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The autonomy of functional biology: a reply to Rosenberg

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Abstract. Rosenberg has recently argued that explanations supplied by (what he calls) ''functional biology'' are mere promissory notes for macromolecular adaptive explanations. Rosenberg's arguments currently constitute one of the most substantial challenges to the autonomy, irreducibility, and indispensability of the explanations supplied by functional biology. My responses to Rosenberg's arguments will generate a novel account of the autonomy of functional biology. This account will turn on the relations between counterfactuals, scientific explanations, and natural laws. Crucially, in their treatment of the laws' relation to counterfactuals, Rosenberg's arguments beg the question against the autonomy of functional biology. This relation is considerably more subtle than is suggested by familiar slogans such as ''Laws support counterfactuals; accidents don't.''

Introduction

Alex Rosenberg, in several recent papers (2001a, b, c), argues that functional explanations in biology are, at best, merely promissory notes for detailed natural-historical explanations that it is the aim of molecular genetics and developmental biology to supply. Functional explanations need to be completed, corrected, and sometimes superceded by less functional accounts until the level of molecular biology is reached, where complete explanations are available in terms of selection operating on organic chemistry. Functional explanations, on Rosenberg's view, do not constitute an autonomous, irreducible stratum of explanation.

As an example, Rosenberg (2001a: 154–160) discusses the fact that the wings of the ''buckeye butterfly'' (*Precis coenia*) have eyespots. A functional explanation of this fact would be that the butterfly's wings have eyespots in order to deter predators, who mistake the eyespots for a raptor's eyes. However, Rosenberg argues, the fact that eyespots deter the butterfly's potential predators is not a natural law. Hence, this fact is incapable of explaining why the butterfly has eyespots on its wings. The only laws of biology, Rosenberg says, are the principles of natural selection; this fact about the butterfly's potential predators is, at best, ''a distributed historical fact about some organisms on this planet around the present time and for several million years in both directions . . . One historical fact cannot by itself [i.e., without laws of nature to supply the requisite sort of necessity] explain another . . . '' (2001a: 155). Rosenberg writes:

> Any subdiscipline of biology – from paleontology to developmental biology to population biology to physiology or molecular biology – can uncover at best historical patterns, owing to the fact that . . . its generalizations will always be overtaken by evolutionary events. Each of the historically limited 'generalizations' of the 'The S is T' form in these disciplines [such as ''The butterfly *Precis coenia* has eyespots on its wings''] is itself to be explained by appeal to the operation of the principles of natural selection on local conditions . . . (2001a: 148; 2001b: 755)

The end of this passage alludes to the sort of explanation Rosenberg favors. Developmental molecular biologists have discovered that ''the genes and the entire regulatory pathway that . . . control anterior/posterior wing development in *Drosophila* (or its common ancestor with butterflies) have been recruited and modified to develop the eyespot . . . '' (2001a: 159). Molecular biologists elucidate the sequence of events by which this pathway was modified from wing-production to eyespot-development, and reveal that there weren't similar raw materials available for selection to transform into alternative defense mechanisms. Therefore, this account explains why eyespots rather than some other defense mechanism emerged – why eyespots had to arise, not merely how they could possibly have arisen. The explanations supplied by developmental molecular biology reflect the fact that ''in biology there is only natural history – the product of the laws of natural selection operating on macromolecular initial conditions'' (2001a: 161).

In this paper, I shall criticize Rosenberg's arguments and maintain that functional explanations are irreducible to the sorts of explanations that Rosenberg favors. Rosenberg's arguments, at bottom, turn out to depend on the special relation between laws of nature and counterfactual conditionals. I shall elaborate this relation more carefully than is usual, and use the result to argue that facts like ''The buckeye butterfly has eyespots'' are actually laws of (what Rosenberg calls) functional biology. By virtue of their special modal status, they possess a special explanatory power. In other words, the close relation between explanatory power and the power to support counterfactuals will be my means of arguing that functional explanations are irreducible in that they supply something having explanatory value that could not be supplied, even in principle, by the sorts of explanations that Rosenberg favors. Ultimately, then, my responses to Rosenberg's arguments will generate a novel account of the autonomy of functional biology.

