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# EVOLUTION, POPULATION THINKING, AND ESSENTIALISM\*

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Ernst Mayr has argued that Darwinian theory discredited essentialist modes of thought and replaced them with what he has called “population thinking”. In this paper, I characterize essentialism as embodying a certain conception of how variation in nature is to be explained, and show how this conception was undermined by evolutionary theory. The Darwinian doctrine of evolutionary gradualism makes it impossible to say exactly where one species ends and another begins; such line-drawing problems are often taken to be the decisive reason for thinking that essentialism is untenable. However, according to the view of essentialism I suggest, this familiar objection is not fatal to essentialism. It is rather the essentialist’s use of what I call the *natural state model* for explaining variation which clashes with evolutionary theory. This model implemented the essentialist’s requirement that properties of populations be defined in terms of properties of member organisms. Requiring such *constituent definitions* is reductionistic in spirit; additionally, evolutionary theory shows that such definitions are not available, and, moreover, that they are not needed to legitimize population-level concepts. Population thinking involves the thesis that population concepts may be legitimized by showing their connections with *each other*, even when they are not reducible to concepts applying at lower levels of organization. In the paper, I develop these points by describing Aristotle’s ideas on the origins of biological variation; they are a classic formulation of the natural state model. I also describe how the development of statistical ideas in the 19th century involved an abandoning of the natural state model.

**1. Introduction.** Philosophers have tended to discuss essentialism as if it were a *global* doctrine—a philosophy which, for some uniform reason, is to be adopted by all the sciences, or by none of them. Popper (1972) has taken a negative global view because he sees essentialism as a major obstacle to scientific rationality. And Quine (1953b), (1960), for a combination of semantical and epistemological reasons, likewise wishes to banish essentialism from the whole of scientific discourse. More recently, however, Putnam (1975) and Kripke (1972) have advocated essentialist doctrines and have claimed that it is the task of each science to investigate the essential properties of its constitutive natural kinds.

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In contrast to these global viewpoints is a tradition which sees the theory of evolution as having some special relevance to essentialist doctrines within biology. Hull (1965) and Mayr (1959) are perhaps the two best known exponents of this attitude; they are *local* anti-essentialists. For Mayr, Darwin's hypothesis of evolution by natural selection was not simply a new theory, but a new *kind of theory*—one which discredited essentialist modes of thought within biology and replaced them with what Mayr has called “population thinking”. Mayr describes essentialism as holding that

. . . [t]here are a limited number of fixed, unchangeable “ideas” underlying the observed variability [in nature], with the *eidos* (idea) being the only thing that is fixed and real, while the observed variability has no more reality than the shadows of an object on a cave wall . . . [In contrast], the populationist stresses the uniqueness of everything in the organic world. . . . All organisms and organic phenomena are composed of unique features and can be described collectively only in statistical terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions, only the individuals of which the population are composed have reality. The ultimate conclusions of the population thinker and of the typologist are precisely the opposite. For the typologist the type (*eidos*) is real and the variation an illusion, while for the populationist, the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different. (Mayr 1959, pp. 28–9).

A contemporary biologist reading this might well conclude that essentialists had no scientifically respectable way of understanding the existence of variation in nature. In the absence of this, typologists managed to ignore the fact of variability by inventing some altogether mysterious and unverifiable subject matter for themselves. The notion of *types* and the kind of anti-empiricism that seems to accompany it, appear to bear only the most distant connection with modern conceptions of evidence and argument. But this reaction raises a question about the precise relation of evolution to essentialism. How could the *specifics* of a particular scientific theory have mattered much here, since the main obstacle presented by essentialist thinking was just to get people to be scientific about nature by paying attention to the evidence? The problem was to bring people down to earth by rubbing their noses in the diversity of nature. Viewed in this

way, Mayr's position does not look much like a form of *local* anti-essentialism.

Other perplexities arise when a contemporary biologist tries to understand Mayr's idea of population thinking as applying to his or her own activity. If "only the individuals of which the population are composed have reality," it would appear that much of population biology has its head in the clouds. The Lotke-Volterra equations, for example, describe the interactions of predator and prey *populations*. Presumably, population thinking, properly so called, must allow that there is something real over and above individual organisms. Population thinking countenances organisms and populations; typological thinking grants that both organisms and types exist. Neither embodies a resolute and ontologically austere focus on individual organisms alone. That way lies nominalism, which Mayr (1969) himself rejects.

Another issue that arises from Mayr's conception of typological and population thinking is that of how we are to understand his distinction between "reality" and "abstraction." One natural way of taking this distinction is simply to understand reality as meaning existence. But presumably no population thinker will deny that there are such things as averages. If there are groups of individuals, then there are numerous properties that those groups possess. The *average* fecundity within a population is no more a property which we invent by "mere abstraction" than is the fecundity of individual organisms. Individual and group properties are equally "out there" to be discovered. And similarly, it is unclear how one could suggest that typologists held that variability is unreal; surely the historical record shows that typologists realized that differences between individuals *exist*. How, then, are we to understand the difference between essentialism and population thinking in terms of what each holds to be "real" about biological reality?

Answering these questions about the difference between essentialist and population modes of thought will be the main purpose of this paper. How did essentialists propose to account for variability in nature? How did evolutionary theory undermine the explanatory strategy that they pursued? In what way does post-Darwinian biology embody a novel conception of variability? How has population thinking transformed our conception of what is *real*? The form of local anti-essentialism which I will propound in what follows will be congenial to many of Mayr's views. In one sense, then, our task will be to explicate and explain Mayr's insight that the shift from essentialist to populationist modes of thinking constituted a shift in the concept of biological reality. However, I will try to show why essentialism was a manifestly *scientific* working hypothesis. Typologists did not

close their eyes to variation but rather tried to explain it in a particular way. And the failure of their explanatory strategy depends on details of evolutionary theory in ways which have not been much recognized.<sup>1</sup>

The approach to these questions will be somewhat historical. Essentialism about species is today a dead issue, not because there is no conceivable way to defend it, but because the way in which it was defended by biologists was thoroughly discredited. At first glance, rejecting a metaphysics or a scientific research program because one of its formulations is mistaken may appear to be fallacious. But more careful attention vindicates this pattern of evaluation. It is pie-in-the-sky metaphysics and science to hold on to some guiding principle simply because *it is possible* that there might be some substantive formulation and development of it. Thus, Newtonianism, guided by the maxim that physical phenomena can be accounted for in terms of matter in motion, would have been rejected were it not for the success of particular Newtonian explanations. One evaluates regulative principles by the way in which they regulate the actual theories of scientists. At the same time, I will try in what follows to identify precisely what it is in essentialism and in evolutionary theory that makes the former a victim of the latter. It is an open question to what degree the source of this incompatibility struck working biologists as central. As I will argue at the end of this section, one diagnosis of the situation which seems to have been historically important is much less decisive than has been supposed.

The essentialist's method of explaining variability, I will argue, was coherently formulated in Aristotle, and was applied by Aristotle in both his biology and in his physics. 17th and 18th century biologists, whether they argued for evolution or against it, made use of Aristotle's Natural State Model. And to this day, the model has not been refuted in mechanics. Within contemporary biology, however, the model met with less success. 20th century population genetics shows that the model cannot be applied in the way that the essentialist requires. But the Natural State Model is not wholly without a home in contemporary biology; in fact, the way in which it finds an application there highlights some salient facts about what population thinking amounts to.

An essentialist view of a given species is committed to there being some property which all and only the members of that species possess.

<sup>1</sup>Mayr (1963) has argued additionally that essentialist errors continue to be made in population biology in the form of the distortions of "bean-bag genetics." The assumption that the fitness of single genes is independent of their genetic context is and has been known to be mistaken; but how this simplifying assumption is essentialist in character is obscure to me.

Since there are almost certainly only finitely many individuals in any given species,<sup>2</sup> we are quite safe in assuming there is some finitely statable condition which all and only the members of the species satisfy. This could trivially be a list of the spatio-temporal locations of the organisms involved. But the fact that such a condition exists is hardly enough to vindicate essentialism. The essentialist thinks that there is a diagnostic property which any *possible* organism must have if it is to be a member of the species. It cannot be the case that the property in question is possessed by all organisms belonging to *Homo sapiens*, even though there might exist a member of *Homo sapiens* who lacked the trait. It must be necessarily true, and not just accidental, that all and only the organisms in *Homo sapiens* have the characteristic.

However, even this requirement of essentialism is trivially satisfiable. Is it not necessarily true that to be a member of *Homo sapiens* an organism must be a member of *Homo sapiens*? This is guaranteed if logical truths are necessary. But essentialism about biology is hardly vindicated by the existence of logical truths. In a similar vein, if it is impossible for perpetual motion machines to exist, then it is necessarily true that something belongs to *Homo sapiens* if and only if it belongs to *Homo sapiens* or is a perpetual motion machine. This necessary truth is not a truth of logic; it is a result of the theory of thermodynamics. But it too fails to vindicate biological essentialism. What more, then, is required?

The key idea, I think, is that the membership condition must be *explanatory*. The essentialist hypothesizes that there exists some characteristic unique to and shared by all members of *Homo sapiens* which explains why they are the way they are. A species essence will be a causal mechanism which works on each member of the species, making it the kind of thing that it is.

