ARE THERE NATURAL LAWS CONCERNING PARTICULAR BIOLOGICAL SPECIES?*

In their 1948 classic "Studies in the Logic of Explanation," Carl Hempel and Paul Oppenheim[1] use 'All robins' eggs are greenish-blue' as an example of a statement of natural law (*ibid.*, p. 267). Since then, prevailing opinion has shifted markedly. Among philosophers of biology today, it is widely accepted that there are no natural laws concerning particular biological species. I shall examine whether this view is correct.

I shall consider various reasons that have been offered for accepting this view. These reasons derive from the nature of biological phenomena: for example, from the variation naturally present among conspecific organisms. These reasons also presuppose various philosophical views about natural law: for example, that a natural law must involve an exceptionless regularity in the instantiations of various properties, and that a statement of natural law cannot refer to particular times, places, or individual objects. I shall argue that both of these philosophical views are mistaken.

I shall argue that certain aspects of biological practice suggest that claims like 'The robin's egg is greenish-blue' express laws of nature. I shall try to understand what it means to say 'The *S* is *T*', where *S* is a species and *T* is a biological property. Ultimately, I shall arrive at a conception of natural law according to which 'The robin's egg is greenish-blue' states a natural law despite the natural variation in the colors of robins' eggs, despite the fact that it is an "accident of evolution" that the robin's egg is greenish-blue, and despite the fact that 'robin' refers to an individual object. I believe that by investigating whether there are natural laws concerning particular biological species, we can learn a great deal about both the concept of a biological species and the concept of a natural law.

I. SOME UNSUCCESSFUL ARGUMENTS AGAINST NATURAL LAWS CONCERNING PARTICULAR BIOLOGICAL SPECIES

Hans Reichenbach,[2] Hempel (*op. cit.*, pp. 267f.), and Rudolf Carnap,[3] among many others, hold that a statement of fundamental law cannot ineliminably include a "local" predicate, that is, a predi-

* I am very grateful to William Fitzpatrick and especially to Philip Kitcher for their helpful suggestions for improving this paper.

[1] Repr. in Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965).

[2] *Elements of Symbolic Logic* (New York: Macmillan, 1947), p. 264.

[3] *Philosophical Foundations of Physics* (New York: Basic, 1966), pp. 211f.

0022-362X/95/9208/430-51

cate that essentially refers to a particular time, place, event, or object. James Clerk Maxwell[4] regards this requirement as deriving from the fact that the spatio-temporal location of an event cannot be causally efficacious. Others see this requirement as cashing out the intuition that a natural law is by definition "universal" or "general."

J. J. C. Smart[5] uses this requirement to argue that there are no natural laws concerning particular biological species. Suppose, he says, that by definition an organism is a "robin" exactly when certain nonlocal predicates apply to it (where none of these denotes the property of having greenish-blue eggs). Even if all of the eggs laid by terrestrial creatures that qualify as robins under this definition are greenish-blue, there might well be extraterrestrial organisms that qualify as robins according to this definition but do not lay greenish-blue eggs. So 'All robins' eggs are greenish-blue' is not likely to be true and, if true, is just an accidental generalization, not a statement of natural law. On the other hand, perhaps by definition an organism is a "robin" exactly when it belongs to a given gene pool, that is, exactly when it is not reproductively isolated from certain other organisms that can potentially interbreed. Geography prevents an extraterrestrial creature from interbreeding with any terrestrial organism. The terrestrial and the extraterrestrial organisms are evolutionarily uncoupled. Therefore, on this conception of a biological species, an extraterrestrial creature does not qualify as a robin even if it is so similar to a robin that in the absence of geographic isolation, it could breed with a robin and produce fertile offspring.[6] But then the predicate 'is a robin' is local, making implicit reference to earth or to a particular robin, and so 'All robins' eggs are greenish-blue' again could not state a law. (At least not a *fundamental* law. I shall disregard this gap in Smart's argument, since Hempel and Oppenheim apparently believe that 'All robins' eggs are greenish-blue' states a fundamental law.)

In the interval since Smart offered this argument, philosophers of biology have elaborated other conceptions of a biological species. But they have nearly always retained Smart's conclusion and endorsed his reasoning. For example, many philosophers of biology today believe that, by definition, an organism is a "robin" in virtue of

[4] *Matter and Motion* (New York: Dover, 1952), p. 13.

[5] *Philosophy and Scientific Realism* (New York: Routledge, 1963), pp. 53ff.

[6] Actually, various authors disagree about how the "biological species" concept applies to two geographically isolated populations that could interbreed were they not geographically isolated. To elaborate a detailed sense of "reproductive isolation" that might be appropriate for individuating species is notoriously difficult.

its place in the genealogy of living things; a species is picked out by its position in the evolutionary tree of life. (Smart may have had this conception of biological species in mind as well.) In that case, extraterrestrial creatures would again not be robins (since their evolutionary origin would be distinct from that of robins). On this view, the predicate 'is a robin' is local and hence (it is argued) inadmissible in law statements. Many of these philosophers have also maintained that a species is itself an individual object, a chunk of the genealogical nexus, and since a law statement cannot refer ineliminably to a particular object (because, Michael Ghiselin[7] says, this is part of what it is for laws to be "general" or to "apply irrespective of place and time"), there are no natural laws concerning particular biological species. For example, Alexander Rosenberg[8] writes:

> [S]pecies are not kinds, they are individuals. As such, we can no more expect laws about particular species than we can expect *laws* about what counts as an instance of Napoleon Bonaparte or Mount Rushmore or Third French Republic (*ibid.*, p. 195f.).

Likewise Ghiselin:

> Species are individuals, and there are no laws about individuals in any science whatsoever. In the biological and physical sciences alike, laws have to be generalizations about classes of individuals (*op. cit.*, p. 53).

If this argument goes through, it has far-reaching consequences, as Rosenberg[9] makes clear:

> [A]ll those special branches of biology, ecology, physiology, anatomy, behavioral biology, embryology, developmental biology, and the study of genetics are not to be expected to produce general laws (*ibid.*, p. 219).

I am not convinced by any of these arguments because I am not convinced that local predicates are barred from statements of fundamental natural law. The manifold difficulties faced by this view fall into three categories.