Actual and possible arms races

Rosenberg argues that ''generalizations'' in ''functional biology,'' such as ''When *Drosophila* wings form at metamorphosis, the future ventral surface folds under the dorsal surface in the distal region'' (2001a: 155), are not genuine laws and so can underwrite only ''explanation sketches'' requiring completion and correction by macromolecular explanations, which apply the principles of natural selection, along with the laws of chemistry and physics, to initial macromolecular and environmental conditions. Here is Rosenberg's argument that ''generalizations'' in functional biology fail to rise to the level of laws:

> Take a simple example, such as ''[buckeye] butterflies have eyespots.'' The explanation for why they do is that eyespots distract birds from butterflies' more vulnerable and more nutritious parts, and provide camouflage when they give the appearance of the eyes of owls that prey on birds. This strategy for survival can be expected in the long run to put a premium on the development of ocular adaptations among birds, say the power to discriminate owl eyes from eyespots, that foil this stratagem for butterflies. This in turn will lead either to the extinction of eyespot butterflies or the development of still another adaptation to reduce predation by birds, say the development of an unappetizing taste, or shift in color to the markings of a butterfly that already tastes bad to birds. And in turn this stratagem will lead to a counter-stroke by the bird lineage. . . . The upshot is that to the extent that general laws must be timeless truths . . . no such laws are attainable in biology . . . . (2001a: 141; cf. 2001b: 739–740)

Let's try to unpack this.

Rosenberg's point here is *not* that variation is the norm – that because of the natural variation (on which selection operates) among members of the same species, there will never be a biologically interesting, exceptionless uniformity ''All S's are T'' among all members of a given species S at a given time. For the purposes of this argument, Rosenberg grants that a so-called generalization ''The S is T'' may hold of the members of S at a given moment even if some of the S's living at that time are not T. If the nonT's are not too numerous, then the generalization will be reliable enough for the purposes of anatomy, physiology, developmental biology, and so forth (2001b: 741, n.6).

Nearer to Rosenberg's real point is that eventually, under selection pressure, the adaptational ''moves'' and ''counter-moves'' in the ''arms race'' will either create a population of S's where the nonT's are so common that ''The S is T'' is no longer reliable enough, or the S will go extinct:

> If the ''space'' of adaptational ''moves'' and ''counter-moves'' is very large, and the time available for trying out these stratagems is long enough, every regularity in biology will be falsified (or turned into a stipulation) eventually. (2001a: 141; cf. 2001b: 739, 2001c: 367)

Well, *if* the time is long enough. But in fact, the time will surely not be long enough; extinction (of the species – indeed, of all life on earth) will occur long before all possible adaptational moves have been made. So there will be *some* biologically interesting generalizations ''The S is T'' that turn out to be true – that will, in fact, never be ''overtaken by evolutionary events.'' (Even more generalizations will never be overtaken within the time frame of interest to, say, medical studies of human physiology.)

Sure, you may say, but if ‘‘The S is T’’ turns out to be true, then that is only because some *possible* adaptational move happened never to have *actually* been made. Thus, ‘‘The S is T’’ is, at best, an accidental truth, a coincidence of natural history, not a law. It lacks ‘‘nomological necessity’’ (2001b: 753); it ‘‘do[es] not support counterfactuals in the way laws do’’ (2001b: 741). Here is the key counterfactual: Had a certain mutation occurred or a certain environmental influence been present, then ‘‘The S is T’’ might not still have held. Therefore, ‘‘The S is T’’ is not a law.

I believe this to be the heart of Rosenberg’s argument: not that ‘‘The S is T’’ *will* eventually be ‘‘overtaken by evolutionary events,’’ but that even if it isn’t, it *could* have been, and so is not a law. This argument deserves closer study.

Necessity and the special sciences

Rosenberg is correct about the key counterfactual: ‘‘The S is T’’ might not still have held, had certain mutations occurred or environmental influences been present. From the truth of this counterfactual, Rosenberg concludes that ‘‘The S is T’’ behaves like an accident rather than a law in connection with counterfactuals. It therefore must not be a law, and so it lacks the sort of necessity – ‘‘physical necessity’’ – that an explanatory generalization in science must possess. This argument presupposes that laws differ from accidents in their invariance under counterfactual suppositions. What is that difference precisely?