The characterization of essentialism just presented is fairly vague. For one thing, a great deal will depend on how one understands the crucial idea of *explanation*. But since explanation is clearly to be a scientific notion, I hope that, on my sketch, essentialism has the appearance of a scientific thesis, although perhaps one that is not terribly precise. Although historically prey to obscurantism, essentialism has nothing essentially to do with mystery mongering,

<sup>2</sup>If species are *individuals*—spatio-temporally extended lineages—as Ghiselin (1966), (1969), (1974) and Hull (1976), (1978) have argued, then we have our assurance of finitude. If, on the other hand, species are kinds of things, which may in principle be found anywhere in the universe at any time, then a slightly different argument is needed for the claim that the same species is overwhelmingly unlikely to have evolved twice. Such an argument is provided by considering the way in which speciation depends on the coincidence of a huge number of initial conditions. See Ayala (1978) for a summary of the received view of this matter.

or with the irrational injunction that one should ignore empirical data. It is a perfectly respectable claim about the existence of hidden structures which unite diverse individuals into natural kinds.

Besides its stress on the giving of explanations, there is another feature of our characterization of essentialism which will be important in what follows. The essentialist requires that a *species* be defined in terms of the characteristics of the *organisms* which belong to it. We might call this kind of definition a *constituent definition*; wholes are to be defined in terms of their parts, sets are to be defined in terms of their members, and so on. Pre-Darwinian critics of the species concept, like Buffon and Bonnet, argued that species are *unreal*, because no such characteristics of organisms can be singled out (see Lovejoy 1936), and pre-Darwinian defenders of the species concept likewise agreed that the concept is legitimate only if constituent definitions could be provided. Constituent definitions are *reductionistic*, in that concepts at higher levels of organization (e.g., species) are legitimate only if they are definable in terms of concepts applying at lower levels of organization (e.g., organisms). It is quite clear that if there are finitely many levels of organization, one cannot demand constituent definitions for concepts at *every* level of organization (Kripke 1978). As we will see in what follows, evolutionary theory emancipated the species concept from the requirement that it be provided with a constituent definition. The scientific coherence of discourse at the population level of organization was to be assured in another way, one to which the label "population thinking" is especially appropriate.

Chemistry is *prima facie* a clear case in which essentialist thinking has been vindicated. The periodic table of elements is a taxonomy of chemical kinds. The essence of each kind is its atomic number. Not only is it the case that all actual samples of nitrogen happen to have atomic number 14; it is necessarily the case that a thing is made of nitrogen if and only if it is made of stuff having atomic number 14. Moreover, this characteristic atomic number plays a central role in explaining other chemical properties of nitrogen. Although things made of this substance differ from each other in numerous respects, underlying this diversity there is a common feature. It was hardly irrational for chemists to search for this feature, and the working assumption that such essences were out there to be found, far from stifling inquiry, was a principle contributor to that inquiry's bearing fruit.

Can an equally strong case be made for an essentialist view of biological species? One often hears it said that evolution undermined essentialism because the essentialist held that species are static, but

from 1859 on we had conclusive evidence that species evolve. This comment makes a straw man of essentialism and is in any case historically untrue to the thinking of many essentialists. For one thing, notice that the discovery of the transmutation of elements has not in the slightest degree undermined the periodic table. The fact that nitrogen can be changed into oxygen does not in any way show that nitrogen and oxygen lack essences. To be nitrogen is to have one atomic number; to be oxygen is to have another. To change from nitrogen into oxygen, a thing must therefore shift from one atomic number to another. The mere fact of evolution does not show that species lack essences.

As a historical matter, some essentialists, like Agassiz (1859), did assert a connection between essentialism and stasis. But others considered the possibility that new species should have arisen on earth since the beginning (if they thought that there was a beginning). Thus, Linnaeus originally hypothesized that all species were created once and for all at the beginning, but later in his career he changed his mind because he thought that he had discovered a species, *Peloria*, which arose through cross-species hybridization (Rabel 1939, Ramsbottom 1938). And in *Generation of Animals* (II 746a30), Aristotle himself speculates about the possibility of new species arising as fertile hybrids. Countenancing such species need have no effect on binomial nomenclature or on deciding which characteristics of organisms to view as diagnostic. The question of *when* there started to be various kinds of things in the universe seems to be quite independent of what makes for differences between kinds.

Another, more plausible, suggestion, concerning how evolution undermined essentialism, is this: The fact that species evolve *gradually* entails that the boundaries of species are vague. The essentialist holds that there are characteristics which all and only the members of a given species possess. But this is no longer a tenable view; it is just as implausible as demanding that there should be a precise number of dollars which marks the boundary between rich and poor. This is the Sorites problem. Since ancient Greece, we have known that being a heap of stones, being bald, and being rich are concepts beset by line-drawing problems. But, the suggestion goes, it was only since 1859 that we have come to see that *Homo sapiens* is in the same boat. Thus, Hull (1965) has argued that essentialism was refuted because of its Aristotelian theory of *definition*; the requirement that species have nontrivial necessary and sufficient conditions runs afoul of the kind of continuity found in nature.

Unfortunately, this limpid solution to our problem becomes clouded a bit when we consider the historical fact that many essentialists

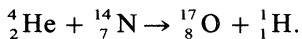


conceded the existence of line-drawing problems. Thus, Aristotle in his *History of Animals*, (588b4 ff.), remarks:

. . . nature proceeds little by little from inanimate things to living creatures, in such a way that we are unable, in the continuous sequence to determine the boundary line between them or to say which side an intermediate kind falls. Next, after inanimate things come the plants: and among the plants there are differences between one kind and another in the extent to which they seem to share in life, and the whole genus of plants appears to be alive when compared with other objects, but seems lifeless when compared with animals. The transition from them to the animals is a continuous one, as remarked before. For with some kinds of things found in the sea one would be at a loss to tell whether they are animals or plants.

It is unclear exactly how one should interpret this remark. Does it indicate that there are in fact no boundaries in nature, or does it mean that the boundaries are difficult to discern? From the time of Aristotle up to the time of Darwin, the principle of continuity seems to have coexisted peacefully with strong essentialist convictions in the minds of many thinkers (Lovejoy 1936). Bonnet, Akenside, and Robinet are 18th century biologists who exemplify this curious combination of doctrines. Does this coexistence imply that the two doctrines are in fact compatible, or rather, does it show that their conceptual dissonance was a long time in being appreciated? To answer this question, let us return to our analogy with the transmutation of elements.

In what sense are the boundaries between chemical kinds any more definite than those which we encounter in biology? At first glance, there appears to be all the difference in the world: in the periodic table, we have discrete jumps—between atomic number 36 and atomic number 37 there are no intermediate atomic numbers to blur distinctions. But let us reflect for a moment on the mechanism of transmutation. Consider, as an example, the experiment which settled the question of how nitrogen can be transmuted into oxygen (Ihde 1964, p. 509):



In this reaction, the  $\alpha$ -particle is absorbed and a proton is expelled. Let us ask of this process a typical Sorites question: At what point does the bombarded nucleus cease to be a nitrogen nucleus and when does it start being a nucleus of oxygen?

There *may* be a precise and principled answer to this question

which is given by the relevant physical theory. But then again there may not.<sup>3</sup> I would suggest that which of these outcomes prevails really does not matter to the question of whether essentialism is a correct doctrine concerning the chemical kinds. It may well be that having a particular atomic number is a vague concept. But this is quite consistent with that (vague) property's being the essence of a chemical kind. This really does not matter, as long as the vagueness of "nitrogen" and that of "atomic number 14" coincide. Essentialism is in principle consistent with *vague essences*.<sup>4</sup> In spite of this, one wonders what the history of chemistry, and its attendant metaphysics, would have looked like, if the transmutation of elements had been a frequent and familiar phenomenon during the second half of the 19th century. Just as the fact of evolution at times tempted Darwin to adopt a nominalist attitude towards species,<sup>5</sup> so in chemistry the impressive taxonomy which we now have in the form of the periodic table might never have been arrived at, line-drawing problems having convinced chemists that chemical kinds are unreal.

As a historical matter, Hull (1965) was right in arguing that essentialism was standardly associated with a theory of definition in which vagueness is proscribed. Given this association, nonsaltative evolution was a profound embarrassment to the essentialist. But, if I am right, this theory of definition is inessential to essentialism. Our argument

<sup>3</sup>I would suggest that quantum mechanical considerations show that the concept of being a nucleus with a particular atomic number is a vague one. Presumably, a collection of protons constitutes a nucleus when the strong force which causes them to attract each other overcomes their mutual electromagnetic repulsion. Whether this happens or not is a function of the distances between the protons. But *this* concept—that of "the" distance between particles—is indeterminate. Hence, the question of whether something is or is not a nucleus with a particular atomic number can only be answered probabilistically.

<sup>4</sup>It is probably a mistake to talk about concepts being vague *simpliciter*. Rather, one should formulate matters in terms of concepts being vague relative to a particular application. The issue of whether a concept is vague seems to reduce to the issue of whether there are cases in which it is indeterminate whether the concept applies or not. I would guess that practically every concept applying to physical objects is vague in this sense. Thus, even such concepts as "being two in number" are such that circumstances can be described in which it is indeterminate whether or not they apply to the objects in question. Degrees of vagueness can be partially defined as follows: If the set of circumstances in which concept *P* is indeterminate in its application is properly included in the set of circumstances in which concept *Q* is indeterminate, then *Q* is more vague than *P*.