First, the notion of a "local" predicate is notoriously hard to explicate. Is Nelson Goodman's[10] 'grue' a local predicate? In the definition of "local" predicate, what precisely is meant by a "particular

[7] "Individuality, History and Laws of Nature in Biology," in Michael Ruse, ed., *What the Philosophy of Biology Is: Essays Dedicated to David Hull* (Dordrecht: Kluwer, 1989), pp. 53–66; see pp. 56f.

[8] "Why Does the Nature of Species Matter?" *Biology and Philosophy*, II, 2 (April 1987): 192–97.

[9] *The Structure of Biological Science* (New York: Cambridge, 1985).

[10] *Fact, Fiction, and Forecast* (Cambridge: Harvard, 1983, 4th ed.), pp. 74ff.

object"? Are sets "objects"? The notion of a predicate's referring *essentially* to a particular time, place, event, or object presupposes a robust notion of what it is to state a predicate's meaning; I shall not dwell here on the difficulties that attend the analytic/synthetic distinction. For each of these reasons, I seriously doubt that there is any coherent concept of a "local" predicate.

But even if we take for granted a rough and ready conception of "local" predicate, we encounter a second profound difficulty: it is not at all clear that scientific practice, even in the physical sciences, bears out the view that *as a matter of logic*, fundamental law statements include no local predicates. For instance, recall P. A. M. Dirac's[11] famous conjecture that the gravitational-force "constant" is inversely proportional to the time since the Big Bang, which would require that a statement of the gravitational-force law include an implicit reference to some particular moment (for example, refer to the time elapsed since the Big Bang, which Dirac calls "a natural origin of time"). Surely, even if Dirac's conjecture is false, he was not making a *logical* error in entertaining it. It might be objected that although the Big Bang constitutes an event, it is so special an event that a principled exception could be made for it. But there are other examples. The unit of length, in terms of which various law statements express the values of physical constants, was once defined by reference to a particular object (a metal bar in Paris). Apparently, this did not impugn the nomic status of claims specifying the values of physical constants. And as many have noted,[12] if it is built into the concept of a natural law that law statements include no local predicates, then an a priori argument can be made against Aristotelian physics, which held that certain law statements refer to the center of the universe and that others refer to the path of the moon. We must be careful not to misconstrue a characteristic of a great many laws, according to currently flourishing research programs in physics, as deriving from the very concept of a natural law.

There is, finally, a third sort of difficulty, which is arguably the most fundamental. What, precisely, is the reason for holding that a true generalization that ineliminably includes a local predicate cannot state a law? If the intuition is that a law must be "universal" or "general," then once again, it is not clear that this is an intuition about *the concept of natural law.* We can imagine that there obtains a

[11] "A New Basis for Cosmology," *Proceedings of the Royal Society (London) Series A*, CLXV, 921 (April 5, 1938): 199–208.

[12] See, e.g., D.M. Armstrong, *What Is A Law of Nature?* (New York: Cambridge, 1983), p. 26.

law whose statement necessarily involves local predicates. Michael Tooley[13] discusses an unrealistic but apparently logically possible case: a garden in which all of the fruit at any time are apples, where if "one attempts to take an orange into the garden, it turns into an elephant. Bananas so treated become apples as they cross the boundary, while pears are resisted by a force that cannot be overcome" (*ibid.*, p. 686); and there is no nonlocal predicate denoting a property possessed by this garden and responsible for these strange phenomena. There seems to be a logically possible world governed by a law statement that ineliminably includes a local predicate applying only to this garden.

An alternative motivation for holding that local predicates are barred from law statements is that it best explains why various true generalizations, such as Goodman's 'Everything in my pocket on VE day was silver', are accidental generalizations rather than law statements (*op. cit.*, p. 19). But we must remember that we shall then need some *other* explanation of why various true generalizations that do not involve local predicates, such as Reichenbach's famous example 'All gold cubes are smaller than one cubic mile', are not lawlike (*op. cit.*, p. 368). It may very well be that whatever makes a generalization like Reichenbach's accidental also renders a generalization like Goodman's accidental. The above motivation for believing that a law statement contains only nonlocal predicates would then disappear.

One might reply that the theory that local predicates are barred from *fundamental* law statements at least explains why various "derivative law statements" are not *fundamental* law statements. This does not seem to me an especially powerful argument, for two reasons. First, the notion of a "derivative law statement" has never been properly explicated. Examples typically offered are Galileo's law of free fall, the statement of which ('Any freely falling material body is accelerated toward the earth's center at 9.8 m/s^2') refers to the earth, and Kepler's laws of planetary motion, the statements of which refer to the sun. But the sense in which these are law statements at all remains obscure. They may not be physically necessary, since (for example) it may be physically possible for the earth to have a much greater mass than it does. Whether this is physically possible apparently depends upon whether this augmented third planet from the sun would still be "earth." For the sake of argument, let us suppose that these are indeed physically necessary regularities whose statements must include local predicates. (Later I

[13] "The Nature of Laws," *Canadian Journal of Philosophy,* VII, 4 (December 1977): 667–98.

shall have something to say about the nature of such "derivative law statements.") This takes us to a second reason to doubt that the derivative status of these law statements is best explained by the theory that local predicates cannot figure in fundamental law statements. We must bear in mind that there are other physically necessary claims that do not constitute fundamental law statements but do not involve local predicates, such as 'All signals travel slower than twice the speed of light' and 'All copper is electrically conductive and all rubber insulates'. (As a historical example, note that before it was discovered to be an accidental generalization, 'All noncyclic alkane hydrocarbons differ in molecular weight by multiples of the atomic weight of nitrogen' was regarded as physically necessary, that is, not an accidental generalization, but nevertheless a "coincidence."[14]) It may very well be that whatever disqualifies generalizations like these from the ranks of fundamental law statements also disqualifies those derivative law statements which involve local predicates. The above motivation for believing that a fundamental law statement contains only nonlocal predicates would then disappear.

Whether scientists regard some claim as stating a natural law is revealed not by whether they call it a "law" but by how they use it. If a scientist takes some claim to be a law statement, then she uses it to perform various functions that she does not regard accidental generalizations as able to perform. Although philosophers have yet to state these functions precisely, they have long believed that these functions involve counterfactual conditionals, scientific explanations, and inductive confirmation. I see no reason to believe that in order for a scientist to be justified in using a claim that includes local predicates to perform the functions characteristic of law statements, she must believe that there are law statements involving no local predicates that logically necessitate the given claim. For example, I do not see why a scientist cannot be justified in using a claim involving local predicates to "support counterfactuals" in the manner of a law statement even if she has no opinion regarding whether that claim is grounded in law statements involving no local predicates. Likewise, to anyone who maintains that a claim involving local predicates, which is not grounded in law statements free of local predicates, would lack the "explanatory power" distinctive of law statements, I

[14] See J.W. van Spronsen, *The Periodic System of Chemical Elements* (Amsterdam: Elsevier, 1969), pp. 73ff. For more on physically necessary "coincidences," see my "Scientific Realism and Components: The Case of Classical Astronomy," *The Monist*, LXXVII, 1 (January 1994): 111–27.

would reply that this seems like a prejudice derived (understandably) from our experience with post-Aristotelian physics.