For example, it is true (but not a law) that all of the apples on my tree now are ripe. This accidental regularity would still have held, had I worn a green shirt this morning, or even had there been another apple on my tree. (However, it would not still have held, had there been more rainy days last summer.) Thus, although this regularity is accidental, there is a *range* of counterfactual suppositions under which it would still have held. How, then, are laws distinguished from accidents by their invariance under counterfactual suppositions?

It might be suggested that the *range* of counterfactual suppositions under which an accident is invariant must be *narrower* than a law’s range of invariance. But this is not the case. Suppose a large number of electrical wires, all made of copper, have been laid out on a table. Had copper been electrically insulating, then the wires on the table would have been useless for conducting electricity. Notice what just happened: the law that all copper is electrically conductive obviously wouldn’t still have held, had copper been electrically insulating. But this counterfactual supposition (in the envisioned conversational context) fails to undermine the accident that all of the wires on the table are made of copper. So the range of counterfactual suppositions under which an accident is preserved can in some respects extend *beyond* the range under which a law is preserved.

How, then, can Rosenberg tell that ‘‘The S is T’’ is not a law from the fact that ‘‘The S is T’’ would not still have held, under certain counterfactual suppositions? Many philosophers have said that laws supply reliable information on what the world would have been like had *p* been the case, for any counterfactual supposition *p* that is ‘‘physically possible,’’ i.e., logically consistent with every physical necessity. (The physical necessities are exactly the logical consequences of the laws.) The laws are invariant under all physically possible counterfactual suppositions. For example, had I missed my bus to work this morning, the natural laws would have been no different: I would have been unable to get to my office simply by making a wish and clicking my heels. Routinely, the laws are used to extrapolate what would have resulted from different initial conditions. In contrast, no accidental generalization is preserved under *every* physically possible counterfactual supposition. (This is trivial: the accident *g* would not still have held, had not-*g* obtained – and not-*g* is physically possible if *g* is accidental.) In other words:

> *Nomic Preservation (NP)*: *g* is physically necessary if and only if *g* would still have held had *p* obtained, for every *p* that is logically consistent with every physical necessity.

Now we can plug in Rosenberg’s argument: It is not the case that ‘‘The S is T’’ would still have held no matter what mutations had occurred or what environmental conditions had obtained. Therefore, by *NP*, ‘‘The S is T’’ is not physically necessary.

However, this conclusion follows from *NP* only if it is physically possible for those mutations or environmental conditions to occur under which ‘‘The S is T’’ is not invariant. Undoubtedly, the laws *of physics* and the principles of natural selection are logically consistent with the occurrence of these mutations and environmental conditions. But to argue in this way that ‘‘The S is T’’ is not a law of functional biology – that is, to take as a premise that the laws of physics and the principles of natural selection constitute all of the laws there are – amounts to reasoning in a circle: to arguing that there are no laws of functional biology by presupposing that there are no such laws (Lange, 2000: 230).

Here is another way to make this point. We can think of *NP* as a general schema. For the laws of physics, it becomes: The laws of physics would still have held under all counterfactual suppositions that are logically consistent with the laws of physics. In other words,

> *NP°*: *g* is one of the laws *of physics* (or a logical consequence of those laws) if and only if *g* would still have held had *p* obtained, for every *p* that is logically consistent with the laws *of physics*.

But how should *NP°* be extended to determine what it would take for there to be laws of functional biology? The italicized bits in *NP°* might be replaced in either of two ways:

> *g* is one of the laws *of functional biology* (or a logical consequence of those laws) if and only if *g* would still have held had *p* obtained, for every *p* that is logically consistent with
>
> *NP′*: the laws *of physics*
>
> *NP″*: the laws *of functional biology*

NP′ requires that any law of functional biology have the same range of invariance as – and, hence, the same sort of necessity as – the laws of physics. On the other hand, *NP″* permits the range of invariance that *g* must possess, in order to qualify as *necessary* for the purposes of functional biology, to be distinct from the range of invariance characteristic of the laws of physics. Rosenberg's argument requires *NP′*. But Rosenberg offers no argument for accepting *NP′* rather than *NP″*.