<sup>5</sup>Thus in his (1859), p. 52, Darwin says: "From these remarks it will be seen that I look at the term species, as one arbitrarily given for the sake of convenience to a set of individuals closely resembling each other, and that it does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms. The term variety, again, in comparison with mere individual differences, is also applied arbitrarily, and for mere convenience sake." Elsewhere in (1859) (e.g., pp. 432-3), Darwin espouses his perhaps more dominant populationist view that, in spite of line-drawing problems, species are real.

that the gradualness of evolution is not the decisive issue in undermining essentialism is further supported, I think, by the fact that contemporary evolutionary theory contains proposals in which evolutionary gradualism is rejected. Eldredge and Gould (1972) have argued that the standard view of speciation (as given, for example, in Ayala 1978 and Mayr 1963) is one in which phylogeny is to be seen as a series of "punctuated equilibria." Discontinuities in the fossil record are not to be chalked up to incompleteness, but rather to the fact that, in geological time, jumps are the norm. I would suggest that this theory of discontinuous speciation is cold comfort to the essentialist. Whether lines are easy or hard to draw is not the main issue, or so I shall argue.<sup>6</sup>

Another local anti-essentialist argument has been developed by Ghiselin (1966), (1969), and (1974) and Hull (1976) and (1978). They have argued that evolutionary theory makes it more plausible to view species as spatio-temporally extended individuals than as natural kinds. A genuine natural kind like gold may "go extinct" and then reappear; it is quite possible for there to be gold things at one time, for there to be no gold at some later time, and then, finally, for gold to exist at some still later time. But the conception of species given by evolutionary theory does not allow this sort of flip-flopping in and out of existence: once a biological taxon goes extinct, it must remain so. Hull (1978) argues that the difference between chemical natural kinds and biological species is that the latter, but not the former, are historical entities. Like organisms, biological species are individuated in part by historical criteria of spatio-temporal continuity. I am inclined to agree with this interpretation; its impact on pre-Darwinian conceptions of species could hardly be more profound. But what of its impact on essentialism? If essentialism is simply the view that species have essential properties (where a property need not be purely qualitative), then the doctrine remains untouched (as Hull himself realises). Kripke (1972) has suggested that each individual human being has the essential property of being born of precisely the sperm and the egg of which he or she was born. If such individuals as organisms have essential properties, then it will presumably also be possible for individuals like *Drosophila melanogaster* to have essential properties as well. Of course, these essences will be a far cry from the "purely qualitative" characteristics which traditional essentialism thought it was in the business of discovering.

<sup>6</sup>I am not arguing that Hull (1965) and others have misidentified the essence of essentialism and that their criticisms thereby fail to get to the heart of the matter. Essentialism, like most isms which evolve historically, probably does not even have an essence. Rather, I am trying to construe essentialism as a fairly flexible doctrine which, in at least some circumstances, can be seen to be quite consistent with the existence of insoluble line-drawing problems.

My analysis of the impact of evolutionary theory on essentialism is parallel, though additional. Whether species are natural kinds or spatio-temporally extended individuals, essentialist theories about them are untenable. Two kinds of arguments will be developed for this conclusion. First, I will describe the way in which essentialism seeks to explain the existence of variability, and will argue that this conception is rendered implausible by evolutionary theory. Secondly, I will show how evolutionary theory has removed *the need* for providing species with constituent definitions; population thinking provides another way of making species scientifically intelligible. This consideration, coupled with the principle of parsimony, provides an additional reason for thinking that species do not have essences.

**2. Aristotle's Natural State Model.** One of the fundamental ideas in Aristotle's scientific thinking is what I will call his "Natural State Model". This model provides a technique for explaining the great diversity found in natural objects. Within the domain of physics, there are heavy and light objects, ones that move violently and ones that do not move at all. How is one to find some order that unites and underlies all this variety? Aristotle's hypothesis was that there is a distinction between the *natural state* of a kind of object and those states which are not natural. These latter are produced by subjecting the object to an *interfering force*. In the sublunar sphere, for a heavy object to be in its natural state is for it to be located where the center of the Earth is now (*On the Heavens*, ii, clr, 296b and 310b, 2–5). But, of course, many heavy objects fail to be there. The cause for this divergence from what is natural is that these objects are acted on by interfering forces which prevent them from achieving their natural state by frustrating their natural tendency. Variability within nature is thus to be accounted for as a deviation from what is natural; were there no interfering forces, all heavy objects would be located in the same place (Lloyd 1968).

Newton made use of Aristotle's distinction, but disagreed with him about what the natural state of physical objects is. The first law of motion says that if a body is not acted upon by a force, then it will remain at rest or in uniform motion. And even in general relativity, the geometry of space-time specifies a set of geodesics along which an object will move as long as it is not subjected to a force. Although the terms "natural" and "unnatural" no longer survive in Newtonian and post-Newtonian physics, Aristotle's distinction can clearly be made within those theories. If there are no forces at all acting on an object, then, *a fortiori*, there are no interfering

forces acting on it either. A natural state, within these theories, is a zero-force state.

The explanatory value of Aristotle's distinction is fairly familiar. If an object is not in its natural state, we know that the object must have been acted on by a force, and we set about finding it. We do this by consulting our catalog of known forces. If none of these is present, we might augment our catalog, or perhaps revise our conception of what the natural state of the system is. This pattern of analysis is used in population genetics under the rubric of the Hardy-Weinberg law. This law specifies an equilibrium state for the frequencies of genotypes in a panmictic population; this natural state is achieved when the evolutionary forces of mutation, migration, selection and drift are not at work.

In the biological world, Aristotle sets forth the same sort of explanatory model. Diversity was to be accounted for as the joint product of natural regularities and interfering forces. Aristotle invokes this model when he specifies the regularities governing how organisms reproduce themselves:

. . . [for] any living thing that has reached its normal development and which is unutilated, and whose mode of generation is not spontaneous, the most natural act is the production of another like itself, an animal producing an animal, a plant a plant . . .  
(*De Anima*, 415a26).

Like producing like, excepting the case of spontaneous generation, is the natural state, subject to a multitude of interferences, as we shall see.

In the case of spontaneous generation, the natural state of an organism is different. Although in the *Metaphysics* and the *Physics* "spontaneous" is used to mean unusual or random, in the later biological writings, *History of Animals* and *Generation of Animals*, Aristotle uses the term in a different way (Balme 1962, Hull 1967). Spontaneous generation obeys its own laws. For a whole range of organisms classified between the intermediate animals and the plants, like *never* naturally produces like. Rather, a bit of earth will spontaneously generate an earthworm, and the earthworm will then produce an eel. Similarly, the progression from slime to ascarid to gnat and that from cabbage leaf to grub to caterpillar to chrysalis to butterfly likewise counts as the natural reproductive pattern for this part of the living world (*History of Animals*, 570a5, 551b26, 551a13).

So much for the natural states. What counts as an interference for Aristotle? According to Aristotle's theory of sexual reproduction,

the male semen provides a set of instructions which dictates how the female matter is to be shaped into an organism.<sup>7</sup> Interference may arise when the form fails to completely master the matter. This may happen, for example, when one or both parents are abnormal, or when the parents are from different species, or when there is trauma during foetal development. Such interferences are anything but rare, according to Aristotle. Mules—sterile hybrids—count as deviations from the natural state (*Generation of Animals*, ii, 8). In fact, the females of a species do too, even though they are necessary for the species to reproduce itself (*Generation of Animals*, ii, 732a; ii, 3, 737a27; iv, 3, 767b8; iv, 6, 775a15). In fact, reproduction that is completely free of interference would result in an offspring which exactly resembles the father.<sup>8</sup> So failure to exactly resemble the male parent counts as a departure from the natural state. Deviations from type, whether mild or extreme, Aristotle labels “*terata*”—monsters. They are the result of interfering forces (*biaion*) deflecting reproduction from its natural pattern.

Besides trying to account for variation within species by using the Natural State Model, Aristotle at times seems to suggest that there are entire species which count as monsters (Preuss 1975, pp. 215–16; Hull 1968). Seals are deformed as a group because they resemble lower classes of animals, owing to their lack of ears. Snails, since they move like animals with their feet cut off, and lobsters, because they use their claws for locomotion, are likewise to be counted as monsters (*Generation of Animals*, 19, 714b, 18–19; *Parts of Animals*, iv, 8, 684a35). These so called “dualizing species” arise because they are the best possible organisms that can result from the matter out of which they are made. The scale of nature, it is suggested, arises in all its graduated diversity because the quality of the matter out of which organisms are made also varies—and nature persists in doing the best possible, given the ingredients at hand.

One cannot fault Aristotle for viewing so much of the biological domain as monstrous. Natural state models habitually have this characteristic; Newton’s first law of motion is not impugned by the fact that no physical object is wholly unaffected by an outside force. Even so, Aristotle’s partition of natural state and non-natural state in biology sounds to the modern ear like a reasonable distinction run wild. “Real *terata* are one thing,” one might say, “but to call

<sup>7</sup>This characterization of Aristotle’s view in terms of some information bearing entity is not completely anachronistic, as Delbrück (1971) points out when he (in jest) suggests that Aristotle should receive a Nobel Prize for having discovered DNA.

<sup>8</sup>In this discussion of Aristotle’s view of *terata*, I have been much helped by Furth’s (1975, section 11).

entire species, and all females, and all males who don't exactly resemble their fathers monsters, seems absurd." Notice that our "modern" conceptions of health and disease and our notion of normality as something other than a statistical average enshrine Aristotle's model. We therefore are tempted to make only a conservative criticism of Aristotle's biology: we preserve the form of model he propounded, but criticize the applications he made of it. Whether this minimal critique of Aristotle is possible in the light of evolutionary theory, remains to be seen.

The Natural State Model constitutes a powerful tool for accounting for variation. Even when two species seem to blend into each other continuously, it may still be the case that all the members of one species have one natural tendency while the members of the other species have a quite different natural tendency. Interfering forces may, in varying degrees, deflect the individuals in both species from their natural states, thus yielding the surface impression that there are no boundaries between the species. This essentialist response to the fact of diversity has the virtue that it avoids the *ad hoc* maneuver of contracting the boundaries of species so as to preserve their internal homogeneity.<sup>9</sup> This latter strategy was not unknown to the essentialist, but its methodological defects are too well known to be worth recounting here. Instead of insisting that species be defined in terms of some surface morphological feature, and thereby having each species shrink to a point, the essentialist can countenance unlimited variety in, and continuity between, species, as long as underlying this plenum one can expect to find discrete natural tendencies. The failure to discover such underlying mechanisms is no strong reason to think that none exist; but the development of a theory which implies that natural tendencies are not part of the natural order is another matter entirely.

