance

(24) Reproduction exhibits recurring patterns of similarity (heritability) between parents and offspring. Certain (sub-)lineages can thus be distinguished by reference to characteristics which are preserved or propagated by inheritance within these (sub-)lineages.

[3] Attempts have, of course, been made to apply "Darwinian" theory to a vast range of domains; I use the "organismic" qualifier here simply to emphasise that I am talking about the application of Darwinism in its original context of the evolution of biological organisms. This usage is loosely related to the *Organismic Evolution* of Beurton (1981), and the *Organismic Selection* of Wright (1980).

(25) Perhaps the best way of describing this notion of a lineage distinguished by inheritance is as an example of a *self-producing* system: it is an instantiation of a set of components collectively having just the property that they can regenerate that same set.

(26) I shall refer to these special, inheritance-based, self-producing, lineages as *similarity-lineages* or *S-lineages*.

(27) Note that, in general, lineages can, of course, be delimited in ways *other* than by reference to inherited characteristics—but only the latter particular kind of lineage, namely S-lineages, will be in question here. Note also that any given offspring of a parent organism may or may not inherit any specified characteristics, and thus may or may not belong to specific S-lineage(s) which the parent belongs to.

4.3 Selection

(28) Organism populations belonging to distinct, inheritance-based, S-lineages can interact in a manner giving rise to a *quasi-deterministic selection dynamics*. That is, an S-lineage population can consistently grow, at the expense of one or more other S-lineage populations, eventually displacing the latter completely. An S-lineage can thus be spontaneously and naturally *selected* over one or more other lineages. I shall call this a *(natural) selection process*.

(29) Of course, lineages in general, and S-lineages in particular, may participate in a very wide range of diverse ecological processes, most of which do *not* involve the quasi-deterministic elimination of one or more or them. Selection processes are, in this sense, only one special case of a much more general phenomenon. Nonetheless, selection processes play a peculiarly important role in the Darwinian explanatory schema, and must therefore be clearly separated out from all other possible ecological interactions.

(30) Note carefully that the conditions under which such a selection process will arise are highly variable, and difficult to characterise in general. Note, in particular, that not all lineages—not even all S-lineages —will necessarily be capable of participating in such a selection process; and whether such a process arises will depend both on the specific S-lineages present, and the extra-organismic ("environmental") situation.

(31) It is worth noting in passing here that the observation that the entities which get selected in Organismic Darwinism selection are a special kind of *lineage* (as opposed to individual organisms) is a major insight that was only fully or properly recognised within the last 40 years. Dawkins (1989, p. ix)

provides a brief review of the genesis of this insight. It was originally formulated as the theory of "kin selection", but has been more recently popularised by Dawkins under the banner of the "selfish gene".[4]

(32) In circumstances where a selection process *will* occur it is generally possible to identify a numerical parameter, which can, in principle, be *independently* evaluated for each S-lineage involved, such that the relative values of this parameter are predictive of the ultimate outcome of selection (and, indeed, of the rate at which the selective displacement will proceed). The parameter will typically be a function of expected fecundity and mortality in the given conditions.

(33) This numerical parameter is commonly referred to as *fitness*; however, like "adaptation", "fitness" is a term which has had a variety of ambiguous and conflicting meanings in the Darwinian literature, so, for the moment at least, I would prefer to avoid it. Instead I shall adopt the relatively neutral term *selective-value*, or *S-value*.[5]

(34) Note that S-value is not a property of an S-lineage as such: it is a property of an S-lineage *relative* to a particular selection process—that is, relative to a particular set of other S-lineages, with which it would interact, and relative to a particular set of common environmental conditions. The *same* S-lineage could be characterised by utterly different S-values relative to different selection processes; and S-value would be literally *undefined* on sets of S-lineages which do not collectively generate selection processes.[6] The common metaphor of Darwinian evolution as a process of hill-climbing on a "fitness landscape" is thus, at best, a highly dubious one.

4.4 Variation

(35) New S-lineages are spontaneously formed, on an ongoing basis, as "variations" on pre-existing S-lineages. In this way, new selection processes arise, and episodes of selective displacement occur, on an ongoing basis.

(36) Furthermore, the mechanisms of S-lineage variation are *unjustified*—

[4] I find the latter term to be seriously confusing. This is discussed at length in (McMullin 1992*c*).

[5] The common "S" prefixes in S-lineage and S-value are co-incidental, since they denote "similarity" and "selection" respectively; but since only S-lineages can have S-values, it is a convenient co-incidence; I did not intend it, but I am happy to allow it stand.

[6] S-value is not even necessarily transitive. One could formulate S-lineages A, B, C, with properties such that all three distinct pairs would give rise to selection processes, where A would reliably displace B, B would reliably displace C, yet C would reliably displace A!

they are not mediated by knowledge or prediction of the properties such variant S-lineages would exhibit. More specifically: the S-value of a variant relative to any other S-lineages with which the variant might interact in a selection process has no influence on whether or not that variant will be generated in the first place.[7]

4.5 Growth of Adaptive Complexity

(37) Finally: in those phylogenetic lineages which have exhibited significant growth of adaptive complexity, this has arisen through sustained or repeated correlation between adaptive complexity and S-value of S-lineages making up these phylogenetic lineages. More specifically, where adaptive complexity has grown, the increments in adaptive complexity have arisen as unjustified variations in the properties of S-lineages, which variant S-lineages have selectively displaced competing lineages of less adaptive complexity, by virtue of their *also* having relatively greater S-value. All long term growth of adaptive complexity is composed of such events of unjustified variation and selective retention.

(38) It is crucial to note here that this final thesis does *not* entail that adaptive complexity be *always* correlated with S-value, or (therefore) that adaptive complexity should *necessarily* grow. Maynard Smith makes the point thus:

> There is nothing in neo-Darwinism which enables us to predict a long-term increase in complexity. All one can say is that since the first living organisms were presumably very simple, then if any large change in complexity has occurred in any evolutionary lineage, it must have been in the direction of increasing complexity; as Thomas Hood might have said, 'Nowhere to go but up'. But why should there have been any striking change in complexity? It is conceivable that the first living thing, although simple, was more complex than was strictly necessary to survive in the primitive soup, and that evolution of greater fitness meant the evolution of still simpler forms.
>
> Maynard Smith (1969, pp. 88–89)

(39) That is, this theory solves P_d only in the special sense that it *permits* a spontaneous growth of adaptive complexity; it does not compel or predict such growth.