An advocate of *NP′* might argue that (as Rosenberg says) ''we are all physicalists now'' and so regard the laws of physics plus the initial conditions of the universe as fixing (in some sense) all of the facts about the universe, including the laws of functional biology (if any). I grant that according to physicalism of a familiar kind, the (approximate) *truth* of ''The S is T'' is fixed by the laws and initial conditions of physics. But our concern is with whether its *lawhood* is determined by physics. That depends on the range of counterfactual suppositions under which some *g* must be invariant in order for *g* to qualify as a law of functional biology. Perhaps, to qualify as a law of functional biology, *g* must be invariant under certain counterfactual suppositions that would *violate* some laws of physics. In that case, it is difficult to see how the laws and initial conditions of physics could suffice to fix *g*'s status as a law or accident of functional biology. I see no way to argue that this is not the case merely from physicalist intuitions (e.g., that there is no *élan vital*, that all entities obey the laws of physics, etc.).

An advocate of *NP′* might insist that any law of functional biology must display exactly the same invariance under counterfactual suppositions as a law of physics. On the other hand, an advocate of *NP″* might insist that *NP′* gives the laws of physics undue influence in determining the qualifications for a law of functional biology. To decide between *NP′* and *NP″*, we must step back and examine more generally the relation between laws and counterfactuals. By basing our argument for functional-biology laws on general considerations regarding laws and counterfactuals, we avoid Rosenberg's charge against some philosophers that they tendentiously ''redefine the meaning of scientific law to accommodate the explanatory devices of biology'' (2001b: 743).

Laws and their stability

According to *NP*, the laws would all still have held under any counterfactual supposition that is logically consistent with the laws. No accident is always preserved under all of these suppositions. But *NP* alone cannot save our intuition that the physical necessities possess an especially great power to support counterfactuals. That's because the *range* of counterfactual suppositions under consideration in *NP* (namely, the physical possibilities) has been designed expressly to suit the physical necessities.

What if we extend the same courtesy to a set containing accidents, allowing it to pick out a range of counterfactual suppositions especially convenient to itself: those suppositions that are logically consistent with every member of that set? Take a logically closed set of truths that includes the accident that all of the wires on the table are copper but omits the accident that all of the apples on my tree are ripe. Here's a counterfactual supposition that is consistent with every member of this set: had either some wire on the table *not* been made of copper or some apple on the tree *not* been ripe. What would the world then have been like? In many conversational contexts, we would deny that the generalization in the set (the one about the wires) would still have held. (Perhaps it is the case, of neither generalization, that it would still have held.)

The same sort of argument could presumably be made regarding any logically closed set of truths that includes *some* accidents but not *all* of them. Given the opportunity to pick out the range of counterfactual suppositions convenient to itself, the set nevertheless is not invariant under all of those suppositions. (Trivially, every member of the set of *all* truths would still have held under any counterfactual supposition logically consistent with all of them, since *no* counterfactual supposition is so consistent.)

Here, then, is my preliminary suggestion for the laws' distinctive relation to counterfactuals. Take a non-empty, logically closed set of sentences. Take the counterfactual suppositions *p* that are logically consistent with every member of the set. Call the set *stable* exactly when every member *g* of the set not only is true, but also would still have been true had *p* been the case, for each of these *p*'s. Then *g* is a logical consequence of the laws exactly when *g* belongs to a non-trivially stable set.

According to *NP°*, the laws of physics generate a stable set. As I just argued, no set containing an accident of physics is stable, except for the set of all truths, which is trivially so. What makes the laws of physics (and their logical consequences) special is their stability: that *taken as a set*, they are invariant under as broad a range of counterfactual suppositions as they *could* logically possibly be. *All* of the laws of physics would still have held under *every* counterfactual supposition under which they *could all* still have held. No set containing an accident of physics can make that boast non-trivially (Lange 2000).

The logical necessities and the set of all truths are trivially stable. The set generated by the laws of physics is stable non-trivially. Because it is non-trivially as invariant under counterfactual perturbations as it could be, there is a sense of *necessity* corresponding to it; necessity involves possessing a *maximal* degree of invariance under counterfactual perturbations. No sense of necessity corresponds to an accident, even to one that would still have held under many counterfactual suppositions. The notion of "stability" allows us to draw a sharp distinction between laws and accidents. It also gives us a way out of the notorious circle that results from specifying the physical necessities as the truths that would still have held under those counterfactual suppositions that are logically consistent with the physical necessities.