I have tried to suggest that no good case has yet been made for the view that local predicates cannot logically possibly figure in statements of natural law, and that there is considerable reason to believe that this view is false. Therefore, none of the arguments I have rehearsed persuades me that there are no natural laws concerning particular biological species.

II. CLAIMS OF THE FORM 'THE *S* IS *T*'

There is another important argument for the view that no natural laws concern particular biological species. Rosenberg presents it just before a passage I quoted earlier. Here is a fuller excerpt:

> There are no laws about particular species.... This fact is reflected not only in the role of specimens, but also in the decline of essentialism among biologists: variation, as Mayr has pointed out, is not viewed as a disturbance from some mean property of members of a species which provides its essence; it is viewed as the normal result of recombinations within a lineage. The generalizations about particular species on which taxonomic decisions rest are full of exceptions, and there is no background theory that will enable us to eventually eliminate, reduce, or explain these exceptions. This should be a major embarrassment for biology.... The simplest explanation of this fact is that species are not kinds, they are individuals....[15]

Variation within any species is the norm, the material on which natural selection operates. It is supposed to follow that any true contingent claim 'All *S*s are *T*' is an accidental generalization.[16]

Another way to make the point is to consider the relation between law statements and counterfactual conditionals. Roughly speaking, the standard view is that, if you believe that 'All *F*s are *G*' states a law, and *Fc* is consistent with all of your beliefs about the natural laws, then you must accept the counterfactual 'Had *Fc* obtained, then *Gc* would have obtained'.[17] So, if I agree with Hempel that 'All crows are black' states a natural law, then I must accept the counterfactual 'If the bird that just flew by had been a crow, then that bird would have

[15] "Why Does the Nature of Species Matter?" pp. 195ff.

[16] Whether this does follow has been provocatively questioned by Philip Kitcher; see, e.g., "Against the Monism of the Moment: A Reply to Elliott Sober," *Philosophy of Science*, LI, 4 (December 1984): 616–30, esp. pp. 622ff. For another rare dissenting voice, see W.J. van der Steen and H. Kamminga, "Laws and Natural History in Biology," *British Journal for the Philosophy of Science*, XLII, 4 (December 1991): 445–67, esp. pp. 459f. My argument will not take for granted the conception of lawfulness that these authors presuppose.

[17] I defend this standard view against some apparent counterexamples in "When Would Natural Laws Have Been Broken?" *Analysis*, LIII, 4 (October 1993): 262–69.

been black' (*op. cit.*, p. 175). But I also accept that coloration tends to vary within a species; as the result of random mutations, the proteins that cause a given pigmentation can be absent from certain individuals. So I believe that had a certain mutation occurred, a crow would have lacked certain proteins and so would have been a nonblack crow. (In certain environments, nonblack crows might even have a selective advantage, and a mutation resulting in a nonblack color might well spread if it happens to occur. I therefore agree with William Kneale[18] that had a population of crows survived for many generations in a snowy region, then it might well have included many nonblack crows; had this fortunate mutation not occurred, the population would likely not have survived.) Apparently, I accept the counterfactual 'Had a certain mutation occurred, there would have been a nonblack crow', even while believing it physically possible for such a mutation to occur. But then, according to the standard view of the relation between law statements and counterfactuals, I cannot be regarding 'All crows are black' as a law statement.

One response to this argument is to maintain that Hempel intends 'All crows are black' to have an implicit ceteris-paribus clause. This proviso would permit crows to be nonblack under certain circumstances, and the counterfactual antecedents 'Had a certain mutation occurred' and 'Had a population of crows survived for many generations in a snowy region' describe such circumstances. That is, if you believe that 'All $(F\&H)$s are G' states a law, the standard view regarding law statements and counterfactuals obviously does not compel you to accept the counterfactual 'Had $Fc\&\neg Hc$ obtained, then Gc would have obtained', even if $Fc\&\neg Hc$ is consistent with all of your beliefs about the natural laws. The difficulty for this response, as Hempel[19] himself would surely have noted, is that the claim 'All crows are black, ceteris paribus' threatens to become the trivial 'All crows are black, except when they are not', which is not a law statement. To make this response plausible, we need an account of the content of the ceteris-paribus clauses in law statements concerning particular biological species.

Still, this approach looks promising, since in the biological literature, one typically finds claims not like 'All crows are black', but claims like 'The crow is a black bird' (or 'The robin's egg is greenish-blue'). This construction, 'The S is T' (or, equivalently, 'Ss are characteristically T'

[18] "Natural Laws and Contrary-to-fact Conditionals," *Analysis*, x, 6 (June 1950): 121–25, p. 123.

[19] See his "Provisos," *Erkenntnis*, xxviii, 2 (March 1988): 147–64, repr. in Grünbaum and Wesley Salmon, eds., *The Limits of Deductivism* (Berkeley: California UP, 1988), pp. 19–36. See also my "Natural Laws and the Problem of Provisos," *Erkenntnis*, xxxviii, 2 (March 1993): 233–48.

or 'typically *T*'), seems to signal that some *S*s are not *T*. Claims of the form 'The *S* is *T*' may well play the roles in connection with scientific explanations, counterfactual conditionals, and inductive confirmation that are distinctive of law statements.[20] To defend this conjecture properly, of course, I would have to offer careful explications of these three functions of law statements, and I cannot do that here.[21] But I shall try to understand the meaning of these claims, in order to see whether they might state laws. Rather than test whether these claims conform to some received view of what it takes to qualify as a statement of natural law, I shall try to learn a lesson from the failure of the received view discussed in the previous section. Once I have some understanding of what these claims mean, I shall consider what laws of nature would have to be like in order for them to be expressed by claims of this sort.