[7] This thesis is commonly expressed by saying that variation is "random"; but that term is very vague and ambiguous.

(40) This is an absolutely crucial point which is very rarely made explicit. Instead it is commonly assumed, implicitly, that increased adaptive complexity will just obviously or naturally involve increased S-value (and will therefore be selected). This is utterly mistaken, and leads directly to the various tautology fallacies surrounding Darwinian theory.

4.6 Testing?

(41) I hope it is clear that each of these theses, which are collectively claimed to compose T_d, makes empirical claims that are potentially testable: each of them could, potentially, be mistaken. Of course, actually carrying out severe tests is difficult (to put it mildly). Popper put the situation thus:

> ...Darwin's own most important contribution to the theory of evolution, his theory of natural selection, is difficult to test. There are some tests, even some experimental tests; and in some cases, such as the famous phenomenon known as "industrial melanism", we can observe natural selection happening under our very eyes, as it were. Nevertheless, really severe tests of the theory of natural selection are hard to come by, much more so than tests of otherwise comparable theories in physics or chemistry.
>
> Popper (1978, p. 344)

(42) Nonetheless, for the purposes of *this* article, I am concerned only to show that the theory *has* empirical content, and does—if accepted—solve its problem (P_d). Thus, as with the theory of common descent, I shall now (tentatively) adopt T_d, the theory of Organismic Darwinism, as *true*.

5 The Status of Organismic Darwinism

(43) I have presented and analysed Organismic Darwinism as an essentially straightforward theory—whose scientific status is not problematic. However, this has not been a universally accepted position—and this brings me to the central topic for this article.

(44) In fact, the status of Organismic Darwinism is a very vexing question, which has received considerable attention in both the biological and philosophical literature; the debate has been positively acrimonious on occasion.

(45) I shall consider two closely related questions: whether Darwinism may be a mere *tautology*, or, failing that, whether it may be *irrefutable* (and thus

metaphysical). These two issues are commonly conflated—for example by Maynard Smith (1969). It is of course true that all tautologies are untestable and therefore (in a trivial sense) metaphysical, but the converse does not hold—theories may well be metaphysical yet not tautologous. Thus, given that Darwinism is *not* actually tautological then its testability becomes entirely moot, and should be considered in its own right.

5.1 Tautology...

> My dear Darwin,—I have been so repeatedly struck by the utter inability of numbers of intelligent persons to see clearly, or at all, the self-acting and necessary effects of Natural Selection, that I am led to conclude that the term itself, and your mode of illustrating it, however clear and beautiful to many of us, are yet not the best adapted to impress it on the general naturalist public ... I wish, therefore, to suggest to you the possibility of entirely avoiding this source of misconception in your great work (if not now too late), and also in any future editions of the 'Origin', and I think it may be done without difficulty and very effectually by adopting Spencer's term (which he generally uses in preference to Natural Selection), viz. 'Survival of the Fittest'.
>
> Wallace (1866)
> [Quoted by Dawkins (1982), pp. 179–180]

(46) I believe that the charge of tautology leveled against Organismic Darwinism is quite mistaken. However, contrary to, for example, Dawkins (1982, pp. 180–181), I also consider that the confusion is deep seated and subtle. In particular, I believe that there are (at least) three quite different *kinds* of misconception involved.

(47) There are a number of more or less authoritative replies to the tautology charge already available in the literature (some of which I shall comment upon below); but these have, in general, only recognised one or another of the various possible misconceptions. I am not aware of any previous discussion which has clearly distinguished all three kinds of misconception which I identify here. I suggest that this may explain why this debate has sometimes appeared interminable: the participants have frequently been talking at cross purposes. In itself, this would justify the somewhat lengthy discussion given here; but the discussion is also justifiable in its own terms for it illuminates some quite important aspects of Darwinian theory which might not otherwise be explicitly dealt with.

(48) The discussion will (not surprisingly) be quite complex and potentially confusing. Therefore let me outline the general structure in advance.

(49) The arguments all revolve around Spencer's unfortunate phrase "the survival of the fittest". I shall initially show how the phrase can be interpreted as an (approximately) correct, non-tautologous, (albeit *partial*) statement of T_d—which is, presumably, the interpretation Spencer intended. The first misconception which I consider is such that the phrase is still "correctly" interpreted, but, in a certain peculiar and austere sense, is labeled as tautologous *anyway*. Under the second misconception, there are two distinct ways in which the phrase can more or less correctly be interpreted as a *definition* of "fitness"; both of these interpretations are, of course, tautologous, but they are not statements of T_d and do not impinge upon its status. Under the third (and final) misconception, there is an interpretation of the phrase which is not strictly tautologous, but is not equivalent to T_d (and is, in fact, mistaken). This last error does involve an element of circular reasoning, and might therefore be still said to "smack" of tautology; in many ways it is the most pernicious misconception of all.

(50) In presenting this analysis I do not claim that my taxonomy is complete or unique. In practice, various combinations and permutations of the errors identified below may well be simultaneously present in any single author's treatment; and, of course, there may be new errors which I am unwittingly originating, in carrying out this very analysis.

5.1.1 On "survival"

(51) To analyse the arguments effectively, it is first necessary to distinguish two quite separate notions of "survival":

- **I-survival:** The survival of individual organisms.
- **L-survival:** The survival of organism lineages (in particular, S-lineages).

(52) I-survival obviously does not refer to survival in any absolute sense: individual organisms are essentially mortal, and have a finite lifetime—it is hardly meaningful to speak of individual organisms "surviving" without qualification.[8] However, some organisms do survive *longer* relative to others. More generally, there is potentially a valid notion of "mean I-survival"—i.e.

[8]Admittedly, it is a moot point whether this is true of *unicellular* organisms; but I shall not pursue that further here.

that a certain "kind" of organism may, in given conditions, show a consistent distribution of values for I-survival. Furthermore, if (and only if) there exist *heritable* characteristic(s) distinguishing such different "kinds" then they can serve to differentiate S-lineages. In that case coherent S-lineages, exhibiting distinctive (statistical) distributions of I-survival, can be formed. This distribution of I-survival would be a characteristic of an S-lineage. In fact, (mean) I-survival can then be thought of as a crude or partial measure of S-value (crude because the latter depends, at least, on fecundity as well as mortality); in particular it will generally be true that the greater the value of (mean) I-survival then the greater the S-value of the corresponding S-lineage.