If *g* is a logical consequence of the laws exactly when *g* belongs to a non-trivially stable set, then the laws of physics have no privileged role in picking out the range of counterfactual suppositions under which *g* must be invariant in order for *g* to be a logical consequence of the laws. Rather, any set of truths picks out for itself the range of counterfactual suppositions under which that set's members must all be invariant in order for that set to be necessary in the manner of laws. We now have something much more like *NP″* than *NP′*.

To see how the laws of a ‘‘special science’’ may fail to be laws of physics, we must go beyond stability *simpliciter* (which, as I have just explained, distinguishes the laws of physics) and consider what it would be for a set to be stable *for the purposes of a given scientific field*. Such stability requires, to begin with, that the set's members all be *reliable* – that is, close enough to the truth for that field's purposes. (Recall Rosenberg's concession that ‘‘The S is T’’ may be reliable for the purposes of anatomy, physiology, and so forth even if a few S's are not T.) Here I am following Mill:

> It may happen that the greater causes, those on which the principal part of the phenomena depends, are within the reach of observation and measurement . . . But inasmuch as other, perhaps many other causes, separately insignificant in their effects, co-operate or conflict in many or in all cases with those greater causes, the effect, accordingly, presents more or less of aberration from what would be produced by the greater causes alone. . . . It is thus, for example, with the theory of the tides. No one doubts that Tidology . . . is really a science. As much of the phenomena as depends on the attraction of the sun and moon . . . may be foretold with certainty; and the far greater part of the phenomena depends on these causes. But circumstances of a local or casual nature, such as the configuration of the bottom of the ocean, the degree of confinement from shores, the direction of the wind, &c., influence in many or in all places the height and time of the tide . . . General laws may be laid down respecting the tides; predictions may be founded on those laws, and the result will in the main . . . correspond to the predictions. And this is, or ought to be meant by those who speak of sciences which are not *exact* sciences. (1961, 6.3.1, pp. 552–553)

For *g* to be ‘‘reliable,’’ it must reflect all of what Mill calls the ‘‘greater causes.’’ But it may neglect a host of petty influences. For example, classical physics might suffice for the purposes of human physiology or marketing; relativistic corrections are negligible. (I will discuss another example more fully in the next section.) Thus, since the laws of physics reflect relativity whereas the laws of physiology do not, laws from distinct fields can, strictly speaking, be inconsistent.

This entails that happily, to discover the laws of physiology, scientists did not first have to discover all of the most fundamental laws of physics. However, it might be objected that I have now opened the door to a radical relativism about natural law. Admittedly, as we shall see, a law of an ‘‘inexact science’’ may be merely an accident of physics. But to qualify as ‘‘reliable’’ for a given field's purposes, it does not suffice for the set's members merely to give sufficiently accurate empirical predictions. They must also capture the ‘‘greater causes.’’ Thus, if there is no such thing as phlogiston, then even if the ‘‘laws’’ of phlogiston theory give sufficiently accurate predictions for the purposes of combustion chemistry, the ‘‘phlogiston’’ laws are not genuine natural laws – of any field. Laws must be close enough to the truth not merely as instruments for making empirical predictions, but for the field's purposes, which include giving scientific explanations.

Admittedly, a good deal more needs to be said about ''reliability.'' I shall return to the relation between laws of physics and laws of an ''inexact science.'' But my main concern now is the range of invariance that the members of a set must exhibit in order for that set to be stable for the purposes of a given scientific field. Since that field's concerns may be limited, certain claims and counterfactual suppositions may lie outside of the field's interests. A logically closed set is stable *for the purposes of a given science*, and hence its members are physical necessities *for that field*, if and only if all of its members not only are of interest to the field and reliable for the field's purposes, but also would still have been reliable, for the field's purposes, under every counterfactual supposition of interest to the field *and* consistent with the set.

Eventually, we must examine whether, according to this formula, there are laws of functional biology. But first let's come to a better understanding of how the notion of ''stability'' applies to an inexact science, such as Mill's Tidology.