That 'The *S* is *T*' is not some antiquated locution for pre-Darwinian naturalists, but is used by modern biologists, comes out forcefully when biologists use it in describing natural variation among conspecifics. A typical remark is Douglas Futuyma's, from his well-regarded textbook *Evolutionary Biology*:[22]

> Within a single species...individuals sometimes have the diagnostic characteristics of related species or even genera. The form and number of teeth in mammals are important for classification; yet in a single sample of the deer mouse *Peromyscus maniculatus*, Hooper (1957) found variant tooth patterns typical of 17 other species of *Peromyscus*. Among fossils of the extinct rabbit *Nekrolagus*, Hibbard (1963) found one with the premolar pattern characteristic of modern genera of rabbits; and the *Nekrolagus* pattern is occasionally found in living species (*ibid.*, p. 161).

Even while emphasizing the variation in dentition among conspecific mammals, Futuyma refers to the dentition "characteristic" or "typical" of a given species, and to "the *Nekrolagus* pattern." But what does it *mean* to say that "*Nekrolagus* has tooth pattern *T*"?

Some philosophers have suggested that 'The *S* is *T*' ascribes *T*-ness to all *S*s that are healthy, or instead to all *S*s that are nondefective,

[20] Wilfrid Sellars calls 'The *S*' a "distributive singular term." See "Abstract Entities," *Review of Metaphysics*, XVI, 4 (June 1963): 627–71, esp. pp. 631ff., and also Pedro Amaral, ed., *The Metaphysics of Experience* (Atascadero, CA: Ridgeview), pp. 235ff. While not elaborating the logic of such statements, Sellars holds that they are law statements and that they contain ceteris-paribus clauses. G.E.M. Anscombe makes a similar remark in "Modern Moral Philosophy," *Philosophy*, XXXIII, 1 (January 1958), p. 14. Of course, a claim can take the form 'The *S* is *T*', refer to no *S* in particular, and still not be lawlike, as when we say 'The lion was once common as far north as Germany'.

[21] This is part of my project in *Laws of Nature* (forthcoming).

[22] Sunderland, MA: Sinauer, 1979.

or perhaps to all *S*s that are functioning properly or flourishing in the manner proper for *S*s. Among those who have advanced this view are Gottlob Frege, Nicholas Wolterstorff, and Philippa Foot.[23] I do not think that 'The *S* is *T*', at least as used in biology, is properly understood along any of these lines. To begin with, although 'The human being has two legs' is true, not every healthy human being has two legs; an amputee can be perfectly healthy. If the claim instead ascribes two legs to each healthy and uninjured human being, then this will not do for 'The human being has ten fingers', since polydactyly is compatible with being healthy and uninjured. Furthermore, claims of the form 'The human being is *T*' are often properly consulted in treating ill or injured human beings. For example, in treating patients who are liable to heart failure, a physician appeals to claims governing the action of certain drugs, for example, 'The human being who is given coumadin at...dosage will have blood with...diminished coagulation'. And 'The *S* is *T*' may be true although no (*S*&*T*)s are nondefective and healthy: 'The human being with trisomy 21 has mental retardation', 'The human being with chronic rheumatoid arthritis has ulnar deviation'. Moreover, suppose that as a result of mutation, a human being is able to synthesize vitamin C for herself. I would not regard her as unhealthy or defective or failing to flourish, though she would be an exception to 'The human being deprived for...days of food containing vitamin C develops scurvy'.

Perhaps these difficulties could be overcome, but there are more fundamental ones. Although 'The robin has greenish-blue eggs' is true, this color may be unnecessary to any robin's health or flourishing. Perhaps this color is just a result of genetic drift; it may have nothing to do with any adaptive trait. Or it may be a side-effect of some mechanism that contributes to a robin's health, where the salubrious effects of this mechanism could in principle be derived without making eggs greenish-blue. Yet it seems implausible to me that those who discovered that the robin has greenish-blue eggs committed themselves to this character's having some direct or indirect relation to robins' good health or proper functioning.

[23] Frege, "On Concept and Object," P. Geach, trans., repr. in *Collected Papers,* B. McGuinness, ed. (Cambridge: Blackwell, 1984), pp. 182–94; see p. 185; Wolterstorff, "On the Nature of Universals," in M. Loux, ed., *Universals and Particulars* (Garden City: Anchor, 1970), pp. 159–85; see p. 170; and Foot, "Naturalism," Romanell Lecture to the Pacific Division of the American Philosophical Association, delivered March 27, 1989.

Another serious difficulty for these interpretations of 'The *S* is *T*', as used in biology, is that whether an *S*'s possession of character *T* contributes to its health, freedom from defect, or proper functioning depends upon the organism's environment, whereas 'The *S* is *T*' is not 'The *S* is *T* under...environmental conditions, and is ¬*T* under...environmental conditions'. Even if 'The *S* is *T*' is true, there may well be an environment in which an *S*'s possession of character *T* is unhealthful or detrimental to its "functioning well" (insofar as I understand what that means). For example, in the arctic, the crow's black color makes its life more difficult, yet this color is characteristic of the crow. Of course, the arctic is not the characteristic habitat of the crow, but not all examples are of this sort. For instance, although cyanide is poisonous to the cells of the clover *Trifolium repens*, and so its production by the plant itself in its characteristic habitat renders the plant unhealthy and impairs its proper functioning, 'The clover *Trifolium repens* manufactures cyanide' is true. Cyanogenesis is selected for because the cyanide discourages grazing by herbivores. So the clover produces cyanide, even though it is thereby slowly poisoning itself. (Of course, to the extent that its cyanide production contributes to an individual plant's reproductive success, the plant's *mechanism for producing cyanide* is functioning well, producing neither too much nor too little. But the poison is detrimental to a *plant*'s flourishing, I would say, as it is detrimental to a herbivore's.)

My view is that in biological practice, 'The *S* is *T*' ('*S*s are characteristically/typically *T*') specifies a kind of default assumption about *S*s: if you believe (with justification) that some thing is an *S*, then you are entitled to believe it *T* in the absence of information suggesting that it is not. In other words, 'The *S* is *T*' means that when we have certain purposes (which are left unstated, but are understood by those who understand 'The *S* is *T*'), we ought to take as our default assumption that any given *S* is *T*, though we should not necessarily believe a given *S* to be *T* if we have sufficient information to the contrary or if our purposes are outside of those for which this default is useful. Whether 'The *S* is *T*' is sufficiently reliable to be true depends upon how reliable it is—for example, on how readily available "information to the contrary" is when an *S* is not *T*—and on how tolerant of error we can afford to be when we have the relevant purposes. So, for example, we are more willing to say 'The lion is tawny', while knowing that white lions occur occasionally, than to say 'The Witch's Hat mushroom is nonpoisonous', while knowing that poisonous Witch's Hats occur occasionally, because our tolerance for eating poisonous mushrooms is lower than our tolerance for making inaccurate predictions of a lion's color.