(53) I emphasize that (mean) I-survival (as with S-value proper) is defined as an objective characteristic of an S-lineage in given conditions, which can (in principle) be evaluated independently of any prior knowledge of the *outcome* of any associated selection process.

(54) L-survival *does* (potentially) refer to "absolute" survival—in the sense that organism lineages can (apparently) survive indefinitely long.

(55) L-survival may also be related to S-value, but not in the relatively direct way which holds for I-survival. Let us suppose that, for *independent* reasons, we believe that two S-lineages will give rise to a selection process in specified conditions. Then we can infer that whichever S-lineage is the eventual L-survivor must have the greater value of S-value. Note carefully that this inference is valid if and only if we *already* know that we are dealing with a selection process.

(56) It should be clear that I-survival and L-survival are not the same thing. While they *may* be related this relationship is a contingent one; it would not hold if, for example, among the organisms being studied, there were no inheritable characteristics which were well correlated with I-survival; or if the S-lineages distinguished by different values of (mean) I-survival were not actually in competition with each other etc.

5.1.2 What Spencer Meant To Say?

(57) I suggest that the only correct interpretation of Spencer's phrase (which is not *necessarily* the interpretation Spencer himself intended) is the following: we interpret "survival" as L-survival (i.e. survival of (S-)lineages); we interpret "fitness" as *S-value*; "the survival of the fittest" is then at once the assertion that:

- There exist lineages which may be distinguished by inheritable characteristics (S-lineages).

- At least some of these S-lineages interact with each other in such a way as to give rise to a selection process.
- The eventual outcome (L-survival) of this process is then quasi-deterministically related to the relative S-values ("fitness") of the participating S-lineages: hence "survival of the fittest".

(58) As it stands this is essentially just a claim that processes of natural selection do, in fact, take place in the biological world. As such, it encompasses just the *inheritance* and *selection* theses of my T_d. It is then a correct, although very incomplete, statement of T_d. It is certainly *not* tautological.

(59) Again, it must be emphasized that this non-tautologous formulation relies on the fact that S-value (fitness) is not *defined* by S-lineage "survival"—rather it is, in principle, something that can be assessed of an S-lineage isolated from the selection process, but which is then *predictive* of the outcome of that process.

(60) However, confusion might arise in cases where, for *independent* reasons, one *already* believes that the displacement of one S-lineage by another is, in fact, a case of selection: in that scenario one can validly, and non-tautologically, infer that the surviving S-lineage must have had the greater S-value.

(61) Essentially this (correct) interpretation has been commonly identified in the literature—for example by Medawar (see Moorhead 1967, p. 12), and Hodge (1983, p. 58).

5.1.3 Misconception 1: Logic

(62) As detailed in the previous section, once the premises for natural selection are granted (and this is an empirical question) then the outcome—"the survival of the fittest"—is assured. That is, if we adopt these premises as *axioms*, in the sense of a formal logical system (i.e. they are taken to be "true" by definition), then "the survival of the fittest" becomes a theorem of the system, which is to say, in the strict terminology of formal logic, a *tautology*.

(63) This is, of course, a technically valid point; but it can hardly be called a *criticism*. It amounts to interpreting Spencer's phrase *only* as the (necessarily "tautological") conclusion of a certain deduction—rather than as an implicit assertion of the truth of the premises which lead to that conclusion. This is at best pedantic, at worst misleading. It is equivalent to saying that $E = mc^2$ (say) is a tautology—given the relevant properties of E, m and c. As Maynard Smith has put it,

> Of course Darwinism contains tautological features: any scientific theory containing two lines of algebra does so...
>
> Maynard Smith (1969, p. 85)

(64) This is such a peculiar misconception that it seems difficult to believe that it should genuinely arise. In practise I suggest that it does not normally arise in isolation, but may be combined with one of the other distinct misconceptions yet to be described. Having said that, there *is* at least one case where this misconception seems to have been uniquely involved:

> The notion of natural selection depends on the empirically verifiable observation that offspring on the average resemble their parents more closely than they do the other members of the population, that individuals are not all the same; that all environments are not the same. Concepts such as natural selection by the survival of the fittest are tautologous; that is, they simply restate the fact that only the properties of organisms which survive to produce offspring, or to produce more offspring than their cohorts, will appear in succeeding generations.
>
> Eden (1967, p. 5)

Eden does seem to use the correct interpretation of Spencer's phrase (notwithstanding the fact that he immediately goes on to use "survive" in the sense of I-survival rather than L-survival); but insofar as he describes it as a tautology he merely seems to mean that *any* valid deduction ("restatement") from true premises is a "tautology". While formally correct, the observation does not add anything except, possibly, confusion.

(65) Consider also, the following comment from Dawkins:

> Biologists thought they needed a word for that hypothetical quantity that tends to be maximized as a result of natural selection. They could have chosen 'selective potential', or 'survivability', or 'W' but in fact they lit upon 'fitness'. They did the equivalent of recognizing that the definition they were seeking must be 'whatever it takes to make the survival of the fittest into a tautology'. They redefined fitness accordingly.
>
> Dawkins (1982, pp. 181–182)

I suggest that what Dawkins means here is that fitness can be (indeed has been) defined as whatever it takes to make "the L-survival of the fittest" into a logical consequence of the existence of heritable fitness variations and competition. I should emphasize that, in context, Dawkins is *not* suggesting that this "tautology" can be translated into any *criticism* of Darwinism; but, as with Eden's version, I still find the reference to "tautology" to be confusing and gratuitous.

5.1.4 Misconception 2: Words

(66) Spencer's phrase does reduce strictly to a tautology if "fitness" is equated with "survival"; that is, the phrase is read as a *definition* of fitness (and definitions are, of course, a paradigmatic case of tautology). This formulation of the tautology argument is the most common; a good example would be that of Popper (1965, pp. 241–242).[9] As Dawkins puts it, this kind of argument is "a remarkable example of the elevation of words above their station" (Dawkins 1982, p. 181).

