DOMAINS, DEDUCTION, THE PREDICTIVE METHOD, AND DARWIN

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ABSTRACT: The predictive (hypothetico-deductive) method can be viewed as an inverse of the deductive method; each method is important in both mathematics and the natural sciences. The deductive method has certain advantages but often can't be used. The domain of a proposition is a critical aspect of all inference but is rarely recognized. The paper gives an explicit and semi-formalized deduction of competition, natural selection, and evolution from a set of assumptions and definitions. The deduction may be of the form that Darwin originally used and is meant to correspond to causal processes in nature. The deductive argument was critical for both Darwin and Wallace in their discovery of evolution by natural selection. The domain of the theory includes phenomena not ordinarily associated with it. Briefer treatments are given of the justification of deduction and the nature of truth. A new interpretation of probability avoids the problems of existing theories and gives a natural justification for the principle of insufficient reason.

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It is commonly believed, especially since Popper (1959), that the propositions of science are hypotheses which can be falsified by disproof of their predictions, but which cannot be proven. Conversely, the propositions of mathematics are believed to be provable statements whose empirical consequences are irrelevant to their truth. I claim to show that both methods of justification are important to both subjects and that evolutionary biology in particular has a strong deductive core.

The name "hypothetico-deductive method" is unwieldy and easily lends the confusion that it is the one appropriate way to use deduction in science. I therefore propose to rename it the <u>predictive method</u>, after its prime characteristic; the name also contrasts suitably with the deductive and inductive methods.

In the predictive method, one or more general propositions G deductively generate, perhaps with the help of additional information, one or more predictions P:

$$G \rightarrow P. \qquad (1)$$

If P is untrue, G is untrue. (I ignore complications such as contextual uncertainties.) But if P is true, G may still be false.

In the deductive method, one or more assumptions A, believed true in some domain, deductively generate one or more general propositions G:

$$A \rightarrow G. \qquad (2)$$

If A is true, G is true. But if A is untrue, G may still be true; some true set of assumptions A' may generate G. This is the reverse of Popper's paradigm: proof is possible and disproof (by disproving A) is not (Van Valen, 1975b).

The symmetry between these two methods encompasses similarities as well as contrasts. Because more than the hypothesis in question is necessary to deduce a prediction, a disconfirming instance is still, at least formally, compatible

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with the hypothesis. And because the domains of assumptions are often poorly delimited, the domains of a formally proven hypothesis may be narrower than supposed or even empty. Moreover, in most or all cases the predictions or assumptions will, on analysis, prove to be hypotheses and so in need of confirmation themselves.

Domains

By the domain of a proposition I mean the set of circumstances (phenomena, relations, and entities, realized and potential) to or in which the proposition would be applicable if true. This is a slight extension of the usage in the mathematical theory of functions[1].

The domain of a theory is of utmost importance but is frequently overlooked. The continuing controversy in ecology on the principle of competitive exclusion is of this nature. The principle itself, that two species whose density is regulated by the same processes cannot coexist indefinitely, is analytic and follows necessarily from less controversial assumptions[2]. Yet Cole (1960), Ayala (1969 but not later) and others have claimed it is false. The interesting point, however, is not truth or falsity (no one claims that every pair of species exclude each other) but the extent of the domain of the principle: how broadly the assumptions apply in the real world and whether different and more general assumptions would give the same result. In general it is difficult to show that one has necessary conditions for a deductive conclusion and thus has bounded the domain. This is simply because all possible conditions of all possible variables must somehow be examined before one knows which are irrelevent, and the examination must be of all combinations because of possible interaction.

The union of the domains of the propositions of a theory (a theory being a set of conceptually related explanatory propositions), or of a set of theories, is itself a useful concept and may be called an <u>inclusive domain</u>. Inclusive domains are the subject-matter of academic disciplines and sub-disciplines; their boundaries and interrelations are poorly understood even descriptively. Inclusive domains change, branch, and disappear as theories change. It is unclear to what extent the boundaries of inclusive domains are artificial and to what extent they reflect natural subcontinuities in the abstract space of possible subject matter (Van Valen, 1972).

Mathematics

Deduction is characteristic of mathematics to such an extent that the non-deductive aspects of mathematical research (as in the generation of theorems or proofs, or in the bounding of domains) are rarely made explicit. Its lack of recognition in "empirical" science may be related to the frequent uncertainty as to the truth or the domain of assumptions.