A worked example

Let's try applying this notion to an inexact science – say, to island biogeography (IB), which deals with the abundance, distribution, and evolution of species living on separated patches of habitat. It has been suggested that *ceteris paribus*, the equilibrium number S of species of a given taxonomic group on an ''island'' (as far as creatures of that group are concerned) increases with the island's area A in accordance with a power law: $S = cA^z$. The (positive-valued) constants c and z are specific to the taxonomic group and island group – e.g., Indonesian land birds or Antillean beetles. One theory purporting to explain this ''area law'' (the ''equilibrium theory of IB,'' developed by Robert MacArthur and E.O. Wilson) is roughly that a larger island tends to have larger available habitats for its species, so it can support larger populations of them, making chance extinctions less likely. Larger islands also present larger targets for stray creatures. Therefore, larger islands have larger immigration rates and lower extinction rates, and so tend to equilibrate at higher biodiversity.

Nevertheless, a smaller island nearer the ''mainland'' may have greater biodiversity than a larger island farther away. This factor is covered by the *ceteris paribus* qualifier to the ''area law.'' Likewise, a smaller island with greater habitat heterogeneity may support greater biodiversity than a larger, more homogeneous island. This factor is also covered by ''*ceteris paribus*.'' And there are others. Nevertheless, to discover the ''area law,'' ecologists did not need to identify *every* factor that may cause deviations from $S = cA^z$, only Mill's ''greater causes.'' They are supposed to suffice for the area law to yield predictions good enough for certain sorts of applications, theoretical and practical, from planning nature reserves to serving as the first step in constructing divers ecological models.

Assume (for the sake of argument) that the ''area law'' is indeed accurate enough (i.e., reliable) for IB purposes. What must its range of invariance be in order for it to count as a law of IB – in order, that is, for it to be *necessary* in a relevant sense? There are counterfactual suppositions under which the laws of physics would still have held, but under which the ‘‘area law’’ would not still have held. For example, had Earth always lacked a magnetic field, then cosmic rays would have bombarded all latitudes, which might well have prevented life from arising, in which case *S* would have been zero irrespective of *A*. Here’s another counterfactual supposition: Had evolutionary history proceeded differently so that many species developed with the sorts of flight, orientation, and navigation capacities possessed by actual airplanes. (This supposition, albeit rather outlandish, is nevertheless consistent with the laws of physics since airplanes exist.) Under this supposition, the ‘‘area law’’ might not still have held, since an island’s size as a target for stray creatures might then have made little difference to its immigration rate. (Creatures without the elaborate organs for flight and navigation could have hitched rides on those so equipped.)

Unlike the laws of physics, IB generalizations are not preserved under every counterfactual supposition consistent with the laws of physics. But from this, we cannot conclude that such generalizations fail to qualify as IB laws. That argument presupposes that a law *of island biogeography* would have to withstand the very same range of counterfactual perturbations as a law of physics. As we have seen, this presupposition begs the question against the possibility of IB laws.

The ‘‘area law’’ is not prevented from qualifying as an IB law (i.e., from belonging to a set that is stable for IB purposes) by its failure to be preserved under the two counterfactual suppositions I just mentioned, although each of these suppositions is consistent with the laws of physics. The first supposition (concerning Earth’s magnetic field) falls outside of IB’s range of interests. It twiddles with a parameter that IB takes no notice of or, at least, does not take as a variable. Of course, IB draws on geology, especially paleoclimatology and plate techtonics. Magnetic reversals are crucial evidence for continental drift. But this does not demand that IB be concerned with how species would have been distributed had Earth’s basic physical constitution been different. Biogeographers are interested in how species would have been distributed had (say) Gonwanaland not broken up, and in how Montserrat’s biodiversity would have been affected had the island been (say) half as large. On the other hand, IB is not responsible for determining how species would have been distributed had (say) Earth failed to have had the Moon knocked out of it by a cataclysm early in its history. (Earth’s rotation rate would then have been greater, its tides would have been less, and the level of carbon dioxide in its atmosphere would have been greater.) Biogeographers do not need to be geophysicists.