(67) This misconception leads to tautology regardless of whether we take "survival" to mean I-survival or L-survival. Both cases are conceptually possible, although they have quite different flavours. In general, writers suffering from this misconception are not clear about which sense they intend. In fact, the most likely scenario may be a failure to distinguish that there *are* two possible, but distinct, strictly tautologous interpretations—for if that fact is once recognised, the possibility of a non-tautologous (and correct) interpretation more or less immediately presents itself.[10]

(68) The first distinct case of this misconception is this:

> **Case 1:** *the I-survival of the I-survivors*

This amounts to defining fitness as (mean) I-survival.

(69) Now this interpretation is not "incorrect". As already discussed above, I-survival is indeed a possible, though extremely crude, measure of S-value; and "fitness" can be (and commonly is) interpreted as synonymous with S-value. So it is not entirely unreasonable to define fitness as (mean) I-survival (other things, particularly fecundity, being equal).

[9] However note that Popper later repudiated this analysis, as I discuss in section 5.2.

[10] There may be a lingering misconception that all tautologies are equivalent, so that the possibility of "distinct" tautologies cannot arise; this is not the case. All tautologies have the same *truth* value (namely, unconditionally "true"), so that they are *logically* equivalent—but this is not at all the same thing. "Cats are a kind of domesticated feline" and "π is the ratio of the circumference to the diameter of a circle" are (*qua* definitions) two *different* tautologies.

(70) But of course, under this interpretation, the phrase is no longer an expression of (the theory of) natural selection—it is merely a preliminary definition. Pointing out its tautologous nature cannot impinge at all on the status of T_d.

(71) The remaining variant on this misconception is:

Case 2: *the L-survival of the L-survivors*

This amounts to defining fitness as L-survival.

(72) Unlike case 1, this is hardly even a coherent definition. Since L-survival is (at any given time) a binary valued quantity, it is at least peculiar to equate it with "fitness" which, on any common sense interpretation, should be continuous valued.

(73) But let us stretch this point, for the time being. We *could* consider the phrase as equating fitness with L-survival, *regardless of whether natural selection is known to be operational.* But, as far as I am aware, this would be a usage of "fitness" which has never been seriously proposed, is counter-intuitive, and would be of no apparent utility.

(74) This leaves only the possibility that we consider the phrase as a definition of fitness *only in cases where we have prior, independent, knowledge of the operation of natural selection.*

(75) Well, in this case we cannot say it is positively incorrect—the operation of selection guarantees precisely that L-survival *will* be related to S-value (the "normal" meaning of fitness), as already discussed for the correct, non-tautologous, interpretation of Spencer's phrase. At this point we are back to a similar situation to that obtaining with Case 1: the interpretation can "reasonably" be adopted, but it is no longer an expression of (the theory of) natural selection—it is merely a preliminary definition (and a rather confusing one at that). Adopting this definition, we would then have to introduce some additional term other than fitness (S-value perhaps?), whose definition would *not* be already contingent on the outcome of selection, before we could even formulate T_d properly. But, in any case, we again conclude that pointing out the tautologous nature of this interpretation cannot impinge at all on the status of T_d.

5.1.5 Misconception 3: Adaptation

(76) The final misconception which arises does not strictly involve a tautology, and is only incidentally inspired or supported by Spencer's phrase. However, it does involve a degree of "circular" reasoning, is sometimes *said* to be tautologous, and is commonly associated with the strictly tautologous misconception(s) of the previous section. It is therefore appropriate to consider it here.

(77) I consider this misconception to be the most significant and pernicious of all, and will discuss it in some detail.

(78) I shall present the "argument" in what I consider to be its plainest form, but caution that it is rarely if ever expressed in such explicit terms.

- T_d is proposed as a solution of P_d—i.e. as an explanation for the growth of adaptive complexity in the phylogenetic tree.
- For T_d to successfully solve P_d would require that it *predict* (stochastically or otherwise) the growth of adaptive complexity.
- At best, the only thing that T_d actually predicts is a growth of S-value in certain lineages.
- Thus T_d can be said to solve P_d only by (re-)defining adaptive complexity to be the same as S-value (fitness). This effectively uses a circular definition, which is just such that the problem to be solved (originally P_d) becomes simply "whatever problem *can* be solved (by T_d)".

(79) Arguments of this sort are tacitly involved whenever there is a debate about the relationship between "fitness" and "adaptation". Darwinism is first taken to be a theory of the growth of "adaptation" (informally conceived of in terms of adaptive *complexity*, or "fit" between organism and environment—which is to say *knowledge*); but it is then noticed that Darwinism *per se* can actually only explain the growth of "fitness" (in the sense of S-value); so it seems that it can work as an explanation only if adaptation is *defined* as equivalent to fitness (and we forget our original informal notion of adaptive complexity, or adaptation to an environment). This does not turn Darwinism into a tautology in any strict sense (though it does involve a kind of circularity); but if this step is taken then Darwinism loses its ability to solve the kind of problem we were originally interested in—for the terms of the problem no longer appear in the theory. It is thus greatly diminished in scope and significance.

(80) Popper raised essentially this problem in what was (as far as I am aware) his earliest consideration of the status of Organismic Darwinism—his Herbert Spencer lecture, *Evolution and the Tree of Knowledge*:

> ... survival, or success in the sense of an increase in numbers, may be due to either of two distinguishable circumstances. A species may succeed or prosper because it has managed, say, to improve its speed, or its teeth, or its skill, or its intelligence; or it may succeed or prosper merely because it has managed to increase its fecundity. It is clear that a sufficient increase in fecundity depending fundamentally on genetical factors, or a shortening of the period of immaturity, may have the same survival value as, or even a greater survival value than, say, an increase in skill or in intelligence.
>
> ... But be this as it may, it should be possible, I think, to [measure] the *success in the adaptation of the individual organisms* of a species ...
>
> Without some distinction such as this ... we are liable to lose sight of the original problems of Lamarck and Darwin, and especially of the explanatory power of Darwin's theory ...
>
> Popper (1961, pp. 271–272)

(81) Popper is essentially pointing out that the temptation to equate adaptation (or adaptive complexity) with fitness (or S-value) must be resisted at all costs, for otherwise we lose contact with the problems we wish to solve. Lewontin (1978) has discussed this problem in very similar terms. Hull has also recently made much the same point, concluding that the requirement to identify adaptive complexity (which he actually calls "fitness"!) independently of S-value (which he terms "differential perpetuation") cannot be circumvented "without evolutionary theory degenerating into an empirically empty formalism" (Hull 1980, pp. 318–319).