Aristotle's model was a fixed point in the diverse conjectures to be found in pre-Darwinian biology. Preformationists and epigeneticists, advocates of evolution and proponents of stasis, all assumed that there is a real difference between natural states and states caused by interfering forces. The study of monstrosity—teratology—which in this period made the transition from unbridled speculation to encyclopedic catalogues of experimental oddities (Meyer 1939), is

<sup>9</sup>If one views Aristotle as excluding monstrous forms from membership in any species category, then one will have an extreme instance of this *ad hoc* strategy; no organism will belong to any species. Hull (1973, pp. 39–40) sees Aristotle and scholastic science as hopelessly committed to this futile strategy. However, on the view I would attribute to Aristotle, most, if not all, monstrous forms are members of the species from which they arose. They, like Newtonian particles which fail to be at rest or in uniform motion, fail to achieve their natural states because of identifiable causal forces.

an especially revealing example of the power exerted by the Natural State Model. Consider, for example, the 18th century disagreement between Maupertuis and Bonnet over the proper explanation of polydactyly. Both had at their fingertips a genealogy; it was clear to both that somehow or other the trait regularly reappeared through the generations. Maupertuis conjectured that defective hereditary material was passed along, having originally made its appearance in the family because of *an error in nature* (Glass 1959b, pp 62–7). Maupertuis, a convinced Newtonian, thought that traits, both normal and anomalous, resulted from the lawful combination of hereditary particles (Roger 1963). When such particles have normal quantities of attraction for each other, normal characteristics result. However, when particles depart from this natural state, either too many or too few of them combine, thus resulting in *monstres par exces* or *monstres par defect*. Bonnet, a convinced ovist, offered a different hypothesis. For him, polydactyly is never encoded in the germ, but rather results from abnormal interuterine conditions or from male sperm interfering with normal development (Glass 1959a, p. 169). Thus whether polydactyly is “naturalized” by Maupertuis’ appeal to heredity or by Bonnet’s appeal to environment, the trait is never regarded as being completely natural. Variability in nature—in this case variability as to the number of digits—is a deviation from type.

In pre-Darwinian disputes over evolution, natural states loom equally large. Evolutionary claims during this period mainly assumed that living things were programmed to develop in a certain sequence, and that the emergence of biological novelty was therefore in conformity with some natural plan. Lovejoy (1936) discusses how the Great Chain of Being was “temporalized” during the 18th century; by this, he has in mind the tendency to think that the natural ordering of living things from those of higher type down to those of lower type also represented an historical progression. Such programmed, directed evolution—in which some types naturally give rise to others—is very much in the spirit of the Natural State Model. Whether species are subject to historical unfolding, or rather exist unchanged for all time, the concept of species was inevitably associated with that of type; on either view, variation is deviation caused by interfering forces.

It was generally presupposed that somewhere within the possible variations that a species is capable of, there is a privileged state—a state which has a special causal and explanatory role. The laws governing a species will specify this state, just as the laws which make sense of the diversity of kinematic states found in physics tell us what is the natural state of a physical object. The diversity of individual organisms is a veil which must be penetrated in the



search for invariance. The transformation in thinking which we will trace in the next two sections consisted in the realization that this diversity itself constituted an invariance, obeying its own laws.

**3. The Law of Errors and the Emergence of Population Thinking.** So far, I have sketched several of the applications that have been made of Aristotle's model within biology. This strategy for explaining variation, I will argue in the next section, has been discredited by modern evolutionary theory. Our current theories of biological variation provide no more role for the idea of natural state than our current physical theories do for the notion of absolute simultaneity. Theories in population genetics enshrine a different model of variation, one which only became possible during the second half of the 19th century. Some brief account of the evolution within the field of statistics of our understanding of the law of errors will lay the groundwork for discussing the modern understanding of biological variation.

From its theoretical formulation and articulation in the 18th century, up until the middle of the 19th century, the law of errors was understood as a law about *errors*. Daniel Bernouilli, Lagrange, and Laplace each tried to develop mathematical techniques for determining how a set of discordant observations was to be interpreted (Todhunter 1865). The model for this problem was, of course, that there is a single true value for some observational variable, and a multiplicity of inconsistent readings that have been obtained. Here we have a straightforward instance of Aristotle's model: interfering forces cause variation in opinion; in nature there is but one true value. The problem for the theory of errors was to penetrate the veil of variability and to discover behind it the single value which was the constant cause of the multiplicity of different readings. Each observation was thus viewed as the causal upshot of two kinds of factors: part of what determines an observational outcome is the real value of the variable, but interfering forces which distort the communication of this information from nature to mind, also play a role. If these interfering forces are random—if they are as likely to take one value as any other—then the mean value of the readings is likely to represent the truth, when the number of observations is large. In this case, one reaches the truth by ascending to the summit of the bell curve. It is important to notice that this application of the Natural State Model is epistemological, not ontological. One seeks to account for variation in our observations of nature, not variation in nature itself. The decisive transition, from this epistemological to an ontological application, was made in the 1830's by the influential Belgian statistician Adolphe Quetelet.

Quetelet's insight was that the law of errors could be given an ontological interpretation by invoking a distinction which Laplace had earlier exploited in his work in Newtonian mechanics.<sup>10</sup> Laplace decomposed the forces at work in the solar system into two kinds. First, there are the *constant causes* by which the planets are affected by the sun's gravitation; second, there are the particular *disturbing causes* which arise from the mutual influences of the planets, their satellites, and the comets. Laplace's strategy was a familiar analytic one. He tried to decompose the factors at work in a phenomenon into components, and to analyze their separate contributions to the outcome. The character of this decomposition, however, is of special interest: one, central, causal agent is at work on the components of a system, but the effects of this force are complicated by the presence of numerous interferences which act in different directions.

In his book of 1835, *Sur l'homme et le développement de ses facultés, ou essai de physique social*, Quetelet put forward his conception of the *average man* which for him constituted the true subject of the discipline of social physics. By studying the average man, Quetelet hoped to filter out the multifarious and idiosyncratic characteristics which make for diversity in a population, and to focus on the central facts which constitute the social body itself. Like Weber's later idea of an ideal type, Quetelet's conception of the average man was introduced as a "fiction" whose utility was to facilitate a clear view of social facts by allowing one to abstract from the vagaries of individual differences. But unlike Weber, Quetelet quickly came to view his construct as real—a subject matter in its own right. Quetelet was struck by the analogy between a society's average man and a physical system's center of gravity. Since the latter could play a causal role, so too could the former; neither was a mere abstraction. For Quetelet, variability within a population *is caused by* deviation from type. When the astronomer John Herschel reviewed Quetelet's *Lettres sur les probabilités* in 1850, he nicely captured Quetelet's idea that the average man is no mere artefact of reflection:

An average may exist of the most different objects, as the heights of houses in a town, or the sizes of books in a library. It may be convenient to convey a general notion of the things averaged; but it involves no conception of a natural and recognizable central magnitude, all differences from which ought to be regarded as deviations from a standard. The notion of a mean, on the other

<sup>10</sup>Hilts (1973, pp. 209–10). My discussion of Quetelet and Galton in what follows leans heavily on Hilts (1973). It has a number of points in common with Hacking's (1975).

hand, does imply such a conception, standing distinguished from an average by this very feature, *viz.* the regular marching of the groups, increasing to a maximum and thence again diminishing. An average gives us no assurance that the future will be like the past. A mean may be reckoned on with the most implicit confidence (Hilts 1973, p. 217).

Quetelet found little theoretical significance in the fact of individual differences. Concepts of correlation and amount of variation were unknown to him. For Quetelet, the law of errors is still a law about errors, only for him the mistakes are made by nature, not by observers. Our belief that there is variation in a population is no mistake on our part. Rather, it is the result of interferences confounding the expression of a prototype. Were interfering forces not to occur, there would be no variation.

It may strike the modern reader as incredible that anyone could view a trait like girth on this mode. However, Quetelet, who was perhaps the most influential statistician of his time, did understand biological differences in this way. He was impressed, not to say awe struck, by the fact that the results of accurately measuring the waists of a thousand Scottish soldiers would assume the same bell-shaped distribution as the results of inaccurately measuring the girth of a single, average, soldier a thousand times. For Quetelet, the point of attending to variation was to *see through it*—to render it transparent. Averages were the very antitheses of artefacts; they alone were the true objects of inquiry.<sup>11</sup>

Frances Galton, who was Darwin's cousin,<sup>12</sup> was responsible for fundamental innovations in the analysis of individual differences. He discovered the standard deviation and the correlation coefficient. His work on heredity was later claimed by both Mendelians and biometricians as seminal, and thus can be viewed as a crucial step towards the synthetic theory of evolution (Provine 1971). But his interest to our story is more restricted. Galton, despite his frequently sympathetic comments about the concept of type,<sup>13</sup> helped to displace the average

<sup>11</sup>Boring (1929, p. 477) brings out the Aristotelian teleology contained in Quetelet's ideas quite well when he characterizes Quetelet as holding that "We might regard such human variation as if it occurred when nature aimed at an ideal and missed by varying amounts."

<sup>12</sup>Although Galton found *The Origin of Species* an encouragement to pursue his own ideas, he indicates that his interest in variation and inheritance were of long standing. See Hilts (1973, p. 220).