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[1]In mathematics, a domain is the set of values over which an independent variable may range; or, from a different viewpoint, a domain is a set (such as an independent variable) that is mapped into another. The domain of an existential proposition I take to be the universe in which existence is proposed: e.g., in "There are 47 men in Burwash Landing, Yukon" it is Burwash Landing. Even an intensionally defined class can be spatiotemporally restricted if its domain is. Thus, the class of all first-order tributaries is only accidentally spatiotemporally restricted if rivers (like pendulums) occur only on Earth, but it is inherently so restricted if the domain being considered is the Yukon River watershed or even the planet Earth.

[2]For example, S.A. Levin (1970) recently derived a generalization of the usual principle.

The use of the predictive method[3] in mathematics is examplified by the classic treatment of Euler's polyhedral theorem by Lakatos (1963-1964). The theorem, which relates the numbers of sides, edges, and vertices of polyhedra, is proved for a poorly bounded domain. Hypotheses as to the extent of this domain are, in many cases, refuted by counterexamples just as is done in natural science. Moreover, it has never been possible to fix the boundaries of the domain deductively. The domain, after nearly two centuries, is still indefinite and epistemologically depends on the imagination of discoverers of new solids whose boundaries do or do not conform to the theorem. Knowledge of the truth or domain of a hypothesis in natural science depends in the same way on the imagination of those who test it.

Deduction leads to quasicertainty, but quasicertainty of exactly what? This is the problem of domains and must be assessed independently of the deduction. Sometimes the assessment is easy, as for the area of a triangle in plane geometry (even though an otherwise plane surface with a bump would sometimes, but only sometimes, have counterexamples, so for full generality the assessment even here is not quite trivial). But sometimes it is in practice insoluble.

I say quasicertainty rather than certainty because we must somehow come to know the rules of deduction just as we come to know anything else. We learn them by experience, and experience is fallible, whether or not we can think of alternatives. What is unthinkable? Before we can answer that we must have a basis to judge.

Through history there has been a progressive decline in the kinds of knowledge that most thoughtful people have believed to be _a priori_, knowledge that is immediately apparent as true and so is not in need of justification. Even after Copernicus most people found it an _a priori_ truth that direct personal observation was the best way to judge natural phenomena such as the movement of the sun. The validity of divine revelation was also apparent, especially but not exclusively as recorded in (mutually and even internally inconsistent) sacred books. The relationship of deductive geometry to the real world has come to be an empirical question, despite Kant's efforts. The justification of basic ethical principles is now commonly, and I believe correctly, believed impossible. The same is true for the justification of induction or, as can be shown to be equivalent, the justification of the methodological principle of simplicity, of not assuming unjustified complexity. (This principle is often misstated as an ontological assumption that reality is simple.)

What is now left is deductive logic and the constructs formed from it. Here, we are told, we can indeed pluck foundations from the naked air. This view remains widespread probably because basically different alternatives don't exist, although there are variant systems in more or less small respects. Other _a priori_ beliefs have proved unfounded and even false; it shouldn't then be surprising that one may be true. But how do we know this to be the case, even if it is? How do we know that an empirically better but basically different form of deductive logic will never be discovered?

A deductive justification of induction lies at the rainbow's end; we must rather analyze the nature of our experiences to see why we accept deduction. Why do we believe that the proposition "Deduction gives certainty" is itself

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[3]One classical method of deductive proof can be confused with the predictive method. This involves assuming that the general proposition is false and deriving a contradiction to an assumption. Symbolically,

$$\bar{G} \rightarrow \bar{A}, \tag{3}$$

but A, therefore G. This is a restatement of (2), not of (1), and gives proof (or disproof) rather than requiring empirical test.

certain? That the necessity given by deduction is necessary, and that its analyticity is analytic? These are central questions. It seems likely that the certainty given in each case by deduction is confused with a certainty of justification of the method itself. The points are totally distinct, in exactly the same way as the certainty given in each case by divine revelation differs from the certainty of revelation in general. Russell (1948) noted that the general principles of deduction are less certain than their instances; we learn the former, and justify them, by simple induction from the latter. Thus our knowledge of deduction would seem to be on just the same footing, both ontogenetically (in our learning) and epistemically, as our knowledge of the physical world. Both are justified inductively or not at all.

Perhaps acceptance of deduction, and of epistemic simplicity, has been built into us by natural selection. If so, it corresponds to reality, or to some part of reality, but we can know this too only by inference. Without sure primitives we lack sure knowledge, but it is better to accept this situation than to create certainty out of our own imaginings.

Natural Sciences

That deduction is a common and important method of justification in the natural sciences is also little appreciated. Dorling (1973) has recently given an account of one kind of deduction, with examples from physics, although he followed tradition and called it "demonstrative induction". An earlier example is the establishment of the atomic weight of all samples of argon from measurement of one sample and a proposition on the uniformity of atomic weights in general. The use of the word "induction" refers to the fact that synthetic general propositions about the real world can be justified in this way partly on the basis of single instances of the general propositions. But the method is deductive, as Dorling himself realizes, and the distinction from other deductions is misleading.