(82) Now I have, indeed, been careful not to define adaptive complexity in terms of S-value (or fitness). Granted, I have not attempted any formal or detailed definition of adaptive complexity; and I have particularly eschewed any attempt to establish a metric for it. But, as already discussed in section 3, this vagueness is not untypical in the field; and I would argue that my general formulation in terms of *inborn knowledge* (Popper 1961, pp. 258–259) is still a more definite ontological commitment than is usual.

(83) It must be repeated that misconception 3 does not strictly involve a tautology. It is in this light that we must read Hodge's (1983, p. 58) claim that it is "a mistake to defend natural selection against the tautology objection by proposing criteria of fitness independent of reproductive success". I suggest that Hodge is here referring only to the *strictly* tautologous arguments already discussed (particularly the two cases of misconception 2), and *not* to misconception 3, which has quite a separate character. Dunbar (1982, p. 10), on the other hand, *rejects* the argument that T_d cannot be tautological because it is empirically testable (i.e. the kind of argument offered by Hodge), saying that "this claim misses the point entirely". I contend that Dunbar is effectively taking up a position precisely complementary to that of Hodge, confining his attention exclusively to misconception 3 and ignoring or dismissing misconceptions 1 and/or 2. My position is that, despite the apparent contradiction between Hodge and Dunbar, they are actually both correct, so far as they go—but they are discussing different problems (I consider Dunbar's analysis in more detail in paragraph 94).

(84) At this point my argument is that misconception 3 is mistaken in concluding that adaptation or adaptive complexity should, or must, be defined in terms of S-value (fitness). I therefore insist on retaining essentially independent definitions of adaptive complexity and fitness (and thus retain P_d in its interesting form). What then are we to make of the original criticism—that T_d cannot actually solve this problem, for it does not predict the growth of adaptive complexity (so-defined)?

(85) This brings us to the nub of the problem, which is to ask how much we can sensibly ask of a proposed solution of P_d. It seems to me that the error lies precisely in supposing that a solution must (or even can) take the form of some general theory which predicts a growth in adaptive complexity. This derives in part from a fundamental misunderstanding of what P_d actually says. P_d recognises that there has been a sustained growth in adaptive complexity, in at least some evolutionary lineages, and asks for an explanation of how this could be (preferably one which does not assume the pre-existence of an even more complex creator). It does *not* say that growth in adaptive complexity *must* occur (in general or in particular lineages); or that it had to occur in the particular way which it did; or that it must continue occurring. *But only the latter kinds of problem would call for a solution which incorporates a general prediction of growth of adaptive complexity.*

(86) We might characterise the general difficulty here as a supposition that P_d entails some kind of guaranteed, monotonic, "progress" (in adaptive complexity). It is a harking back to the "great chain of being". As Gould puts it: "The familiar iconographies of evolution are all directed—sometimes crudely, sometimes subtly—toward reinforcing a comfortable view of human inevitability and superiority" (Gould 1989, p. 28). The idea of necessary progress in evolution is so deeply entrenched that it is very difficult to free oneself of it. Not even Darwin himself was completely immune. Although he explicitly emphasized that the *absence* of "perfection" in biological organisms should be interpreted as positive evidence in favour of the operation of natural selection (at least as compared with a theistic theory), we still find him remarking, in the concluding pages of *The Origin*, that "... as natural selection works solely by and for the good of each being, *all corporeal and mental endowments will tend to progress towards perfection*" (Darwin 1859, Chapter XIV, emphasis added).[11]

(87) Once, however, we free ourselves from the preconception that organismic evolution is a story of necessary or inevitable "progress"—and excise this idea from P_d—then the way is open to consider T_d as offering at least a partial solution.

(88) Briefly, as has been commented upon several times, T_d cannot and does not predict the growth of adaptive complexity. However, given that adaptive complexity *has* grown, T_d can retrospectively offer a generalised explanation for it—namely that the growth of adaptive complexity "happens" to have been correlated with the growth of S-value—and, crucially, T_d can do this without postulating the existence of any "designer" (prior knowledge) directing the overall course of evolution.

(89) T_d in itself, does not explain why adaptive complexity should be correlated with S-value either in general, or in any particular case; not does it explain how (heritable) adaptive complexity can increase at all, in general or in any particular case. In addressing P_d it *does* entail that some (heritable) increases in adaptive complexity have occurred, and that, of these, at least some have been correlated with S-value; but it specifically *denies* that such increases in adaptive complexity have been "designed" or "anticipated", or that they have been *universally* correlated with S-value.

(90) Now, of course, T_d *does* predict more or less monotonic "progress" in one very specialised sense: increase in S-value (within some lineages). But that much is true of all systems where (S-lineage) selection processes arise, and does not impinge at all on the central problem of the growth of adaptive complexity. No doubt we can conduct a certain amount of evolutionary theorizing without ever referring to adaptive complexity: but we can certainly never solve P_d. The crucial extra step, which is rarely made explicit is to say that, given T_d, adaptive complexity can grow in evolutionary lineages if and only if at least some ("unjustified") increases in adaptive complexity occur, and, of these, at least some are correlated with a net increase in S-value (i.e. are selectively retained).

[11] See (Gould 1978, Essay 4) for a more detailed discussion of whether Darwin "really" endorsed the idea of necessary progress in evolution.

(91) Of course, the reasoning given here only works at all if it is accepted that an increase in adaptive complexity *might* be correlated with greater S-value. If I had defined adaptive complexity in some (strange) way which was intrinsically opposed to S-value (say, necessarily involving unconditional altruism on the part of the S-lineage) then the argument could not go through. But my actual definition—which corresponds to something like "inate knowledge"—does precisely have the characteristic that, *ceteris paribus*, it may be expected to be correlated with higher S-values. But the *ceteris paribus* clause is absolutely crucial here; to ignore or omit it would effectively mean a reversion to equating adaptive complexity with S-value, and thus defining away the real problem.