<sup>13</sup>In his *Hereditary Genius*, Galton compared the development of species with a many-faceted spheroid tumbling over from one facet or stable equilibrium to another. See Provine (1971, pp. 14–15). This saltative process insured unity of type. In spite of Galton's adherence to the idea of discontinuous evolution and certain other essentialist

man and the idea of deviation from type. He did this, not by attacking these typological constructs directly, but by developing an alternative model for accounting for variability. This model is a nascent form of the kind of population thinking which evolutionary biologists today engage in.

One of Galton's main intellectual goals was to show that heredity is a central cause of individual differences. Although the arguments which Galton put forward for his hereditarian thesis were weak, the conception of variability he exploited in his book *Hereditary Genius* (1869) is of great significance. For Galton, variability is *not* to be explained away as the result of interference with a single prototype. Rather, variability within one generation is explained by appeal to variability in the previous generation and to facts about the transmission of variability. Galton used the law of errors, but no longer viewed it as a law *about* errors. As Hilts (1973, pp. 223–4) remarks: "Because Galton was able to associate the error distribution with individual differences caused by heredity, the distinction between constant and accidental causes lost much of its meaning." At the end of his life, Galton judged that one of his most important ideas was that the science of heredity should be concerned with deviations measured in statistical units. Quetelet had earlier denied that such units exist. Galton's discovery of the standard deviation gave him the mathematical machinery to begin treating variability as obeying its own laws, as something other than an idiosyncratic artefact.

Eight years after the publication of *Hereditary Genius*, Galton was able to sketch a solution for the problem he had noted in that work: What fraction of the parental deviations from the norm are passed on to offspring? Galton described a model in which hereditary causes and non-hereditary causes are partitioned. Were only the former of these at work, he conjectured, each child would have traits that are intermediate between those of its parents. In this case, the amount of variation would decrease in each generation. But Galton suspected that the amount of variation is constant across generations. To account for this, he posited a second, counteracting force which causes variability within each family. Were this second force the only one at work, the amount of variation would increase. But in reality, the centrifugal and centripetal forces combine to yield a constant quantity of variability across the generations. An error distribution is thus accounted for by way of a hypothesis which characterizes it as the sum of two other error distributions.

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predilections (Lewontin 1974, p. 4), his innovations in population thinking were anti-essentialist in their consequences, or so I will argue.

In his *Natural Inheritance* of 1889, Galton went on to complete his investigations of the correlation coefficient, and introduced the name "normal law" as a more appropriate label for what had traditionally been called the law of errors.<sup>14</sup> Bell curves are normal; they are found everywhere, Galton thought. This change in nomenclature crystalized a significant transformation in thinking. Bell curves need not represent mistakes made by fallible observers or by sportive nature. Regardless of the underlying etiology, *they are real*; they enter into explanations because the variability they represent is lawful and causally efficacious.

The transition made possible by statistical thinking from typological to population thinking was not completed by Galton.<sup>15</sup> Although his innovations loosened the grip of essentialism, he himself was deeply committed to the idea of racial types and believed that evolutionary theory presupposes the reality of types. Both Galton and Darwin (1859, ch. 5; 1868, ch. 13) spoke sympathetically about the ideas of unity of type and of reversion to type, and sought to provide historical justifications of these ideas in terms of common descent. Unity of type was just similarity owing to common ancestry; reversion to type was the reappearance of latent ancestral traits. But the presence of these ideas in their writings should not obscure the way in which

<sup>14</sup>Hilts (1973, p. 228). Walker (1929, p. 185) claims that the origin of the name "normal curve" is obscure. It occurs in Lexis and, she says, "it is not improbable that the term goes back to Quetelet." As natural and inevitable as Quetelet found his interpretation of the bell curve in terms of the Natural State Model, by the time Galton's *Natural Inheritance* appeared in 1889, there was growing sentiment that this interpretation was acceptable, if at all, only as a special case. Thus we find Galton, in that work (p. 58), saying that "the term Probable Error is absurd when applied to the subjects now in hand, such as Stature, Eye-colour, Artistic Faculty, or Disease." A year earlier, Venn, in his *The Logic of Chance* (p. 42), made a similar comment: "When we perform an operation ourselves with a clear consciousness of what we are aiming at, we may quite correctly speak of every deviation from this as being an error; but when Nature presents us with a group of objects of every kind, it is using a rather bold metaphor to speak in this case also of a law of error, as if she had been aiming at something all the time, and had like the rest of us missed her mark more or less in every instance." Quotations are drawn from Walker (1929, p. 53).

<sup>15</sup>It would be important to trace the development of statistical ideas from Galton through Pearson and his circle to R. A. Fisher, and to see whether Pearson's positivistic convictions had the effect of further proscribing the idea of types on the grounds that it is "unscientific." Cohen (1972) sees Galton as already adopting some positivistic attitudes in his idea that heredity was to be understood in terms of correlations, and not in terms of causal forces. Also, see Hacking's (1975) for a bold attempt to link Galton's innovations to other developments in nineteenth century thought. I should point out that a fuller treatment of the emergence of population thinking would have to ascribe a central role to Mendel. He, much more than Galton, provided the central elements of our present conception of the relation of heredity and variation. I have stressed Galton, however, because of his interpretation of statistics and because of his view of the population as a unit of explanation.

their theorizing began to undermine typological thinking.

Darwin and Galton focused on the population as a unit of organization. The population is an entity, subject to its own forces, and obeying its own laws. The details concerning the individuals who are parts of this whole are pretty much irrelevant. Describing a single individual is as theoretically peripheral to a populationist as describing the motion of a single molecule is to the kinetic theory of gases. In this important sense, population thinking involves *ignoring individuals*: it is holistic, not atomistic. This conclusion contradicts Mayr's (1959, p. 28) assertion that for the populationist, "the individual alone is real."

Typologists and populationists agree that averages exist; and both grant the existence of variation. They disagree about the explanatory character of these. For Quetelet, and for typologists generally, variability does not explain anything. Rather it is something to be explained or explained away. Quetelet posited a process in which uniformity gives rise to diversity; a single prototype—the average man—is mapped onto a variable resulting population. Galton, on the other hand, explained diversity in terms of an earlier diversity and constructed the mathematical tools to make this kind of analysis possible.

Both typologists and populationists seek to transcend the blooming, buzzing confusion of individual variation. Like all scientists, they do this by trying to identify properties of systems which remain constant in spite of the system's changes. For the typologist, the search for invariances takes the form of a search for natural tendencies. The typologist formulates a causal hypothesis about the forces at work on each individual within a population. The invariance underlying this diversity is the possession of a particular natural tendency *by each individual organism*. The populationist, on the other hand, tries to identify invariances by ascending to a different level of organization. For Galton, the invariant property across generations within a lineage is the amount of variability, and this is a property *of populations*. Again we see a way in which the essentialist is more concerned with individual organisms than the populationist is. Far from ignoring individuals, the typologist, *via* his use of the Natural State Model, resolutely focuses on individual organisms as the entities which possess invariant properties. The populationist, on the other hand, sees that it is not just individual organisms which can be the bearers of unchanging characteristics. Rather than looking for a reality that *underlies* diversity, the populationist can postulate a reality *sustained* by diversity.

I have just argued that there is an important sense in which typologists are more concerned with individual organisms than populationists are. However, looked at in another way, Mayr's point that populationists

assign a more central role to organisms than typologists do can be established. In models of natural selection in which organisms enjoy different rates of reproductive success because of differences in fitness, natural selection is a force that acts on individual (organismic) differences. This standard way of viewing evolution assigns a causal role to individual idiosyncracies. Individual differences are not *the effects* of interfering forces confounding the expression of a prototype; rather they are *the causes* of events that are absolutely central to the history of evolution. It is in this sense that Mayr is right in saying that evolutionary theory treats individuals as real in a way that typological thought does not (see also Lewontin 1974, pp. 5–6). Putting my point and Mayr’s point, thus interpreted, together, we might say that population thinking endows individual organisms with more reality *and* with less reality than typological thinking attributes to them.

To be real is to have causal efficacy; to be unreal is to be a mere artefact of some causal process. This characterization of what it is to be real, also used by Hacking (1975), is markedly different from the one used in traditional metaphysical disputes concerning realism, verificationism, and idealism (Sober 1980b). There, the problem is not how things are causally related, but rather it concerns what in fact *exists*, and whether what exists exists “independently” of us. The causal view of what it is to be real offers an explanation of a peculiar fact that is part of the more traditional metaphysical problem. Although two predicates may name real physical properties, natural kinds, theoretical magnitudes, or physical objects, simple operations on that pair of predicates may yield predicates which fail to name anything real. Thus, for example, “mass” and “charge” may name real physical magnitudes, even though “mass<sup>2</sup>/charge<sup>3</sup>” fails to name anything real. This is hard to explain, if reality is simply equated with existence (or with existence-that-is-independent-of-us). After all, if an object has a mass and if it has a charge, then there must be such a thing as what the square of its mass over the cube of its charge is. While this is quite true, it is *not* similarly correct to infer that because an object’s mass causes some things and its charge causes other things, then there must be something which is caused by appeal to the square of its mass divided by the cube of its charge. Realism, in this case at least, is a thesis about what is cause and what is effect.

If we look forward in time, from the time of Galton and Darwin to the Modern Synthesis and beyond, we can see how population models have come to play a profoundly important role in evolutionary theorizing. In such models, properties of populations are identified

and laws are formulated about their interrelations. Hypotheses in theoretical ecology and in island biogeography, for example, *generalize over populations* (see, for example, Wilson and Bossert 1971, chs. 3 and 4). The use of population concepts is not legitimized in those disciplines by defining them in terms of concepts applying at some lower level of organization. Rather, the use of one population concept is vindicated by showing how it stands in law-like relations with other concepts *at the same level of organization*. It is in this way that we can see that there is an alternative to constituent definition. Here, then, is one way in which evolutionary theorizing undermined essentialism: Essentialism requires that species concepts be legitimized by constituent definition, but evolutionary theory, in its articulation of population models, makes such demands unnecessary. Explanations can proceed without this reductionistic requirement being met.