The domain of the propositions of the deduction may clarify the situation. The truth of any proposition is relative to its domain. Ontologically the domain may be universal or restricted, its boundaries sharp or fuzzy, and epistemologically its boundaries may be known or unknown. The domain may contain empirical or analytic elements, discrete or continuous. The intersection of the potential domain of a proposition with reality (including conceptual reality for hypothetical or counterfactual propositions, as about the future or mermaids), i.e. that part of the potential domain in which the proposition is true, may be called its <u>domain of truth</u>. The domain of truth may be empty, in which case the proposition is universally false.

I take it as axiomatic that the conclusion of a valid deduction has been shown to be true only with a probability[4] equal to the product of the

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[4]In a deterministic universe a claim of randomness is a badge of ignorance. To the extent that the world is ontologically deterministic, all probabilities are conditional, being relative to the state of knowledge of an observer. The probability of the same event can differ for observers with different knowledge. For an observer with complete knowledge, the probability is either 0 or 1. A propensity, then (such as my propensity to die next week), is an empirical and objective value summarizing the likelihoods of the various possible causes (often not separately identifiable in practice, but not an arbitrary reference class) of a given outcome, relative to existing knowledge. In turn, these likelihoods are related to other likelihoods of the relevant causal network, and their ultimate estimation (which is often at the stage of the outcome itself) is by the observed frequency of sufficiently similar events when relevant

probabilities of truth of each premise and of the method of inference used[5]. And usually the conclusion can be shown to be true only within the intersection of the domains of truth of the premises. But with "demonstrative induction" the domain of truth of the conclusion can be the same as that of one premise, and much wider than that of another. This is because the more general premise is compound and incorporates the other by substitution.

Such inductive deduction is by no means the only kind of deduction used for justification of general propositions in the natural sciences. Another important kind, perhaps even more prevalent, is based on the isomorphism between causation and part of deduction. Causation is the image of implication in this mapping. It is no coincidence that both implication and causation are represented in ordinary language by "if. . . then;" in fact it is why mathematics can represent the real world dynamically as well as statically, in process as well as in form. There are of course deductions that do not involve causality, and because of the correlation-causation fallacy an extensionally sufficient condition need not be a cause even in a causal theory (heart sounds don't cause movement of the blood).

For housemice and probably other species, high density causes social stress, which causes low reproduction, which causes a decrease in population

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variables are controlled in a large enough sample. This view of probability, which incorporates aspects of the propensity, subjectivist, and frequentist views into an outlook based on the availability of information, may be called the <u>epistemological interpretation of probability</u>. It gives an immediate derivation for the much maligned principle of insufficient reason: the limit of any probability as knowledge approaches 0 is the reciprocal of the number of possible alternatives.

[5]Deduction is usually thought to be nonprobabilistic, despite the existence of probabilistic logics. But, like all claims to certain knowledge, deductive certainty is an idealization never fully attained. Truth of the conclusion is always conditional on truth of the premises and on validity of the inference.

The probability of truth of a proposition, relative to a random element of its domain, has four aspects: (1) the proportion of the domain of the proposition that is its domain of truth, (2) the accuracy of knowledge of this proportion, (3) the sharpness of the boundary of the domain of truth, and (4) the accuracy of knowledge of the criteria for bounding the domain of truth. "All men have blue eyes" is from the usual viewpoint false, but in the sense that it has a nonempty domain of truth it is less false than "All men have blue wings". One way to bound the domain of truth of the former proposition is to use the set of men lacking only brown pigment in their irises. In this restricted domain it is probably true that all men have blue eyes. In ordinary deduction the only aspect of probability is whether the proposition is true or false, which is a part of the second aspect above (since the first aspect will have a value of either 0 or 1). The generalization of the concept of truth proposed here is ontological only, being the first aspect above. (The third is well known.) It often has values between 0 and 1, even a continuum of them, but one is never required to use it. It can be inserted into ordinary deductions whenever explicit consideration of total domains happens to be useful. The epistemological aspects give effectively three imprecisely bounded states of knowledge of truth (true, false, and indeterminate, their total summing to 1 and therefore depictable on a standard triangular graph) because the probabilities of truth and falsehood will ordinarily not sum to 1. Fuzzy concepts (aspect 3) give an indeterminacy that could be considered ontological or a matter of mere convention: "All men are tall".

density. The translation to deductive form is obvious. In both forms the relation between high density and a decrease in density depends on the context (e.g. on whether the population is confined spatially); it has a nonuniversal domain of truth even within one species. The intermediate steps above are better justified than the proposition "If mice are at high density, then the density will decrease." It is these causal intermediaries that make the proposition seem to us more than an accidental generalization. This proposition itself makes no mention of causation, and its stated relation is observed directly both experimentally and in uncontrolled situations in nature. But its justification as a low-level law is deductive.