(92) None of this is to say that the growth of adaptive complexity cannot be explained (or even predicted); it simply claims that there cannot be any *general* theory of it (as always, this is just another way of denying the existence of a logic of induction). T_d asserts precisely that, for all *particular* historical cases of an increase in adaptive complexity, there is a *particular* explanation, involving an "unjustified" variation in adaptive complexity (which happened to be an increase) which was selectively favoured. It may or may not be possible to organise these particular, historical, explanations into a smaller number of more general cases: but I take the view that it will not be possible to translate these particular explanations, nor generalisations of them, into predictions for continuing growth of adaptive complexity into the future. The aggregation of all the particular cases (if such could be individually established) would then be the complete (historical) "explanation" of all growth of adaptive complexity in the biological world. But T_d does not assert that such growth had to occur, nor that it will continue into the future, nor even that it would necessarily recur on another "similar" planet.

(93) The confusions and misconceptions discussed here have centred on the distinction between "adaptation" and "fitness". For this reason I have tried to avoid these terms in my own general presentation of Darwinism: the only lingering remnant is the "adaptive" in "adaptive complexity". I have retained this in deference to the existing biological literature, but I suggest that it might actually be better to eliminate even this concession. In speaking of the "adaptation" of biological organisms it seems almost impossible not to think in terms specifically relating to their success in living and procreating—which is to positively invite a reversion to the relatively sterile concept of S-value.

(94) A particularly "good" example of this is Dunbar's analysis (Dunbar 1982). He is very clear about the *need* to distinguish "adaptation" and "fitness". Furthermore, he seems to adopt much the same kind of distinction as I have suggested above, interpreting adaptation in terms of "problem-solving" (Dunbar 1982, p. 11) (following Lewontin 1978, among others). He seems to recognise the essential *independence* of the two concepts when he cites Dobzhansky (apparently favourably) as saying that "we cannot draw inferences about fitness from a knowledge of adaptation, nor of adaptation from a knowledge of fitness" (Dunbar 1982, p. 14). However, he then goes on to explicitly deal with the alleged circularity of Darwinian explanations, in the following terms:

> The relationship between the concepts of adaptation and fitness might seem to confirm the worst fears of the anti-Darwinians. Each appears to depend on the other in a way which makes them virtually inseparable. It is, however, crucial to appreciate that they are not *definitionally* interdependent: adaptation is not *defined* in terms of fitness, nor vice versa. Adaptation (and hence reproductive success) is defined with reference to *individuals*, whereas fitness is defined with reference to genes and is thus a characteristic of populations.
>
> (Dunbar 1982, p. 16–17)

Dunbar here seems to suggest that the distinction between adaptation and fitness is (merely?) a distinction between properties of organisms and (consequent) properties of lineages. The best interpretation I can offer of this is as a somewhat tortuous reference to what I have called misconception 2 above—effectively a failure to distinguish I-survival and L-survival. Having thus retreated from the real issue—misconception 3—Dunbar finds that he must admit that a significant circularity may still remain in Darwinian theory. It is perhaps not surprising that he then resorts to the philosophical relativism of Kuhn and Feyerabend as his final defence of Darwinism against circularity. I, of course, take the view that such a conclusion is unsatisfactory and unnecessary—that the conceptual independence of fitness and adaptation (S-value and adaptive complexity) can and should be recognised, and this can be done without depriving Darwinism of its power as an explanatory schema. However, it does underline the point that the terminology of "fitness" and "adaptation" may be critically flawed. Thus, if one confines oneself to discussion in terms of (inate) *knowledge* instead of adaptation or adaptive complexity, it may be easier to remember that there is no *necessary* connection between these things and their retention or growth under natural selection. It should then be clear that any connection which may exist will have to be individually argued for in each particular case.

(95) In short, I propose that the term "adaptation" (and "fitness" too, for that matter) be henceforth considered harmful, and be avoided in formulating and presenting the theory of Organismic Darwinism.

5.2 ... or Metaphysics?

(96) As with the idea that Darwinism may be tautological, the assertion that it is essentially metaphysical has been more or less independently suggested (and criticised) by a number of different authors.

(97) The criticism of the so-called *adaptationist programme* by Gould and Lewontin is an example of this kind of argument (Gould & Lewontin 1979, p. 589). Indeed, Lewontin has explicitly claimed that "the adaptationist programme makes of adaptation a metaphysical postulate, not only incapable of refutation, but necessarily confirmed by every observation" (Lewontin 1977, as quoted by Maynard Smith 1978, p. 38).

(98) However, the claim put forward by Gould and Lewontin is not strictly that Darwinism *per se* is metaphysical. Indeed, they accept the empirical reality of natural selection; their argument is about which evolutionary phenomena are explicable in terms of selection. The adaptationist programme, which they criticise, presumes that *all* evolutionary phenomena (or organismic attributes) are a direct outcome of selection.[12] Conversely, it seems clear that Gould and Lewontin accept that what I have called adaptative complexity, where it exists, does demand an explanation in terms of selection; and, while such explanations may, individually, be almost impossible to test in practise, they are testable in principle, and are not therefore metaphysical. Thus, *in terms of the problem of adaptative complexity* (P_d), Gould and Lewontin appear to accept that T_d is not metaphysical, and, indeed, that it is the best theory currently available.

(99) Concerns of a similar sort to those raised by Gould and Lewontin have been independently discussed by various other workers. For example, they were prominent in the discussions at the *Wistar* symposium on "Mathematical Challenges to the neo-Darwinian Interpretation of Evolution" (Moorhead & Kaplan 1967). Although Popper was not present at this symposium, he was repeatedly cited (directly or indirectly) as the source for such concerns. Thus, for example, Medawar makes the following comment in introducing the symposium:

> Then there are philosophical or methodological objections to evolutionary theory. They have been very well voiced by Professor Karl Popper—that the current neo-Darwinian Theory has the methodological defect of explaining too much. It is too difficult to imagine or envisage an evolutionary episode which could *not* be explained by the formulae of neo-Darwinism.

[12] In this respect, the term "adaptationist" is unfortunate, as it invites a form of the tautology misconception 3; "selectionist" might be less prejudicial.

(100) Unfortunately, there are no detailed citations to original sources, and I am not aware of Popper ever having published *exactly* this criticism of Darwinism. In any case, my response to this argument is essentially as already discussed in the case of Gould and Lewontin: while T_d is, undoubtedly, difficult to test in respect of the evolution of specific complex adaptations, it is still testable in principle (i.e. is not metaphysical) and is the best theory currently available.