One consequence of this construal of 'The *S* is *T*' is that 'The *S* is *T*' conjoined with 'The *S* is *V*' does not logically imply 'The *S* is *T* and *V*'. This appears to accord with ordinary usage.[24] My intuitions are that 'The lion is tawny' and 'The lion is white if it possesses gene *A*' (which is the gene for albinism) are both true, but 'The lion is both tawny and, if it possesses gene *A*, white' is not true. On my view, that is because if we believe 'The lion is tawny' and 'The lion is white if it possesses gene *A*', and we believe Leo to be a lion but do not have any information about whether it possesses gene *A*, then we should not regard ourselves as entitled to believe by default that Leo is tawny *and*, if Leo possesses gene *A*, white. Rather, we should regard ourselves as entitled to believe that Leo is tawny but that, if Leo turns out to possess gene *A*, then Leo is white instead of tawny. Of course, if we believe 'The lion is tawny' and 'The lion is white if it possesses gene *A*', believe Leo to be a lion, and believe that Leo possesses gene *A*, then 'The lion is tawny' is inapplicable, since we should conclude that Leo is white (so long as we have no information to the contrary) and at that point we have information contrary to Leo's being tawny. One might ask why, on my interpretation of 'The *S* is *T*', it is not the case that we are entitled to conclude first that Leo is tawny (since we believe 'The lion is tawny'), and then we have information contrary to Leo's being white, so 'The lion possessing gene *A* is white' is inapplicable. The answer is that part of understanding the ceteris-paribus clause 'unless there is information to the contrary' is understanding where that information might come from—in particular, that a more specific default-specifying rule, 'The (*S*&*H*) is *V*', takes precedence over a less specific one, 'The *S* is *T*'. The former rule should be applied before the latter.

Although we believe 'The crow is black', the counterfactual antecedent 'Had there been a population of crows surviving for many generations in a snowy region...' provides "information to the contrary." Hence, it is outside the scope of 'The crow is black'. Accordingly, the standard view I mentioned earlier concerning the relation between law statements and counterfactual conditionals permits us to believe 'Had there been a population of crows surviving for many generations in a snowy region, then there might well have been a nonblack crow', even while believing that 'The crow is black' states a law. On this view, if we believe that 'The *S* is *T*' is a law statement, then we ought to believe, of a given *S* in the closest possible world in which some counterfactual an-

[24] On this point I appear to disagree with Michael Thompson, "The Representation of Life," in R. Hursthouse, G. Lawrence, and W. Quinn, eds., *Virtues and Reasons* (New York: Oxford, forthcoming). But he may believe that in biological practice, there are several different kinds of judgement of the form 'The *S* is *T*', and this rule of inference applies to some of these kinds but not to others.

tecedent obtains, that it is *T*—so long as we have no information to the contrary and this counterfactual antecedent is consistent with all of our beliefs about the laws. One way we can have information to the contrary is if it is provided by our beliefs about other laws and the counterfactual antecedent, as in the above example. We know, on the basis of other laws, that had a population of crows survived for many generations in a snowy region, then it might well have included nonblack crows. We can also have information to the contrary with the help of our background beliefs about matters of nonnomic fact. I recall reading somewhere that Anne Boleyn had eleven fingers. Let us consider whether she would have had only ten fingers had she not married Henry VIII. I am not guided by 'The human being has exactly ten fingers'. Admittedly, the counterfactual antecedent combined only with my beliefs regarding natural laws does not suggest that Anne is an exceptional case. But my background beliefs about Anne Boleyn, brought to bear on the antecedent in light of the other natural laws I know, tell me that she would still have had eleven fingers, and this counts as "information to the contrary."

Suppose I was not already aware of the fact that Anne Boleyn had eleven fingers. Then I would have had no information to the contrary, and so (since I know 'The human being has exactly ten fingers') I should have believed 'Had Anne Boleyn not married Henry VIII, she would have had only ten fingers'. Suppose that later I learn that Anne Boleyn had eleven fingers. Then I should believe that though I was formerly justified in believing 'Had Anne Boleyn not married Henry VIII, she would have had exactly ten fingers', that belief was false. Recall that the standard view regarding the relation of law statements to counterfactual conditionals concerns whether I am *justified* in believing various counterfactuals under various circumstances, not whether those counterfactuals are *true*.

III. NATURAL LAWS AND BIOLOGICAL SPECIES

I have already remarked that whether a claim is regarded by scientists as stating a natural law should be judged by whether that claim functions in scientific practice as a law statement. I have argued that, at least in their relation to counterfactual conditionals, claims of the form 'The *S* is *T*' function as law statements. In the absence of some comprehensive account of the other functions distinctively performed by law statements (for example, in connection with inductive confirmation and scientific explanation), I cannot examine whether claims of the form 'The *S* is *T*' perform the other roles characteristically played by law statements. What, then, can be said about whether such claims state natural laws concerning particular biological species?

It might be objected that since some *S*s are not *T*, even when no information is known suggesting that they are not *T*, it cannot be a law of nature that all *S*s are *T* unless there is information to the contrary. This objection presupposes that a natural law must involve an exceptionless regularity. I believe that the above analysis of 'The *S* is *T*' suggests otherwise. It suggests, on the contrary, that a natural law must involve a reliable rule of inference—for example, from '...is *S*', in the absence of contrary information and in the service of the relevant purposes, to '...is *T*'. (As we have seen, that a rule is "reliable" in this sense does not require that it be truth preserving, only that it be reliable enough for the implicitly understood purposes.) I cannot hope to elaborate this analysis fully here, but later I shall develop it a bit further.

It might likewise be objected that however 'The *S* is *T*' is elaborated as specifying some kind of reliable rule of inference, it cannot express a law, because it is not physically necessary that 'The *S* is *T*' specify a default assumption useful for certain purposes. The prevalence of *T*-ness among *S*s is an accident, by which I mean a physically unnecessary fact. In other words, it was physically possible for genetic drift and natural selection to have resulted in so few *T*s among the *S*s that 'The *S* is *T*' was not sufficiently "reliable" (however this notion is elaborated) to specify a useful default assumption for the relevant purposes, and so to qualify as a law statement. (Indeed, this might happen sometime in the future.)