A more general ecological law, that the total number of individuals of any species is regulated by some set of factors whose effect is directly related to the total number of individuals, has a somewhat different kind of deductive justification. One premise was used by Darwin and Malthus, that each species increases indefinitely in its environments unless checked. But to apply this premise to the real world there must be some positive lower bound on the rate of the potential increase. This is biologically trivial, because all rates vastly exceed the lower bound, but it seems an artificial and awkward restriction deductively. However, the law is in practice justified only deductively; predictive justifications have failed because of difficulty in generating adequate predictions (e.g. Brockelman and Fagen, 1972). There have also been conceptual problems with the concept of regulatory factor (Van Valen, 1973a), and the identification of regulatory factors in most specific cases is difficult.

Another premise, which like the preceding can itself be derived deductively, is that the resultant of any constantly increasing function and decrements uncorrelated with the value of the function when they are applied, is a random walk about some mean trend. Except in a vanishingly small proportion of cases, such random walks lead eventually either to extinction or indefinitely large size. This is not observed, and by similar arguments it follows that the decrements must be positively correlated with the number of individuals. (A stronger statement is also provable.) But again there are quantitative restrictions necessary, here on the nature of "eventually". Nevertheless, again these restrictions are irrelevant to the biology, as the time available is much longer than is required, so a general law in biology results. The domain of the law may be less than universal, as some species that become extinct may represent a class of exceptions to an assumption.

Because the predictive method is so emphasized, the force of deductive justification is often not recognized by both practicing scientists and philosophers. Thus Murdoch (1966) criticized the simple, if elliptical, deductive justification of a set of proposed new ecological laws by Hairston, Smith, and Slobodkin (1960) in part because the authors gave no testable predictions. (The authors then proceeded to do so and didn't directly defend their use of deduction: Slobodkin, Smith, and Hairston, 1967.) Williams (1973) claimed to justify evolutionary theory for the first time because she gave some predictions of it. Kochanski (1973) stated that prediction was necessary for "testability-in-principle" of scientific hypotheses. Examples of this view are numerous.

Much of evolutionary theory is justified primarily deductively. I have sketched examples above from ecology and give a full deduction of the core of the theory in the appendix. Many other examples exist. This seems to be the main reason why many people, unbiased religiously, find the theory unsatisfactory. Yet they don't find mathematics similarly unsatisfactory, which is strange. If deduction is tautological in one discipline it is tautological in all. But of course it isn't tautological except in a strictly formal sense.

Deduction can give results which aren't immediately apparent and conceptually is thus nontautological. Such people are confused by this ambiguity in the word "tautology". The step in a deductive justification that corresponds to testing predictions is the establishment and testing of assumptions.

Deductive models are rather widely accepted. But I am not talking about models. A model is a deliberate simplification of reality, or even a guess, made in the hope (but not in the knowledge) that it contains enough of the causal structure of the real world to be useful. Models are appropriate when one doesn't have an adequate theory. I am talking about real, explanatory theories. In my deduction of evolution I make certain simplifications that appreciably restrict the domain of the conclusions. This doesn't give a model, in the above sense, but represents a convenience. Removal of the simplifications is trivial but produces appreciable, and conceptually unnecessary, complexity. The deduction thus represents the basic causal processes in the real world and has a structure isomorphic to this causation. Whether it should also include more peripheral causal processes is a matter of taste.

A sometimes important advantage of the deductive method is that it lets one estimate the probability that one's conclusion is true. The probability of truth of the conclusion is conditional on the probabilities of truth of the assumptions, for practical purposes being their product. These probabilities will themselves usually be only imprecisely known, but even then the result will be real information (a probability distribution) on the truth of the conclusion. The predictive method inherently gives no such information whatever.