If this argument is correct, there is a standard assumption made in traditional metaphysical problems having to do with identity which needs to be reëvaluated. There could hardly be a more central category in our metaphysics, both scientific and everyday, than that of an enduring physical object. The way philosophers have tried to understand this category is as follows: Imagine a collection of instantaneous objects—i.e., objects at a moment in time. How are these various instantaneous objects united into the temporally enduring objects of our ontology? What criteria do we use when we lump together some time slices, but not others? This approach to the problem is basically that of looking for a constituent definition: enduring objects are to be defined out of their constituent time-slices. But, if populations can be scientifically legitimized in ways other than by using constituent definitions, perhaps the same thing is true of the category of physical object itself. I take it that Quine's (1953a) slogan "no entity without identity" is basically a demand for constituent definitions; this demand, which has been so fruitful in mathematics, should not be generalized into a universal maxim (nor can it be, if there are finitely many levels of organization. See Kripke 1978).

If constituent definitions for population concepts are theoretically unnecessary, then we have one argument, *via* the principle of parsimony (Sober 1980a), for the view that species do not have essences. However, there are equally pressing problems which essentialism faces when the Natural State Model is evaluated in the light of our current understanding of the origins of variability. It is to these problems that we now turn.

**4. The Disappearance of a Distinction.** The fate of Aristotle's model at the hands of population biology bears a striking resemblance to



what happened to the notion of absolute simultaneity with the advent of relativity theory. Within classical physics, there was a single, well-defined answer to the question “What is the temporal separation of two events  $x$  and  $y$ ?” However, relativity theory revealed that answering this question at all depends on one’s choice of a rest frame; given different rest frames, one gets different answers. We might represent the way the temporal separation of a pair of events may depend on a choice of frame as in the graph in Figure 1. As is well known, the classical notions of temporal separation and spatial separation gave way in relativity theory to a magnitude that is not relative at all: this is the spatio-temporal separation of the two events. How large this quantity is does not depend on any choice of rest frame; it is frame invariant. Minkowski (1908) took this fact about relativity theory to indicate that space and time are not real physical properties at all, since they depend for their values on choices that are wholly arbitrary. For Minkowski, to be real is to be invariant, and space and time become mere shadows.

Special relativity fails to discriminate between the various temporal intervals represented in Figure 1; they are all on a par. No one specification of the temporal separation is any more correct than any other. It would be utterly implausible to interpret this fact as indicating that there is a physically real distinction which special relativity fails to make. The fact that our best theory fails to draw this distinction gives us a very good reason for suspecting that the distinction is unreal, and this is the standard view of the matter which

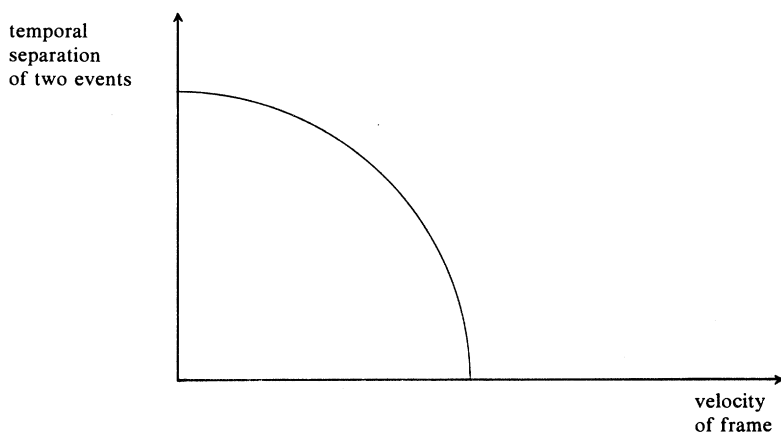


Figure 1: The temporal separation of a pair of events, relative to choices of rest frame.

was crystallized in the work of Minkowski.

According to the Natural State Model, there is one path of foetal development which counts as the realization of the organism's natural state, while other developmental results are consequences of unnatural interferences. Put slightly differently, for a given genotype, there is a single phenotype which it can have that is the natural one. Or, more modestly, the requirement might be that there is some restricted range of phenotypes which count as natural. But when one looks to genetic theory for a conception of the relation between genotype and phenotype, one finds no such distinction between natural state and states which are the results of interference. One finds, instead, the *norm of reaction*, which graphs the different phenotypic results that a genotype can have in different environments.<sup>16</sup> Thus the height of a single corn plant genotype might vary according to environmental differences in temperature, as is shown in Figure 2. How would one answer the question: "Which of these phenotypes is the natural one for the corn plant to have?" One way to take this obscure question is indicated by the following answer: Each of the heights indicated in the norm of reaction is as "natural" as any other, since each happens in nature. Choose an environment, and relative to that choice we know what the phenotypic upshot in that environment is. But, of course, if the question we are considering is understood in terms of the Natural State Model, this sort of answer will not do. The Natural State Model presupposes that there is some phenotype which is the natural one *which is independent of a choice of environment*. The Natural State Model presupposes that there is some environment which is the natural environment for the genotype to be in, which determines, in conjunction with the norm of reaction, what the natural phenotype for the genotype is. But these presuppositions find no expression in the norm of reaction: all environments are on a par, and all phenotypes are on a par. The required distinctions simply are not made.

When one turns from the various phenotypes that a single genotype might produce, to the various genotypes that a population might contain, the same result obtains. Again, according to the Natural State Model, there is a single genotype or restricted class of genotypes, which count as the natural states of the population or species, all other genotypes being the result of interfering forces. But again, statistical profiles of genotypic variance within a population enshrine no such difference. Genotypes differ from each other in frequency;

<sup>16</sup>The discussion of the norm of reaction in what follows depends heavily on some points made in Lewontin (1977).

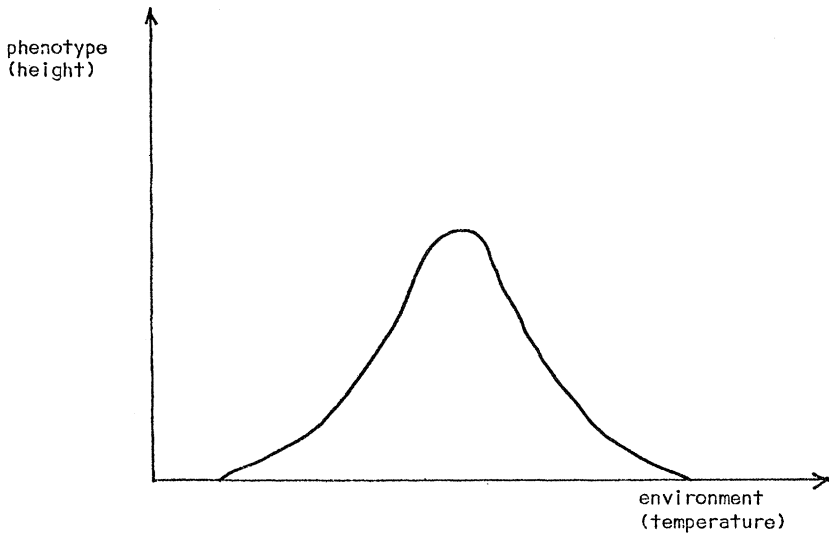


Figure 2: The norm of reaction of a given corn plant genotype, showing height as a function of temperature.

but unusual genotypes are not in any literal sense to be understood as deviations from type.

When a corn plant of a particular genotype withers and dies, owing to the absence of trace elements in the soil, the Natural State Model will view this as an outcome that is not natural. When it thrives and is reproductively successful, one wants to say that *this* environment might be the natural one. Given these ideas, one might try to vindicate the Natural State Model from a selectionist point of view by identifying the natural environment of a genotype with the environment in which it is fittest.<sup>17</sup>

This suggestion fails to coincide with important intuitions expressed in the Natural State Model. First of all, let us ask the question: What is the range of environments relative to which the fittest environment is to be understood? Shall we think of the natural state as that which obtains when the environment is the fittest *of all possible environments*? If so, the stud bull, injected with medications, its reproductive capacities boosted to phenomenal rates by an efficient artificial insemination program, has achieved its natural state. And

<sup>17</sup>This selectionist suggestion needs to be made more precise by specifying the notion of fitness used. I will not lay out these different conceptions here. Rather, I invite the reader to choose the one that he or she finds most plausible. The upshot of my argument does not seem to depend on which biologically plausible characterization is chosen.

in similar fashion, the kind of environment that biologists use to characterize the intrinsic rate of increase ( $r$ ) of a population—one in which there is no disease, no predation, no limitations of space or food supplies—will likewise count as the natural environment. But these optimal environments are *not natural*, the Natural State Model tells us. They involve “artificially boosting” the fitness of resulting phenotypes by placing the genotypes in environments that are more advantageous than the natural environment.

Let us consider another, perhaps more plausible, way to understand the range of environments with respect to which the fittest environment is to be calculated. Instead of taking the best of all possible environments, why not, more modestly, consider the best of all environments that have been historically represented? This suggestion evades the second, but not the first, counterexample mentioned above. However, other problems present themselves. The natural state of a genotype is often understood to be one which has yet to occur. Perhaps every environment that a species has historically experienced is such that a given genotype in that environment results in a *diseased* phenotype, or one which is developmentally impaired in some way. The natural state of a genotype is often taken to be some sort of ideal state which may or may not be closely approximated in the history of the species.