As it stands, this objection presupposes what it sets out to show. To deem some conceivable state of affairs "physically possible" is just to deem it consistent with natural law. So to assert that the prevalence of *T*-ness among *S*s is an accident—that it is physically possible for 'The *S* is *T*' to be unreliable—is just to assert baldly, not to show that 'The *S* is *T*' is not a law statement.

But surely, the objector might insist,[25] it is a very strong intuition that the natural laws are consistent with the occurrence of a mutation that introduces a lineage of robins whose females lay rose-colored eggs and can fully interbreed with the very same males as do the female robins that lay greenish-blue eggs. Furthermore, the intuition runs, the natural laws are consistent with the trait of laying rose-colored eggs spreading among robins to such an extent that 'The robin's egg is greenish-blue' does not specify a sufficiently reliable default assumption to be useful for the relevant purposes.

[25] See, e.g., John Beatty, "What's Wrong with the Received View of Evolutionary Theory?" in Peter Asquith and Ronald Giere, eds., *PSA 1980, Volume II* (East Lansing, MI: Philosophy of Science Association, 1981), pp. 397–426, esp. pp. 406f.; Kitcher, "Species," *Philosophy of Science*, LI, 2 (June 1984): 308–33, p. 312.

I agree with this intuition, to a point. I believe that what counts as a natural law (or a physical possibility) varies with the scientific discipline. In physics or evolutionary biology, it is an accident (that is, a physically unnecessary fact) that no such mutation occurs (if, indeed, none occurs). But from the point of view of many biological disciplines—such as those Rosenberg enumerates in the passage I quoted earlier—it is not a physical accident. To see why, we must see what it takes for a reliable rule of inference to qualify as a law statement in a given scientific discipline. After all, to every accidental generalization 'All *F*s are *G*', there corresponds a rule of inference ('From "...is *F*" infer "...is *G*"') which is truth preserving and so maximally reliable. We must see why these rules do not qualify as statements of natural law.

This is a very difficult and controversial question, and all I can do here is to sketch an approach to answering it.[26] But this sketch will suggest how an accident of evolution can be a law of ornithology.

Consider Hooke's law, which is typically expressed as holding that a body attached to a spring feels a restoring force proportional (ceteris paribus) to the elongation of the spring from its equilibrium length. On my view, a statement of Hooke's law says that in certain circumstances (such as when the spring is not stretched beyond its elastic limit and feels no disturbing influences[27]), an acceptable step to take in pursuing a particular sort of approach to making certain kinds of empirical predictions for certain purposes is to infer a conclusion about (say) the spring's restoring force F from x (the vector extending from the equilibrium position to the body's location) and the spring constant k $(=(F_o/x_o)$, where F_o is the magnitude of the restoring force exerted on a body when it is displaced from its equilibrium position by some small amount x_o) in accordance with $F = -kx$. *Part* of what makes this step acceptable is that by reasoning in this way, we infer claims that (if used in accordance with certain other rules) yield conclusions that (under certain circumstances) are close enough to the truth for the relevant purposes. Merely "close enough to the truth" because even if the premises are true and the circumstances fall within the range in which Hooke's law is appropriately used, the conclusion so yielded need not be true. That is because Hooke's law treats the restoring force as varying linearly with x, but for any actual spring, there are higher-order terms (that is, terms in x^2, x^3, and so on). So

[26] Part of my project in *Laws of Nature* is to offer an answer along roughly the lines I am about to sketch.

[27] On the meaning of the proviso 'in the absence of disturbing influences', see my "Natural Laws and the Problem of Provisos."

whether it is acceptable for scientists to use Hooke's law depends upon the degree of approximation that their project can tolerate.

Now, Hooke's law is no more reliable than a rule of inference corresponding to an accidental generalization. I suggest that the *lawlikeness* of Hooke's law consists not in its reliability but in its belonging to the reliable set of inferential rules that would eventually be adopted by anyone who pursued the best set of "inductive strategies" for discovering reliable rules of inference. To pursue the "inductive strategy" that yields Hooke's law and belongs to this best set, you would see if the data gathered by observing various bodies attached to springs (that are under no disturbing influences, and so on) suggest the reliability for the relevant purposes of some rule concerning all such springs underwriting inferences to (say) x from premises that include F. You would then believe lawlike whichever rule of this kind (if any) presently seems salient to us in light of the evidence—salient in the way that the correct answer to 'Give the next member of the sequence 2,4,6,8...' jumps out at us. You would then regard any case discovered to conform sufficiently to the salient rule as inductively confirming its reliability.[28]

One reason that this strategy belongs to the *best* set of inductive strategies[29] is that when we pursue this strategy, a reliable rule (namely, Hooke's law) jumps out at us quickly, that is, after we have examined very few cases. Another reason is that it covers a wide range of cases—springs of all materials and constructions (feeling no disturbing influences, and so on)—and by reviewing data involving

[28] By *inductively* confirming the rule's reliability, I mean (roughly) confirming the rule's *predictive* accuracy by confirming, of each conceivable *unexamined* case in a certain broad class, that it would conform sufficiently closely to the rule. I characterize "inductive" confirmation more precisely in "Earman on the Projectibility of Grue," David Hull, Micky Forbes, and Richard Burian, eds., *PSA 1994, Volume I* (East Lansing, MI: Philosophy of Science Association, 1994), pp. 87–95. It is not my view that scientists typically come to adopt various claims as law statements by pursuing such inductive strategies. But that is not to say that inductive strategies are not central to a rational reconstruction of scientific practice. For instance, I hold that the reason we must treat the claims we believe to be law statements as having a distinctive capacity to support counterfactuals is ultimately because we must take these claims as resulting from the best set of inductive strategies, so that we must regard the evidence as confirming, of each unrealized (and perforce unexamined) case in a certain broad class, that it would have conformed to these claims. Cf. *Laws of Nature* and "Inductive Confirmation, Counterfactual Conditionals, and Laws of Nature" (*Philosophical Studies*, forthcoming).