The difference between the deductive and predictive methods is important in doing science as well as in analyzing it. Recently the staid science of paleontology has had two independent, but surprisingly similar (relative to previous work), analyses of its causal foundations. Mine (Van Valen, 1973b) started with the discovery of an empirical regularity, a law, in the distribution of extinctions. I have proceeded primarily to move back from this law to its causal antecedents. This has mostly involved a new approach to ecology and natural selection (e.g. Van Valen, 1976). I haven't yet succeeded in deriving the explanatory theory from known first principles, perhaps in part because not all the relevant first principles are themselves well established yet. But I find this approach potentially more fruitful than the construction of deliberately unrealistic models and seeing how closely they approximate reality. The latter approach is that of Raup, Gould, Schopf, and Simberloff (1974 and later work by them and others, mostly in the journals Paleobiology and Systematic Zoology). It involves the standard predictive outlook, is easy to do, and gives clear results. It has shown that certain arguments, based on the absence of alternatives, are false because alternatives exist. It hasn't yet contributed positively to our understanding of the real causal nexus behind major evolutionary patterns, and I am skeptical that it will prove important in this way until true causal theories are developed from other approaches. It is at this point that the predictive method has some power.

Darwin

There is a moderate literature on the subject of the supposed nontestability of the theory of evolution by natural selection. Ghiselin (1969) has taken the opposite view and has shown various ways in which the theory makes testable predictions, some of which Darwin gave explicitly. I accept Ghiselin's analysis but believe it is incomplete. Darwin used several other methods of justification than the predictive, such as induction, analogy, and tracing isomorphisms of causal structure between different processes. My extraction of a fifth aspect, the deductive, does not belittle the other aspects but merely ignores them. Darwin's deduction is in fact a major reason why we believe in evolution today.

Newton's deduction of the law of falling bodies contrasts with Darwin's deduction of evolution by natural selection. Newton had the law (it was Galileo's) and found sufficient conditions for it; Darwin, however, had certain ecological and other premises that were known to be true and deduced a necessary consequence, one which had not been well established. This is a stronger procedure because Newton might have chosen the wrong premises, as other theoreticians have in fact done in analogous cases. (Whether Newton really did choose wrong premises, or whether his law merely has a restricted domain, is perhaps a matter of viewpoint.)

I will be concerned only with evolution directed by natural selection that operates by differential pre-reproductive mortality and competition. This case is perhaps the simplest, and it was probably the one Darwin first considered. In fact, it may well be that the argument given below is an elaboration of the deduction Darwin made in 1838 when he discovered the applicability of competitive natural selection to evolution, thus largely completing his theory. Some of the language of the statements is Darwin's but there is not enough evidence to be sure that my reconstruction follows Darwin's implicit argument exactly.

There have been several attempts to construct a deduction of evolution or natural selection in more or less detail[6]. I believe all of these attempts are deficient in one way or another[7] (V.C. Kavaloski has pursued this point in as yet unpublished work), but rather than repeat and modify Kavaloski's criticism I will present a formalized deduction which, whatever its own deficiencies, I hope lacks previous ones. The value of such efforts is to clarify concepts and assumptions, and to establish minimal boundaries on the domain within which the theory is operative. This domain proves to be broader than is sometimes thought.

The controversial debt of Darwin to Malthus has recently been clarified by Vorzimmer (1969), Herbert (1971), and Schwartz (1974). I agree with their interpretation (the papers differ in detail) and summarize it. In 1838, when Darwin read Malthus and deduced a mechanism for adaptation, he had already concluded that evolution probably occurs, that it is adaptive, and that some process more or less analogous to the artificial selection of breeders might be relevant. But what is it in nature that selects adaptive variations? There is no breeder. Malthus provided the answer, to Wallace as well as to Darwin, by showing that reproduction tends to occur indefinitely, beyond the limits of food supply, and so some offspring must die. To Malthus the crucial point was the geometric increase of numbers of individuals beyond the

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[6]Some examples are the following: Wallace (1871, p. 302), Huxley (1942, pp. 14-15), Flew (1959), Beckner (1967), Lee (1969), Lewontin (1969, 1970), Allen (1970), Williams (1970), Vorzimmer (1970, pp. 6-20), Ruse (1971).

[7]For instance, Lewontin's implied deduction (in part modified by an early version of mine) fails because he implicitly uses a strictly frequentist interpretation of probability. One of his three assumptions is that "Different phenotypes have different rates of survival and reproduction in different environments (differential fitness)" (Lewontin, 1970, p. 1). Apart from minor problems of wording, this ignores the essential qualifier "expected" for rates. It is thus entirely compatible with evolution by random drift, which isn't what Lewontin meant at all.

arithemetic increase of food; Darwin (1845, p. 126) knew that the total "supply of food, on an average, remains constant," and so it was even more obvious that competition would occur.

Darwin's acknowledged inspiration from Malthus, and the equally crucial role which reading Malthus played in Wallace's independent discovery, emphasize the historic importance of the deductive argument for natural selection. The only way in which Malthus's insight is useful, is its role in the deductive argument.