I have just argued that the idea of a fittest environment does not allow one to impose on the norm of reaction the kind of distinction that the Natural State Model requires. Precisely the same reasons count against construing the idea of a genotype’s being the natural state of a species in terms of maximal fitness. It is part of the Natural State Model that the natural genotype for a species can be less fit (in some range of environments) than the best of all possible genotypes. And the natural genotype can likewise fail to be historically represented.

Aristotle is typical of exponents of the Natural State Model in holding that variation is introduced into a population by virtue of interferences with normal sexual reproduction. Our current understanding of the mechanisms of reproduction shows that precisely the opposite is the case. Even if one dismisses mutations as “unnatural interferences,” the fact of genetic recombination in meiosis looms large. Generally, the number of total genotypes that a gene pool can produce by recombination is the product of the number of diploid genotypes that can be constructed at each locus. For species like *Homo sapiens* and *Drosophila melanogaster*, the number of loci has been estimated to be about 10,000 or more. What this means is that the number of genotypes that can be generated by recombination is greater than the number of atoms in the visible universe (Wilson

and Bossert 1971, p. 39). For species with this number of loci, even a single male and a single female can themselves reproduce a significant fraction of the variation found in a population from which they are drawn. All sorts of deleterious phenotypes may emerge from the recombination process initiated by a founder population.

A doctrinaire advocate of the Natural State Model may take these facts to show that recombination has the status of an interference with what is natural. But this desperate strategy conflicts with the received evolutionary view of the function of sexuality. The deploying of prodigious quantities of variability is not a dysfunction which sexual organisms are vulnerable to. Rather it is the principal advantage of sexuality; it is standardly construed to be *what sexuality is for* (but see Williams 1975 for a dissenting opinion). If the notion of a natural state is to make any sense at all, then variability must be viewed as the upshot of natural forces.

The Natural State Model is a *causal*, and thereby a *historical, hypothesis*. The essentialist attempts to understand variation within a species as arising through a process of deviation from type. By tracing back the origins of this variability we discover the natural state of a species. To do this is to uncover that natural tendency possessed by each member of the species. But the science which describes the laws governing the historical origins of variation within species—population genetics—makes no appeal to such “natural tendencies.” Rather, this frame invariant “natural tendency”—this property that an organism is supposed to have regardless of what environment it might be in—has been replaced by a frame relative property—namely, the phenotype that a genotype will produce *in a given environment*. The historical concept of a natural state is discredited in much the same way that the kinematic concept of absolute simultaneity was.

Our current concepts of function and dysfunction, of disease and health, seem to be based on the kinds of distinctions recommended by the Natural State Model. And both of these distinctions resist characterization in terms of maximum fitness. For virtually any trait you please, there can be environments in which that trait is selected for, or selected against. Diseases can be rendered advantageous, and health can be made to represent a reproductive cost. And even if we restrict our attention to historically actual environments, we still encounter difficulties. A perfectly healthy phenotype may be historically nonexistent; the optimum actually attained might still be some diseased state.

The functional notions just mentioned make distinctions which are sanctioned by the Natural State Model. Given the inadequacy of this

model, does this show that the difference between disease and health and the difference between function and dysfunction are mere illusions? I do not think that this follows. What we should conclude is that these functional notions of normality are not to be characterized in terms of a historical notion of fitness. Perhaps they can be understood in some other way; that remains to be seen.

In addition to the influence that the Natural State Model continues to exert in scientific thinking,<sup>18</sup> perhaps even more pervasive is the way that notions of naturalness have had, and continue to have, an influence in politics and in popular culture. Political theorists of both the left and the right have appealed to something called “human nature” (Lewontin 1977, Hull 1978). Political optimists see human nature as essentially good; the evil that human beings have done is to be chalked up to interferences on the part of civilization, or of the state, or of particular economic institutions. Pessimists, on the other hand, see in human beings a natural tendency towards evil, which the restraints made possible by civilization can perhaps correct. The common presupposition here is that each human being has a particular dispositional property—a natural tendency—whose manifestation is contingent on whether environmental forces facilitate the expression of what is natural, or, on the other hand, go against nature by imposing unnatural interferences.

A more recent manifestation of the same habit of mind is to be found in debates about “environmental policy.” Current environmental controversy, both on the part of those who want further industrialization to take its course and on the part of those who want to check or alter the way in which industry impinges on wildlife, tends to picture nature as something apart from us. The question before us, both sides imply, is how we should behave towards this separate sphere. We are not part of what is natural, and what we do has the character of an intervention from the outside into this natural domain. Our pollution of lakes, disruption of ecosystems, and extinction of species is just not natural. Natural, it would seem, is a good thing to be nowadays. Civilization is more often than not an interfering force, deflecting us from what is natural.

The Victorians, too, had their unnatural acts, thus hoping to find their ethics at least consistent with, and possibly vindicated by, the

<sup>18</sup>Lewontin (1977, p. 11) has argued that the idea of a “natural phenotype” has been used in some hereditarian thinking in the IQ controversy. He quotes Herrnstein (1971, p. 54) as talking about “artificially boosting” an individual’s IQ score. The presupposition seems to be that each human genotype has associated with it an IQ score (or range of such scores) which counts as its natural phenotype. As in Aristotle, the individual can be deflected from what is natural by environmental interference.

natural order. But they, at least, maintained some distance from the automatic equation of natural and good. Although some unnatural acts were wrong, others were decidedly right: here natural tendencies had to be checked if morally desirable qualities were to emerge. Perhaps it is a sign of our crumbling moral confidence that we no longer find it possible to separate questions of what is natural from what is good. By equating the two, we hope to read off our ethics directly from what happens in nature, and this gives us the illusion of needing to make no moral decisions for ourselves. This moral buck-passing is incoherent. What happens in nature is simply everything that happens. There is no other sense of "natural". Human society is not external to nature but a special part of it. It is no more a part of human nature to be healthy than to be diseased. Both kinds of phenotypes are to be found, and the norm of reaction makes no distinction between them. If we prefer one and wish to create environments in which it is encouraged, let us say so. But our reasons cannot be given in terms of allowing what is natural to occur unimpeded—by letting nature take its course, as if it has only one. Our activity, and inactivity, requires a more substantive justification than this.

**5. Conclusion.** Essentialism is as much entitled to appeal to the principle of tenacity as any other scientific hypothesis or guiding principle. It was hardly irrational for nineteenth century research on the chemical elements to persist in its assumption that chemical kinds exist and have essential properties. The same holds true for those who hold that species are natural kinds and have essential properties; repeated failure to turn up the postulated items may be interpreted as simply showing that inquiry has not proceeded far enough. Matters change, however, when theoretical reasons start to emerge which cast doubt on the existence claim. For example, if the existence claim is shown to be theoretically superfluous, that counts as one reason for thinking that no such thing exists, or so the principle of parsimony would suggest (Sober 1980a). In another vein, if the causal mechanism associated with the postulated entity is cast in doubt, that too poses problems for the rationality of the existence claim. Our discussion of how population thinking emancipated biology from the need for constituent definitions of species is an argument of the first kind. Our examination of the theory of variation presupposed by essentialism is an argument of the second kind.

No phenotypic characteristic can be postulated as a species essence; the norm of reaction for each genotype shows that it is arbitrary to single out as privileged one phenotype as opposed to any other.

Similar considerations show that no genotypic characteristic can be postulated as a species essence; the genetic variability found in sexual populations is prodigious and, again, there is no biologically plausible way to single out some genetic characteristics as natural while viewing others as the upshot of interfering forces. Even if a species were found in which some characteristic is shared by all and only the organisms that are in the species, this could not be counted as a species essence. Imagine, for example, that some novel form of life is created in the laboratory and subjected to some extreme form of stabilizing selection. If the number of organisms is kept small, it may turn out that the internal homogeneity of the species, as well as its distinctness from all other species, has been assured. However, the explanation of this phenomenon would be given in terms of the selection pressures acting on the population. If the universal property were a species essence, however, explaining why it is universal would be like explaining why all acids are proton donors, or why all bachelors are unmarried, or why all nitrogen has atomic number 14. These latter necessary truths, if they are explainable at all, are not explained by saying that some contingent causal force acted on acids, bachelors or samples of nitrogen, thereby endowing them with the property in question. Characteristics possessed by all and only the extant members of a species, if such were to exist, would not be species essences. It is for this reason that hypotheses of discontinuous evolution like that proposed by Eldredge and Gould (1972) in no way confirm the claims of essentialism.

The essentialist hoped to penetrate the veil of variability found within species by discovering some natural tendency which each individual in the species possesses. This natural tendency was to be a dispositional property which would be manifest, were interfering forces not at work. Heterogeneity is thus the result of a departure from the natural state. But, with the development of evolutionary theory, it turned out that no such property was available to the essentialist, and in fact our current model of variability radically differs from the essentialist's causal hypothesis about the origins of variability.

At the same time that evolutionary theory undermined the essentialist's model of variability, it also removed the need for discovering species essences. Characteristics of populations do not have to be defined in terms of characteristics of organisms for population concepts to be coherent and fruitful. Population biology attempts to formulate generalizations about kinds of populations. In spite of the fact that species cannot be precisely individuated in terms of their constituent organisms, species undergo evolutionary processes, and the character of such processes is what population biology attempts to describe.



Laws generalizing over population will, of course, include the standard *ceteris paribus* rider: they will describe how various properties and magnitudes are related, as long as no other forces affect the system. At least one such law describes what happens when *no* evolutionary force is at work in a panmictic Mendelian population. This is the Hardy-Weinberg equilibrium law. This law describes an essential property—a property which is necessary for a population to be Mendelian. But, of course, such laws do not pick out *species*' essences. Perhaps essentialism can reëmerge as a thesis, not about species, but about *kinds* of species. The Natural State Model arguably finds an application at that level of organization in that the Hardy-Weinberg zero-force state is distinguished from other possible population configurations.