[29] I cannot fully explicate here what makes one set of inductive strategies "better" than another. For instance, my remarks in the following paragraphs will not directly explain why the best set of inductive strategies omits the inductive strategy yielding the rule of inference corresponding to 'All gold cubes are smaller than one cubic mile'. I address this question in *Laws of Nature*.

springs of any one kind (for example, iron "slinky"-type springs), we would be led by this inductive strategy to a rule that turns out to be reliable regarding springs of any other kind (for example, balls of plastic wrap); though our evidence might include no balls of plastic wrap, the rule that jumps out at us when we examine other springs (for example, iron "slinky"-type springs) is nevertheless reliable regarding the restoring forces exerted by balls of plastic wrap. It is therefore advantageous to pursue this broad strategy, that is, to look for such a rule covering springs of all kinds, rather than to pursue one inductive strategy that examines only iron "slinky"-type springs in order to find a rule covering them, to pursue another inductive strategy that examines only balls of plastic wrap in order to find a rule covering them, and so on. In other words, to pursue an inductive strategy that searches for the broader rule, and so to regard iron "slinky"-type springs that conform to the salient rule as confirming this rule's reliability not only to other iron "slinky"-type springs but also to balls of plastic wrap, is to use the evidence on hand most effectively.[30]

Nevertheless, in addition to Hooke's law, there are laws each of which applies only to one particular kind of spring. But unlike Hooke's law, each of these laws includes higher-order terms and always yields true predictions from true premises. One might then wonder: Why is Hooke's "law" lawful? Why are there not just these various more specialized laws that include nonlinear terms? For that matter, why are there *different* nonlinear laws for springs of different kinds, rather than a single complicated law statement that specifies the higher-order terms for springs of all kinds?

The answer arises from the following consideration. For many purposes, the linear term suffices to yield a sufficiently accurate prediction of the restoring force. Although k (the coefficient of the linear term) depends on the spring's material and design, the spring constant of *any* spring can be expressed as (F_o/x_o) for small x_o. But the precise coefficients of the higher-order terms depend on the material and construction of the spring in such a way that there are no simple expressions for these terms that apply to all springs. Rather, there are relatively simple expressions for the higher-order terms for rubber bands, different expressions for the higher-order terms for panes of

[30] Michael Ruse gestures toward an analogous feature of Mendel's Laws: "There can be few statements of sciences which have been found to satisfy the limited condition of unrestricted universality more fully. Since Mendel first proposed his laws, they have been found to hold for a range of organisms from elephants to cod fish, from sea-weed to oak trees"—*The Philosophy of Biology* (London: Hutchinson, 1973), p. 29.

glass, different expressions for the higher-order terms for balls of plastic wrap, different expressions for the higher-order terms for iron "slinky"-type springs, and so on. For this reason, in searching for truth-preserving (or extremely accurate) rules mediating inferences to claims providing information about the restoring forces of springs—for there are some contexts in which only a very high degree of accuracy will do—it is better to treat the different kinds of springs separately. Evidence derived from examining balls of plastic wrap helps to make jump out at us a truth-preserving rule (with higher-order terms) for balls of plastic wrap, but will mislead us concerning a truth-preserving rule for iron "slinky"-type springs; the correct higher-order terms for iron "slinky"-type springs will not jump out at us if we examine only balls of plastic wrap. In contrast, Hooke's law is a simple rule; it jumps out at us quickly as we accumulate evidence that accords tolerably well with it, even evidence drawn from examining only balls of plastic wrap, and it is sufficiently reliable for various purposes. Hooke's law, then, states a genuine natural law, even though it is not truth preserving and even though there are more specialized, truth-preserving laws covering various types of springs.

This account, albeit sketchy, allows us to understand the sense in which a so-called "derivative" law statement, such as Galileo's law of freely falling bodies, is a law statement, even though (from another perspective) it depends on various physically unnecessary facts (for example, the earth's mass). A law statement is a reliable inferential rule yielded directly by an inductive strategy in the set of inductive strategies that is best for us to have been pursuing. Whether a set of inductive strategies is the best set for us to have been pursuing depends upon the range of evidence that has been accessible to us. Newton's gravitational-force law will not jump out at us so long as we look only at the forces exerted by the earth on various bodies falling freely from modest heights; when our evidence is so restricted, it is better for us instead to pursue an inductive strategy that directly yields Galileo's law. When new sorts of data become available (for example, concerning the forces exerted by the sun on various bodies), a different set of inductive strategies qualifies as the best for us, and a different set of reliable rules qualifies as the law statements. (And as always having been the law statements; presumably, it is at all times the case that those inductive strategies are the best for us to have been pursuing if we have had access to exactly the sorts of evidence now accessible to us.) If it would have been better for us (considering the range of evidence accessible to us) to have pursued a set of inductive strategies that directly yields Newton's two-body gravitational-force law than a set that directly yields Galileo's "law" of freely falling bodies, the latter is an accidental generalization (if true). Now, it may happen that the additional cases covered by a broad rule like Newton's are outside the scope of a certain specialized scientific field. In that case, an accidental generalization in a more inclusive science may be a law statement in a more specialized discipline. For instance, recalling Galileo's work for fusiliers of the Venetian artillery, perhaps ballistics concerns itself only with falls to the earth in which, as a matter of fact, no factors "disturbing" to the law of free fall are present. (This results from the range of cases covered by ballistics—for example, falls from modest altitudes of bodies feeling little air resistance—and from our interests—for example, the deviation from 9.8 m/s^2 is too slight to matter.) In that event, the "law" of falling bodies might genuinely be a natural law of ballistics, since it results directly from the set of inductive strategies that it is best for researchers in that discipline to pursue. Likewise, 'The crow is black' and 'The human being deprived of food containing vitamin C for...days develops scurvy' may be law statements of ornithology and human medicine, respectively.

Some biological disciplines concern all conceivable organisms in a certain range of conceivable circumstance—roughly speaking, those compatible with the laws of physics. The laws of evolutionary biology, for example, are intended to cover not just actual but also certain merely hypothetical species of organism. The range of the "physically possible" in ornithology, human medicine, and the disciplines listed by Rosenberg is much narrower; ornithology is concerned with only actual species of birds. On my view, an accident of evolution, such as the nonoccurrence of a certain mutation in crows or the absence from the human genome of a gene for synthesizing vitamin C, can in another context be tied up with physical necessities. This seems to me no different from a physicist's remarking that the values of the fundamental physical constants, while built into the physical laws, sometimes seem from the perspective of cosmology like accidents in the universe's construction. If these physical "constants" change slowly, as Dirac proposed, or differ in different oscillations of the universe's expansion and contraction, then on this account, the familiar "law statements," expressed in terms of the current values of these constants, would still be law statements of those scientific disciplines concerned exclusively with the present "cosmic epoch."