Structure of the deduction

General Comments: I present the deduction in four segments, each ending with an important conclusion. Each statement has a name (e.g. C8), and the names of the statements used to justify a given conclusion are in parentheses after the conclusion. A more formal treatment appears as an appendix.

Whenever the assumptions hold, i.e. within the domain of the theory, the conclusions hold. Obviously the assumptions are not always true, for there is not always competition or other phenomena of the conclusions. The conclusions do in fact sometimes hold in cases where the assumptions do not. Competition is not the only cause of evolution, as Darwin knew, but it was the one he considered in 1838. I similarly do not consider Darwin's extensions of the argument to differential extinction, multiplication of species, and ecological diversity.

The point where one starts a deductive argument is arbitrary. If a conclusion is itself obviously true, it can be taken as an assumption and the deduction leading to it thereby eliminated. If an assumption is not obvious, it can often be derived from more acceptable assumptions. I have given Darwin's intellectual situation in 1838 above, and by comparison with the Origin of Species I believe that the argument I give is close to his thoughts. The chronology of his thoughts differs from the sequence of the argument because he pursued the justification of his assumptions. I have added the number theory without explicit textual basis; it is implicit in the Malthusian argument, and the conclusion that population size cannot be infinite was not obvious in 1970 to a graduate student in theoretical biology.

My main purpose is to clarify the structure of Darwin's original deductive argument (of 1838, not 1859). This is still a major part of why we today believe in evolution.

The assumptions are not all independent of each other (e.g., A6-A7, A11-A12, A17-A18), but as will be seen from the appendix they or functionally equivalent assumptions are all necessary for the argument with its present structure. Some terms, e.g. "population," refer to what I defined as stochastic sets (Van Valen, 1964) and which were renamed fuzzy sets by Zadeh (1965). I do not present a minimum axiomatization because the interesting structure is present in the version given.

Some of the propositions are simplifications of the real world and narrow the domain of the theory. Elaboration of these would obscure the structure of the argument with small gain. I indicate which are simplified; at least the direction of generalization should be obvious to people who know what is being approximated.

I. Competition

Definitions

D1. L is the amount of resource R available.

D2. M is some minimum amount of resource R (see A1).

D3. N is the maximum stable number of population W, as determined by L.

D4. A struggle for life occurs in population W whenever at least one member of W dies because of having an insufficient amount of a resource and this lack is due to the size of W being greater than N.

Assumptions

A1. If any individual in population W does not receive at least some minimum amount M of some resource R, this individual will die because of having an insufficient amount of a resource.

A2. If A1 is true, then N is the integer next below the quotient L/M.

A3. L and M are positive, constant, and finite.

A4. The quotient of any two positive numbers is positive and finite.

A5. The integer next below any positive finite number is finite or 0.

A6. At any finite size of population W, the number of next-generation offspring produced is greater than the size of the parental population.

A7. The size of W will increase at least until it reaches N, unless it is already at least as large as N.

A8. Whenever the size of W is greater than N, one or more individuals in W do not receive at least M.

Conclusions

C. N is finite or 0. (A2, A3, A4, A5)

C2. The number of offspring produced will sometimes be greater than N. (C1, A6, A7)

C3. One or more individuals in W will sometimes not receive at least M. (C2, A8)

C4. A struggle for life occurs in W. (C3, A1, A8, D4)

II. Natural Selection

Definitions

D5. Natural selection occurs in population W if the proportions of the varieties comprising W change because of the properties of these varities.

D6. A variety is one of two or more phenotypes in a population relative to one character or set of related characters.

Assumptions

A9. There are varieties in population W.

A10. If there is a struggle for existence, some individuals in W will die between birth and maturity for this reason.

A11. Young individuals of different varieties have different probabilities of surviving to maturity whenever some individuals die between birth and maturity because of a struggle for existence.

A12. A change in the proportions of the varieties comprising a population occurs from birth to maturity if young individuals of different varieties have different probabilities of surviving to maturity.

Conclusions

C5. Young individuals of different varieties in population W have different probabilities of surviving to maturity. (C4, A9, A10, A11)

C6. Natural selection occurs in population W. (D5, A12, C5)

III. Evolution

Definitions

D7. Genotypic natural selection occurs in population W if natural selection occurs among heritable varieties in W.

D8. Evolution occurs in population W if the proportions of the varieties comprising W change from one generation to the next.

Assumptions

A13. There is a tendency to inheritance of the varieties.

A14. The effect of natural selection at one stage is not exactly cancelled by effects at other stages.

A15. The proportions of the varieties comprising W change from one generation to the next whenever genotypic natural selection occurs and A14 is true.