The second counterfactual supposition I mentioned (positing many species capable of covering long distances over unfamiliar terrain nearly as safely as short distances over familiar territory) is logically inconsistent with other generalizations that would join the ‘‘area law’’ in forming a set stable for IB purposes. For example, the ‘‘distance law’’ says that *ceteris paribus*, islands farther from the mainland equilibrate at lower biodiversity. Underlying both the area and distance laws are various constraints: that creatures travel along continuous paths, that the difficulty of crossing a gap in the creature's natural habitat increases smoothly with the gap's size (*ceteris paribus*), and so forth. These "continuity principles" (MacArthur 1972: 59–60) must join the area and distance laws in the set that is stable for IB purposes.

The area law's *ceteris-paribus* clause does *not* need to rule out exceptions to these constraints. Although it isn't the case that the area law would still have held, had these constraints been violated, the area law's range of invariance under counterfactual suppositions may suffice for it to qualify as an IB law because *other* IB laws, expressing these constraints, make violations of these constraints physically impossible (as far as IB is concerned). Here's an analogy. Take the Lorentz force law: In magnetic field $\boldsymbol{B}$, a point body with electric charge q and velocity $\boldsymbol{v}$ feels a magnetic force $\boldsymbol{F} = (q/c)\boldsymbol{v} \times \boldsymbol{B}$. Presumably, it isn't the case that this law would still have held, had bodies been able to be accelerated beyond c. But this law requires no proviso limiting its application to cases where bodies fail to be accelerated beyond c. That's not because there are actually no superluminal accelerations, since a law must hold not merely of the actual world, but also of certain possible worlds. The proviso is unnecessary because *other* laws of physics deem superluminal acceleration to be physically impossible (as far as physics is concerned). Hence, the Lorentz force law can belong to a stable set – can have the range of invariance demanded of a law of physics – without being preserved under counterfactual suppositions positing superluminal accelerations.

Of course, there may actually be *no* IB laws. Perhaps only a case-by-case approach makes approximately accurate predictions regarding island biodiversity. Perhaps there aren't just a few "greater causes," but myriad major influences: weather and current patterns, the archipelago's arrangement, differences between island and mainland conditions, an island's habitat heterogeneity, the presence on the island of predators and parasites on potential colonists, the choices made by individual creatures, rare storms promoting immigration of species with low dispersal capacities, etc. It's an open *scientific* question whether there are IB laws. I am merely trying to explicate what it would *take* for IB to have its own set of laws: to be *autonomous*.

A set that is IB stable need not include all of the laws of physics. The *gross* features of the laws of physics captured by constraints like those I've mentioned, along with the other IB laws and the field's interests, may suffice *without the fundamental laws of physics* to limit the relevant range of counterfactual suppositions. The area law would still have held had there been (e.g.) birds equipped with modest anti-gravity organs, assisting in takeoffs. The factors affecting species dispersal would have been unchanged: for example, smaller islands would still have presented smaller targets to off-course birds and so accumulated fewer strays, *ceteris paribus*. Likewise, the area law would still have held had material bodies consisted of some continuous rigid substance rather than corpuscles. The IB laws's range of stability may thus in places extend *beyond* the range of stability of the laws of physics; the island-biogeographical laws don't reflect every *detail* of the laws of physics.

This is a crucial point. The IB laws's necessity corresponds to their range of stability. But that range is not wholly contained within the range of stability of the laws of physics (since, as we have just seen, it includes some counterfactual suppositions that violate the laws of physics). Consequently, the stability of the laws of physics cannot be responsible for the IB laws's stability for IB purposes. The IB laws do not inherit their *necessity* from the laws of physics. The kind of necessity characteristic of IB laws is *not possessed* by the laws of physics (since the laws of physics are not invariant under all of the counterfactual suppositions within the IB laws' range of stability). Admittedly (as I mentioned in discussing ''physicalism'' in section Laws and their stabilities), the approximate *truth* of IB laws might well follow from the laws of physics and certain initial conditions that are accidents of physics. The IB laws would then be *reducible* (in an important sense) to physics. Nevertheless, the *lawhood* (as distinct from the *truth*) of IB laws – their stability for IB's purposes – *cannot* follow from laws and initial conditions of physics. The stability of IB laws depends on their remaining reliable under certain counterfactual suppositions *violating* laws of physics. The laws of physics obviously cannot be responsible for the area law's remaining reliable under those counterfactual suppositions.