(101) However, Popper *has* published a slightly different argument for regarding Darwinism as metaphysical—or, more precisely, as a *metaphysical research programme* (Popper 1974*a*, Section 37). This arose (at least partly) because, as already noted in section 5.1.4, Popper's earliest considerations of the status of Darwinian theory suffered from a form of misconception 2, and he felt that the theory was therefore "almost tautological"; yet he also felt that, *despite this,* the theory had considerable explanatory power. Popper seems to have thought that this apparent contradiction might be resolved by regarding Darwinism as a metaphysical research programme. While I think his interpretation of Darwinism as tautologous was mistaken, I actually agree that, in a certain limited sense, it *can* usefully be regarded as metaphysical.

(102) Firstly, as discussed in (McMullin 1992*a*), I consider that it is not unreasonable to describe the *abstract* form of Darwinian theory, presented in that essay, as a metaphysical research programme in Popper's sense. It is not a scientific theory until the primitive entities (especially D-actors) are given some specific empirical interpretation. It must be emphasized that to view this admission (of the metaphysical nature of the abstract theory) as a *criticism* of any particular interpretation of the theory (such as Organismic Darwinism) would be to indulge again in a form of the tautology misconception 1. This, for example, is the only way in which I can understand one of Peters' purported criticisms of Organismic Darwinism (Peters 1976, p. 4), which is apparently based on its being a specific interpretation of the axiomatic Darwinism of Williams (1970).

(103) Having said that, it must be recognised that the abstract or axiomatic form of Darwinism is metaphysical in a deeper or more profound sense than the conventional abstract theories underlying all science. The general kind of problem which abstract Darwinism seeks to solve is the growth of knowledge; and its mechanism of solution entails a refusal to make predictions—it "works" (in the face of the impossibility of a logic of induction) precisely by *declining* to predict the growth of knowledge. This is a quite unique kind of abstract theory. It follows that, *even* when the abstract theory is interpreted in specific empirical terms (such as in the form of Organismic Darwinism) it can never predict the *future* growth of knowledge. We must say that, as long as such a particular interpretation of Darwinism is viewed as an *historical* theory of the *past* growth of knowledge it is perfectly testable (in terms of "retrodictions") and scientific; but if it is mistaken for a "universal law" of the growth of knowledge, then, since it cannot predict such growth, it must be treated as metaphysical. The (metaphysical) position adopted here is, of course, that no "universal law" of the growth of knowledge exists.

(104) It can be seen that this argument for viewing even Organismic Darwinism (as opposed to Darwinism in the abstract) as metaphysical hinges on its (in)ability to predict the future growth of knowledge, or adaptive complexity; thus it is closely related to what I have labeled tautology misconception 3. This is brought out clearly by considering Popper's most substantive presentation of this viewpoint:

> ...assume that we find life on Mars consisting of exactly three species of bacteria with a genetic outfit similar to that of three terrestrial species. Is [organismic] Darwinism refuted? By no means. We shall say that these three species were the only forms among the many mutants which were sufficiently well adjusted to survive. And we shall say the same if there is only one species (or none). Thus Darwinism does not really *predict* the evolution of variety. It therefore cannot really *explain* it. At best, it can predict the evolution of variety under "favourable conditions". But it is hardly possible to describe in general terms what favourable conditions are—except that, in their presence, a variety of forms will emerge.
>
> And yet I believe I have taken the theory almost at its best—almost in its most testable form. One might say that it "almost predicts" a great variety of forms of life. In other fields, its predictive or explanatory power is still more disappointing. Take "adaptation". At first sight natural selection appears to explain it, and in a way it does; but hardly in a scientific way. To say that a species now living is adapted to its environment is, in fact, almost tautological. Indeed we use the terms "adaptation" and "selection" in such a way that we can say that, if a species were not adapted, it would have been eliminated by natural selection. Similarly, if a species has been eliminated it must have been ill adapted to the conditions. Adaptation or fitness is *defined* by modern evolutionists as survival value, and can be measured by actual success in survival: there is hardly any possibility of testing a theory as feeble as this.
>
> Popper (1974*a*, pp. 136–137)

(105) As already mentioned, Popper had earlier (Popper 1961) recognised the danger of misconception 3, and the consequent need to keep adaptation and fitness (selection) clearly separated; but in this later consideration of the problem he seems to have decided that such separation cannot be achieved. Viewed as a putative theory of the growth of adaptive complexity (i.e. of the evolution of a "rich variety" of more or less "well adapted" forms), Darwinism then becomes irrefutable (metaphysical), for, no matter how little the variety or adaptation we observe, it *could* still result from Darwinian processes.

(106) Clearly, I agree with the essence of Popper's argument; the difference is that instead of discarding Darwinism, I discard the idea that adaptation and fitness be defined in terms of one another. Granted, Darwinism cannot then "predict" the growth of adaptation or adaptive complexity. But, once adaptation is interpreted in terms of *knowledge* this becomes precisely consistent with Popper's general evolutionary epistemology, and is seen as the strongest kind of theory we can expect. And, as a theory of the *historical* growth of adaptive complexity in the biological world, it is perfectly scientific.

(107) I have expended some effort in considering Popper's criticism of the scientific status of Darwinism because I think it relates to some difficult and important issues, which are relevant objectives of this article as a whole. However, it must be added that Popper himself subsequently modified his views significantly (Popper 1978). In particular, Popper recognised and corrected the error implicit in tautology misconception 2, and accepted that (Organismic) Darwinism can be so formulated that it is definitely not tautologous. Unfortunately, he then goes on to say that, in such a form, it is literally false (has been refuted). At first sight this is an even worse accusation than the original assertion that the theory was (almost) tautologous and/or metaphysical. However, Popper's reformulation is the strong one that *all* aspects of the phylogenetic tree are the outcome of cumulative natural selection; such a strong claim, which is essentially equivalent to the adaptationist programme criticised by Gould and Lewontin, is, indeed, false. Popper does not explicitly consider the lesser (but still non-tautologous) claim that all instances of the growth of adaptive complexity (i.e. my P_d) are the outcome of cumulative natural selection (i.e. my T_d), and certainly does not argue that *this* formulation has been refuted; so there is, in fact, no conflict with the views I have expressed.