Conclusions

C7. Genotypic natural selection occurs in W. (C6, A13, D7)

C8. Evolution occurs in W. (C7, A14, A15, D8)

IV. The Origin of Species

Definitions

D9. A new species originates whenever divergence of character occurs over a sufficient period of time.

D10. Divergence of character (from the ancestral form) occurs if evolution occurs in predominantly the same direction for many generations.

Assumptions

A16. If evolution occurs in 1 generation, any evolution that may occur in later generations is predominantly in the same direction as that initially.

A17. If evolution occurs in 1 generation, it occurs in many later generations.

A18. There is a sufficient period of time.

Conclusions

C9. Divergence of character occurs. (C8, D10, A16, A17)

C10. A new species originates. (D9, C9, A18)

Discussion

I regard the following statements as unconditionally adequate: D1, D2, D3, A1, A4, A5, A8, D6, A9, D10. The following are adequate but unrealistically restrict the domain of the theory: D4, D5, A10, A12, D7, D8, A15, D9. The following are simplifications of uninterestingly complicated propositions, i.e. they are idealizations: A2, A3, A6. Assumption A8 may be regarded as derivative, but then some other assumption, such as one or more involving iteration over generations, must be added. A16 is also replaceable by a variety of other assumptions.

The remaining propositions (A7; A11;A13, A14; A16, A17, A18) are crucial in that their truth realistically determines whether the conclusion is true. They specify realistic boundaries for their respective domains. For instance, we need to look only at whether A7 is true in a given case to see whether competition occurs. I think this conclusion is tacitly granted by both sides in the controversy as to the frequency of competition.

It has long been known that natural selection operates on other entities than individuals of the sort of species familiar to us. Lyell[8] and Darwin discussed differential extinction of species, and selection at other levels is now a commonplace. The structure of the theory is such that it is broadly applicable, although I have phrased it in terms of individual selection to avoid circumlocution. Some recent treatments of the subject are by Lewontin (1970), Williams (1973), and Van Valen (1975a). As Muller (1929) noted, self-reproduction is normally the most crucial requirement for adaptive evolution and so for defining life, although I will show elsewhere that both the latter are possible without it. Maude (1962) has in fact speculated on the possibility of evolution by natural selection among vortices in stars. The domain of Darwin's initial theory includes even this, and aspects are applicable more broadly. Natural selection of businesses, ideas, and other superficially unrelated entities is more than a metaphor[9]; the structure of the process is often the same.

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Appendix: Symbolic deduction of evolution

This is a semi-formalized deduction, subdivided as in the text. All steps proceed by recognized operations; a complete formalization would be several times as long and add nothing of substance. The degree of rigor is comparable to that in mathematics. The meaning of the symbols should be obvious from comparison with identically numbered propositions (D4, etc.) in the discursive text.

* * *

[8]It is perhaps significant that Lyell (1832, chap. 8-11, and in later editions) explicitly proposed differential extinction of species by much the same mechanism that Darwin used (cf. Van Valen, 1975a). This is natural selection; Lyell used the phrase "struggle for existence," and Darwin read Lyell in several editions before he read Malthus.

[9]I have discussed generalized natural selection briefly elsewhere (Van Valen, 1972; cf. also J.E. Cohen [1967], Toulmin [1972], and earlier work cited in these sources). L.J. Cohen's critique (1973) of Toulmin misses the structure of both the biology and Toulmin's argument. In terms of the present structure, a population corresponds to the set of coexisting ideas or theories that purport to explain some domain. The ideas or theories correspond to both individuals and varieties, which are therefore coextensive in this application. The proof of competition will differ slightly because of this, although the maximum stable number of coexisting theories to explain any domain is ordinarily 1. (A unified treatment involves more general conditions for competition, with one entity preventing another entity from acquiring some resource such as support for a theory.)