The transposition of Aristotle's distinction is significant. The essentialist searched for a property of *individual organisms* which is invariant across the organisms in a species. The Hardy-Weinberg Law and other more interesting population laws, on the other hand, identify properties of *populations* which are invariant across all populations of a certain kind. In this sense, essentialism pursued an individualistic (organismic) methodology,<sup>19</sup> which population thinking supplants by specifying laws governing objects at a higher level of organization. From the individualistic (organismic) perspective assumed by essentialism, species are real only if they can be delimited in terms of membership conditions applying to individual organisms. But the populationist point of view made possible by evolutionary theory made such reductionistic demands unnecessary. Since populations and their properties are subject to their own invariances and have their own causal efficacy, it is no more reasonable to demand a species definition in terms of the properties of constituent organisms than it is to require organismic biology to postpone its inquiries until a criterion for sameness of organism is formulated in terms of relations between constituent cells. Essentialism lost its grip when populations came to be thought of as real.<sup>20</sup> And the mark of this latter transformation in thought was the transposition of the search for invariances to a higher level of organization.<sup>21</sup>

<sup>19</sup>It is significant that biologists to this day tend to use "individual" and "organism" interchangeably. For arguments that populations, and even species, are to be construed as individuals, see Ghiselin (1966), (1969), (1974), and Hull (1976), (1978).

<sup>20</sup>I borrow this way of putting matters from Hacking's (1975) in which he describes the series of transformations in thought which resulted in "chance becoming real."

<sup>21</sup>The group selection controversy provides an interesting example of the question of whether, and in what respects, it is appropriate to view populations as objects. In some ways, this debate recapitulates elements of the dispute between methodological holism and methodological individualism in the social sciences. See Sober (1980c) for details.

## REFERENCES

- Agassiz, L. (1859), *Essay on Classification*. Cambridge, Mass: Harvard University Press.
- Ayala, F. (1978), "The Mechanisms of Evolution," *Scientific American* 239, 3: 56–69.
- Balme, D. (1962), "Development of Biology in Aristotle and Theophrastus: Theory of Spontaneous Generation," *Phronesis*, 2, 1: 91–104.
- Boring, E. (1929), *A History of Experimental Psychology*. New York: Appleton-Century-Crofts.
- Buffon, L. (1749), *Histoire Naturelle*. Paris.
- Cohen, R. (1972), "Francis Galton's Contribution to Genetics," *Journal of the History of Biology* 5, 2: 389–412.
- Darwin, C. (1859), *On the Origin of Species*. Cambridge, Mass.: Harvard University Press.
- Darwin, C. (1868), *The Variation of Animals and Plants Under Domestication*. London: Murray.
- Delbrück, M. (1971), "Aristotle-totle-totle," in Monod, J. and Borek, J. (eds.), *Microbes and Life*: 50–55. New York: Columbia University Press.
- Eldredge, N. and Gould, S. (1972), "Punctuated Equilibria: an Alternative to Phyletic/Gradualism," in T. Schopf (ed.), *Models in Paleobiology*: 82–115. San Francisco: Freeman Cooper.
- Furth, M. (1975), *Essence and Individual: Reconstruction of an Aristotelian Metaphysics*, chapter 11, duplicated for the meeting of the Society for Ancient Greek Philosophy, unpublished.
- Ghiselin, M. (1966), "On Psychologism in the Logic of Taxonomic Controversies," *Systematic Zoology* 15: 207–15.
- Ghiselin, M. (1969), *The Triumph of the Darwinian Method*. Berkeley: University of California Press.
- Ghiselin, M. (1974), "A Radical Solution to the Species Problem," *Systematic Zoology* 23: 536–44.
- Glass, B. (1959a), "Heredity and Variation in the Eighteenth Century Concept of the Species," in Glass, B., et al. (eds.): *Forerunners of Darwin*: 144–72. Baltimore: The Johns Hopkins Press.
- Glass, B. (1956b), "Mauerpertuis, Pioneer of Genetics and Evolution," in Glass, B., et al. (eds.): *Forerunners of Darwin*: 51–83. Baltimore: The Johns Hopkins Press.
- Hacking, I. (1975), "The Autonomy of Statistical Law," talk delivered to The American Philosophical Association, Pacific Division, unpublished.
- Herrnstein, R. (1971), "IQ," *The Atlantic Monthly* 228 (3): 43–64.
- Hilts, V. (1973), "Statistics and Social Science," in Giere, R. and Westfall, R. (eds.), *Foundations of Scientific Method in the Nineteenth Century*: 206–33. Bloomington: Indiana University Press.
- Hull, D. (1965), "The Effect of Essentialism on Taxonomy: 2000 Years of Stasis," *British Journal for the Philosophy of Science* 15: 314–16; 16: 1–18.
- Hull, D. (1967), "The Metaphysics of Evolution," *British Journal for the History of Science* 3, 12: 309–37.
- Hull, D. (1968), "The Conflict between Spontaneous Generation and Aristotle's Metaphysics." *Proceedings of the Seventh Inter-American Congress of Philosophy*, 2 (1968): 245–50. Quebec City: Les Presses de l'Université Laval.
- Hull, D. (1973), *Darwin and his Critics*. Cambridge, Mass.: Harvard University Press.
- Hull, D. (1976), "Are Species Really Individuals?" *Systematic Zoology* 25: 174–91.
- Hull, D. (1978), "A Matter of Individuality," *Philosophy of Science* 45: 335–60.
- Ihde, A. (1964), *The Development of Modern Chemistry*. New York: Harper & Row.
- Kripke, S. (1972), "Naming and Necessity," in Davidson, D. and Harman, G. (eds.), *Semantics of Natural Languages*: 253–355; 763–9. Dordrecht: Reidel.
- Kripke, S. (1978), "Time and Identity." Lectures given at Cornell University, unpublished.
- Lewontin, R. (1974), *The Genetic Basis of Evolutionary Change*. New York: Columbia University Press.

- Lewontin, R. (1977), "Biological Determinism as a Social Weapon" in the Ann Arbor Science for the People Editorial Collective: *Biology as a Social Weapon*: 6–20. Minneapolis, Minnesota: Burgess.
- Lloyd, G. (1968), *Aristotle: The Growth and Structure of His Thought*. Cambridge: Cambridge University Press.
- Lovejoy, A. (1936), *The Great Chain of Being*. Cambridge, Mass. Harvard University Press.
- Mayr, E. (1959), "Typological versus Population Thinking," in *Evolution and Anthropology: A Centennial Appraisal*: 409–12. Washington: The Anthropological Society of Washington; also in Mayr (1976): 26–9; page references to Mayr (1976).
- Mayr, E. (1963), *Animal Species and Evolution*. Cambridge, Mass.: The Belknap Press of Harvard University Press.
- Mayr, E. (1969), "The Biological Meaning of Species," *Biol. Journal of the Linnean Society*, 1: 311–20; also in Mayr (1976): 515–25; page references to Mayr (1976).
- Mayr, E. (1976), *Evolution and the Diversity of Life*. Cambridge, Mass.: Harvard University Press.
- Meyer, A. (1939), *The Rise of Embryology*. Stanford, Calif.: Stanford University Press.
- Minkowski, H. (1908), "Space and Time" in Lorentz, H., Einstein, A., et al., *The Principle of Relativity*: 73–91. New York: Dover.
- Popper, K. (1972), *Objective Knowledge*, Oxford: Oxford University Press.
- Preuss, A. (1975), *Science and Philosophy in Aristotle's Biological Works*. New York: Georg Olms.
- Provine, W. (1971), *The Origins of Theoretical Population Genetics*. Chicago: University of Chicago Press.
- Putnam, H. (1975), "The Meaning of 'Meaning'," *Mind, Language and Reality*: 215–71. Cambridge: Cambridge University Press.
- Quetelet, A. (1842), *A Treatise on Man and the Development of his Faculties*, Edinburgh.
- Quine (1953a), "Identity, Ostension, Hypostasis" in *From a Logical Point of View*: 65–79. New York: Harper Torchbooks.
- Quine, W. (1953b), "Reference and Modality" in *From a Logical Point of View*: 139–59. New York: Harper Torchbooks.
- Quine, W. (1960), *Word and Object*. Cambridge, Mass.: MIT Press.
- Rabel, G. (1939), "Long Before Darwin: Linne's Views on the Origin of Species," *Discovery, N.S.*, 2: 121–75.
- Ramsbottom, J. (1938), "Linnaeus and the Species Concept," *Proceedings of the Linnean Society of London*: 192–219.
- Roger, J. (1963), *Les Sciences de la Vie dans la Pensée Française du XVIII<sup>e</sup> Siècle*. Paris: Armand Colin.
- Sober, E. (1980a), "The Principle of Parsimony," *British Journal for Philosophy of Science*, forthcoming.
- Sober, E. (1980b), "Realism and Independence," *Noûs*, forthcoming.
- Sober, E. (1980c), "Significant Units and the Group Selection Controversy," *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, forthcoming.
- Todhunter, I. (1865), *History of the Theory of Probability to the Time of Laplace*. New York: Chelsea Publishing.
- Walker, H. (1929), *Studies in the History of Statistical Method*. Baltimore: Williams & Wilkins.
- Williams, G. C. (1975), *Sex and Evolution*. Princeton, New Jersey: Princeton University Press.
- Wilson, E. and Bossert, W. (1971), *A Primer of Population Biology*. Sunderland, Mass.: Sinauer.