(108) In conclusion, let me reiterate that I consider T_d, viewed as an historical theory of the growth of adaptive, organismic, complexity, to qualify as a good scientific theory— *despite* the fact that actually testing it in specific cases is enormously difficult. More specifically, T_d qualifies as scientific according to *Popper's* criteria. I emphasize this last point because, even though Popper might be called a "naive" falsificationist with respect to the *logic* of (scientific) theories, he has always been a critical falsificationist with respect to the *methodology* of actually carrying out scientific research. This point has, apparently, been commonly misunderstood or misrepresented (Magee 1973, pp. 23–24; Popper 1974*b*, pp. 981–984). The relevance of the distinction here is that it can be a perfectly consistent Popperian position to assert that Darwinism is scientific by virtue of its (logical) falsifiability, *even* if such falsifiability is *methodologically* almost impossible to exploit (i.e. tests which could falsify the theory may be perfectly conceivable, yet wholly impractical).

6 Epilogue: Artificial Life

> ... I do not really believe that we shall succeed in creating life artificially; but after having reached the moon and landed a spaceship or two on Mars, I realize that this disbelief of mine means very little. But computers are totally different from brains, whose function is not primarily to compute but to guide and balance an organism and help it to stay alive. It is for this reason that the first step of nature toward an intelligent mind was the creation of life, and I think that should we artificially create an intelligent mind, we would have to follow the same path.
>
> Popper & Eccles (1977)

(109) My interest in Darwinian theory stems from attempts to realise something like a spontaneous growth of knowledge, by Darwinian means, in *artificial* systems—a problem which is now generally captured under the rubric of *Artificial Life* (Langton 1989). I may note in passing that, as the quotation above indicates, Popper deserves to be recognised as at least one of the founders of this field; indeed, as early as 1961, in his Herbert Spencer lecture *Evolution and the Tree of Life*, Popper presented a moderately elaborate, if schematic, discussion of the possible evolutionary growth of knowledge of a strictly artificial (robotic) system (Popper 1961).

(110) So, in this epilogue I would like to briefly wave my hands in the direction of some recent work in Alife, and the problems considered in the article.

(111) First, let me note that any attempt to realise the growth of adaptive complexity, via (genuinely) Darwinian processes, in artificial systems, could, in itself, represent a further kind of *test* of T_d. While such tests could hardly provide a strong *refutation* of T_d (due to the necessarily limited scale of artificial systems compared to biological evolution) the *successful* demonstration of significant, spontaneous, growth in adaptive complexity in artificial systems, were it to be achieved, might still represent a significant *corroboration* for T_d.

(112) I hasten to add that I do not consider that the achievements to date in this direction should be overrated—but there are some intriguing straws in the wind, at least.

(113) Among the best known efforts in this direction are the *BioMorphs* of Richard Dawkins (1986) and the *Tierra* system of Tom Ray (1992). However, the *BioMorphs* system is limited to static (albeit very "lifelike") *images* of artificial "organisms"; and while *Tierra* does involve *dynamic* entities (fragments of computer code) their environment is so alien to our normal experience that it is very difficult to assess whether "knowledge", in anything like its Popperian sense, is being created or discovered.

(114) In my view then, the most impressive work of this sort to date is that on evolving "virtual creatures" by Karl Sims (Sims 1994*b*, Sims 1994*a*). Sims first constructed a "virtual" (computational) environment which reasonably faithfully models normal, three dimensional, Newtonian space—optionally including a viscous medium and/or a uniform gravitational field. He was able to embed within this environment virtual or simulated creatures. These creatures are "grown" from "genetic" *descriptions*. They consist of roughly cuboid components, of various sizes, jointed together in various ways, and controlled by a distributed artificial "nervous system". Starting from randomised descriptions, and using artificial selection (i.e. controlled by a predetermined "fitness function") Sims has been able to demonstrate the spontaneous evolution of creatures exhibiting swimming, walking, and jumping behaviours; creatures which can track an environmental stimulus; and creatures which can compete (in very stylized combat rituals) for possession of "resources".

(115) Furthermore, in achieving these behaviours, many of Sims' creatures exhibit morphologies and organisation that would undoubtedly be called "adaptations" if found in biological organisms. I suggest that there is at least a *prima facie* case for claiming that these creatures have spontaneously acquired significant *knowledge* of their world; that this growth of knowledge was by fundamentally Darwinian means; and that this lends some degree of independent corroboration to conventional, biological, Darwinism. At the very least, I suggest that this work demonstrates that there is more to Darwinism than a tautology!

(116) Having said that, there are also limitations in this work. For example, reproduction and development do not take place within the virtual environment, thus limiting the opportunities for anything like somatic time learning.

(117) But, more importantly I think, selection is driven by a predefined, and externally imposed, evaluation function. I suggest that, for this reason, evolution in this system may be effectively limited to the refinement of what Popper (1961, p. 275) called the *skill-structure* of the creature. The higher level *aim-structure* is effectively determined by the imposed evaluation function (as "walk", or "swim", or "fight" etc.). Popper conjectured that mutations in the *aim-structure* might, in the biological world, explain certain very significant long term phylogenetic trends. In my terms, this seems to offer something like a mechanism for a long term correlation between knowledge and S-value—which is to say, a potential explanation for major episodes of Darwinian growth of knowledge. But it seems to me that, for the moment, this possibility is effectively closed off in Sims' system.

(118) The central difficulty in all of this is, of course, that the empirical phenomena with which T_d deals are of such a scale in both time and space that they are very difficult to subject to any severe test, even via computer simulations. I remain convinced that T_d is, in principle, testable; but I will leave the last word with Popper again:

> ... we have to add that the phrase *in principle* is a very important restriction. Neither Darwin nor any Darwinian has so far given an actual causal explanation of the adaptive evolution of any single organism or any single organ. All that has been shown—and this is very much—is that such explanations might exist (that is to say, that they are logically possible).
>
> Popper (1961, p. 267)

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Presented at the *Annual Conference on the Philosophy of Sir Karl Popper*[13], London School of Economics, March 9th, 1996. The article title is a somewhat strained allusion to (Dijkstra 1968)—see the relevant entry in *The Jargon File*[14] for more information. This article is derived in part from material first presented in a much more extensive technical report (McMullin 1992*b*).

[13] `http://www.eeng.dcu.ie/~tkpw/popconf/popconf.html`
[14] `http://beast.cc.emory.edu/Jargon30/JARGON_C/CONSHARM.HTML`

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