Line	Text Reference	Proposition	Derivation or (Note)
1	(D4)	$[(\exists x)((x \varepsilon W)(x \varepsilon D)) \ \& \ (\text{cause } 1)] \supset \text{struggle}$	
2	(C1)	$(N \varepsilon F) \vee (N = 0)$	(1)
3		$N \varepsilon F$	(2)
4	(A7)	$(S)[(S \geqslant N) \vee (S \varepsilon \text{inc})]$	
5		$(S)[(S \varepsilon \text{inc}) \supset (S \geqslant N)]$	(3)
6		$(S)[(S \geqslant N) \supset (S > N)]$	(4)
7		$(S)(S \geqslant N)$	4, 5
8	(C2)	$(S)(S > N)$	6, 7
9	(A8)	$(S)(S > N) \supset [(\exists x)(x \varepsilon W)(x \varepsilon \bar{M})]$	
10	(C3)	$(\exists x)(x \varepsilon W)(x \varepsilon \bar{M})$	8, 9
11	(A1)	$(x)[((x \varepsilon W) \ \& \ (x \varepsilon \bar{M})) \supset ((x \varepsilon W) \ \& \ (x \varepsilon D))]$	
12		$(\exists x)(x \varepsilon W)(x \varepsilon D)$	10, 11
13		$(S)(S > N) \supset [(\exists x)(x \varepsilon W)(x \varepsilon D) \supset (((\exists x)(x \varepsilon W)(x \varepsilon D)) \ \& \ (\text{cause } 1))]$	(5)
14		$((\exists x)(x \varepsilon W)(x \varepsilon D)) \ \& \ (\text{cause } 1)$	8, 12, 13
15	(C4)	struggle	1, 14
16	(D5)	$[(\exists V_i)(V_i \text{ change } 1) \ \& \ (\text{cause } 2)] \supset \text{NS}$	
17	(A9)	$(\exists V_i)(V_i \subset W)(\cup V_i = W)(V_i \cap V_j = \emptyset)(j \neq i)$	
18	(A10)	$\text{struggle} \supset [(\exists a)(a \varepsilon D) \ \& \ (\text{cause } 3)]$	
19		$(\exists a)(a \varepsilon D) \ \& \ (\text{cause } 3)$	15, 18
20	(A11)	$[(\exists V_i)(x)(x \varepsilon V_i) \ \& \ ((\exists V_j)(y)(y \varepsilon V_j)(j \neq i) \ \& \ (\text{cause } 3))] \supset [(x)(y)(P(x \varepsilon D) \neq P(y \varepsilon D))]$	
21	(C5)	$(x)(y)(P(x \varepsilon D) \neq P(y \varepsilon D))]$	17, 19, 20
22	(A12)	$[(\exists V_i) \ \& \ (x)(y)(P(x \varepsilon D) \neq P(y \varepsilon D))] \supset [(\exists V_i)(V_i \text{ change } 1) \ \& \ (\text{cause } 2)]$	(6)
23		$(\exists V_i)(V_i \text{ change } 1) \ \& \ (\text{cause } 2)$	17, 21, 22
24	(C6)	NS	16, 23
25	(A13)	$(V_i)(V_i \varepsilon H)$	
26	(D7)	$[(\exists V_i)(V_i \text{ change } 1) \ \& \ (\text{cause } 2) \ \& \ (V_i)(V_i \varepsilon H)] \supset (\exists x)(x \varepsilon \text{GNS})$	
27	(C7)	$(\exists x)(x \varepsilon \text{GNS})$	23, 25, 26
28		$\text{GNS} \subset \text{NS}$	(7)
29		$(\exists x)(x \varepsilon \text{NS})$	27, 28
30	(D8)	$(\exists V_i)(V_i \text{ change } 2) \supset \text{evolution}$	
31	(A14)	$(\exists x)(x \varepsilon \text{NS}) \supset \bar{c}$	
32		$\bar{c}$	
33	(A15)	$[\bar{c} \ \& \ (\exists x)(x \varepsilon \text{GNS})] \supset (\exists V_i)(V_i \text{ change } 2)$	
34		$(\exists V_i)(V_i \text{ change } 2)$	27, 32, 33
35	(C8)	evolution	30, 34
36	(D10)	$[\text{evolution} \ \& \ \text{direction} \ \& \ \text{generations}] \supset \text{DC}$	
37	(A16)	$\text{evolution} \supset \text{direction}$	
38	(A17)	$\text{evolution} \supset \text{generations}$	
39		direction	35, 37
40		generations	35, 38
41	(C9)	DC	35, 36, 39, 40
42	(D9)	$[\text{DC} \ \& \ \text{time}] \supset \text{species}$	
43	(A18)	time	
44	(C10)	species	41, 42, 43

Notes to the symbolic deduction

(1) I omit symbolization of the number theory.

(2) I omit the proof that (N = 0) implies the absence of W and so eliminates the context.

(3) This inference, taken as an assumption equivalent in function to A6, condenses a formally complex but conceptually trivial iteration.

(4) A further assumption, based on the ubiquity of some random variation in both N and S.

(5) Some such assumption is required to symbolize the causation of the death.

(6) The lack of a logical quantifier expressing "some" and not simultaneously expressing existence forces the circumlocution in proposition 22. This is a defect in symbolic logic. The two concepts are quite separate; there is no more requirement for existence in relation to "some" than in relation to "all".

(7) This assumption, and the consequently convenient existential mode of expression of propositions 26 and 27, is merely a convenience to preserve the generality of A14.

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