A great deal of biology is concerned with discovering what Ernst Mayr[31] calls the "biological properties" of various species—for exam-

[31] *Animal Species and Evolution* (Cambridge: Harvard, 1965).

ple, their external morphology, internal anatomical structure, physiology, chemical constituents, ecological and environmental tolerances, and so on. This is evident from even a casual perusal of journals in such disciplines as physiology, anatomy, genetics, neurobiology, pathology, psychology, developmental biology, and so on. One encounters scores of articles with titles like "Growth of Cottontail Rabbits (*Sylvilagus floridanus*) in Response to Ancillary Sodium,"[32] "Establishment and Maintenance of Claw Bilateral Asymmetry in Snapping Shrimps (*Alpheus heterochelis*),"[33] "Antibiotic Activity of Larval Saliva of *Vespula* wasps,"[34] and "Learning to Discriminate the Sex of Conspecifics in Male Japanese Quail (*Coturnix coturnix japonica*)."[35] The research discussed in these articles presupposes that there are natural laws concerning the "biological properties" characteristic of particular biological species. A challenge for philosophy of biology is to explain how this presupposition is compatible with evolutionary theory. (I have proposed an explanation.) This challenge cannot be recognized, let alone met, so long as philosophy of biology neglects these biological disciplines and attends primarily to evolutionary biology.

The approach I have sketched suggests that in such biological disciplines, the species are the sets of organisms that appear as *S*s in law statements of the form 'The *S* is *T*'. That is, they are the sets of organisms around which are built the inductive strategies in the best set for researchers in these fields to pursue; they are the sets of organisms that it is best for us to project over in inductively confirming the reliability of rules for predicting an organism's "biological properties." Admittedly, this suggestion remains programmatic in the absence of a fuller account of what makes one set of inductive strategies better than another. On the other hand, this proposal has the virtue of accounting for some features of the species concept in biological practice that otherwise often prove hard to accommodate, such as the use of species terms to characterize asexual organisms and the absence, even among sexually reproducing creatures, of any single criterion for conspecificity.[36] Moreover, though this proposal

[32] Clark D. McCreedy and Harman P. Weeks, Jr., *Journal of Mammalogy*, LXXIV, 1 (February 19, 1993): 217–24.

[33] R.E. Young, J. Pearce, C.K. Govind, *The Journal of Experimental Zoology*, CCLXIX, 4 (July 15, 1994): 319–26.

[34] Parker Gambino, *Journal of Invertebrate Pathology*, LXI, 1 (January 1993): 110–11.

[35] Susan Nash and Michael Domjan, *Journal of Experimental Psychology: Animal Behavior Processes*, XVII, 3 (July 1991): 342–53.

[36] See, e.g., Mayr, *Principles of Systematic Zoology* (New York: McGraw-Hill, 1969), p. 26; and Julian Huxley, "Introductory," Huxley, ed., *New Systematics* (New York: Oxford, 1940), pp. 1–46, esp. p. 11.

would make the reality of various biological species (in the sense used in physiology, developmental biology, and so on) dependent upon the range of data available to us, it seems to me that paleontologists have long recognized that the widespread presence of gaps in the fossil record is partly responsible for the utility of classifying fossil specimens into distinct species. There is some controversy over whether these gaps are inevitable in virtue of the nature of sedimentation and diagenesis,[37] or whether they result from the fact that evolutionary novelties usually arise in small marginal populations (hardly ever known as fossils) by evolution so rapid as to appear instantaneous in terms of detectable geological time.[38] In any event, most paleontologists agree that no amount of future fieldwork will ever alter the fact that within typical lineages, only segments displaying little evolutionary change are represented in the fossil record. Had this been otherwise, the best inductive strategies for us to pursue would have been different. As A. J. Cain[39] puts the point:

> The imperfections of the fossil record are very useful. Because of them, the known fossils of most groups also fall into rather discrete assemblages, and the hierarchical classification, devised originally for living forms, could therefore be applied without modification to fossils....But when good series are available, forms that seem to be good species at any one time may become indefinable since they are successive stages in a single evolutionary line and intergrade smoothly with each other....It is interesting to reflect on what system of classification might have been adopted if for some reason good series of fossils were so well known to mankind that living animals were recognized from earliest times as the present terminators of evolutionary series (*ibid.*, p. 107).

I suggested earlier that it is best for certain purposes to co-categorize all springs, and for other purposes to allow observations of iron "slinky"-type springs to bear on other such springs but not on balls of plastic wrap. Likewise, the biological species appropriate for work in physiology, developmental biology, and so on, where there are laws concerning particular species, may well not be the biological species appropriate for work in evolutionary biology, where there

[37] See J. Imbrie, "The Species Problem with Fossil Animals," Mayr, ed., *The Species Problem, American Association for the Advancement of Science Publication,* XL (1957), pp. 125–53; see pp. 142ff.

[38] N. Eldredge and S. Gould, "Punctuated Equilibria: An Alternative to Phyletic Gradualism," in T.J.M. Schopf, ed., *Models in Paleobiology* (San Francisco: Freeman and Cooper, 1972), pp. 82–115.

[39] *Animal Species and Their Evolution* (London: Hutchinson, 1954), pp. 107f.; see also pp. 112, 123. See also David Raup and Steven Stanley, *Principles of Paleontology* (San Francisco: Freeman, 1971), pp. 104f.

are laws of the form 'All biological species of...kind have...property' (for example, a law specifying that unisexual species are especially vulnerable to extinction).[40] But neither this, nor devastating critiques of phenetic taxonomy, nor the importance of the capacity to produce fertile offspring as an indicator of conspecificity should be allowed to obscure the fact (emphasized by Beckner,[41] Mayr,[42] and Simpson[43]) that in many branches of biology, the point of grouping various organisms together as conspecific is to enable us better to predict the biological properties of organisms as yet unexamined for those properties.

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[40] Kitcher advocates a similar view in "Species"; see esp. p. 320.
[41] *The Biological Way of Thought* (Berkeley: California UP, 1968), pp. 52f. and 62f.
[42] *Principles of Systematic Zoology*, pp. 78ff.
[43] *Principles of Animal Taxonomy* (New York: Columbia, 1961), p. 25.