Hence, if there turned out to be IB laws, IB would have an important kind of *autonomy*. Because the IB laws' lawhood would be irreducible to the lawhood of the fundamental laws of physics (and initial conditions), IB's nomological explanations (of, for instance, Mauritius's biodiversity) would be irreducible to the explanations of the same phenomena at a more microphysical level (See Lange, 2000).

In other words, there would be two different explanations of why (say) *n* species of land bird currently inhabit Mauritius. One explanation would proceed on the macro level, using IB laws and Mauritius's area, distance from the mainland, and so forth, to explain why there are *n* species rather than many more or fewer. The other explanation would proceed on the micro level, by explaining the fates of various individual creatures that might have migrated to Mauritius and left descendants. (That we could never in practice discover all of these details does not alter the fact that this would be an explanation, as Rosenberg (2001b: 756) recognizes.) This micro account explains not merely what the macro account explains (why Mauritius is currently inhabited by *n* species rather than *many* more or fewer), but also why Mauritius is currently inhabited by *n* species rather than *one* more or fewer – and, indeed, why Mauritius is inhabited by those particular *n* species rather than a different combination. (Note the differences in contrast classes.) However, it does not follow that the macro account is merely a rough sketch of or promissory note for the micro account. On the contrary, the macro account includes explanatorily relevant information omitted from the micro account, despite its rich detail. In particular, the micro account does not say that Mauritius's biodiversity would have been nearly the same even if, say, the stock of potential migrants (the mainland species of birds) had been very different – indeed, even if some of those species had been made of continuous rigid substance or had possessed anti-gravity organs assisting slightly in takeoffs. The IB laws would then still have applied.

A micro account explaining merely the *actual* fates of individual potential migrants does *not* reveal that Mauritius would still have been inhabited by roughly *n* species even under slightly different initial conditions. However, a fuller micro account details not only the *actual* fates of various potential migrants, but also what their fates *would have been*, under various counterfactual circumstances. This account reveals that Mauritius would still have been inhabited by roughly *n* species even if, say, some long-ago storm had not occurred to deflect a given bird to Mauritius. So (it might be argued) the macro account supplies nothing that could not be supplied, at least in principle, by a full micro account.

This objection afflicts various other proposals (e.g., Garfinkel 1981; Sober 1984; Jackson and Pettit 1992) according to which macro accounts supply counterfactual explanatory information that micro accounts cannot supply, even in principle. These proposals focus on the omission from the micro account of the fact that the same macro outcome would still have resulted, even under certain other initial conditions that are *physically possible* as far as the laws of physics are concerned. This information about counterfactuals nevertheless follows from the laws of physics and a full statement of those counterfactual initial conditions, since as we have seen, the laws of physics would still have held under any initial conditions that do not violate them. My account, in contrast, treats not only these initial conditions but also initial conditions that *violate* the laws of physics. That the same macro outcome would still have resulted, even under certain of these initial conditions, is explanatorily relevant information that does *not* follow from the laws of physics and a full statement of counterfactual initial conditions, since the laws of physics would *not* all still have held under these counterfactual initial conditions. The macro explanation thus reveals the macro result to be inevitable in a sense that no microphysical account, however full, could comprehend. Fortified with the notion of a set that's stable for the purposes of a given inexact science, we can see how various macro-level generalizations come to possess a distinctive sort of necessity and explanatory power.

As far as IB is concerned, the fact that there are no birds equipped with modest antigravity organs or made of continuous rigid substance is merely an accident of the actual world (like the occurrence of the long-ago storm that deflected a given bird to Mauritius). The macro outcome is insensitive to this accident. The IB explanation of Mauritius's biodiversity uniquely supplies this information.

The MacArthur-Wilson equilibrium theory in IB is typical of many biological models and idealizations. I could just as well have discussed the Hardy-Weinberg law, the logistic equation of population growth, or the Wright/Fisher model of selection. For that matter, I could have discussed macro-level explanations from thermodynamics or economics. All are idealizations that are reliable (for certain purposes), despite including only the "greater causes," and that would still have been reliable under a range of counterfactual suppositions that includes some violations of the laws of physics.

Functional biology

Let's now apply these lessons to the case of functional biology. As Rosenberg notes, some ‘‘The S is T’’ generalizations are reliable for the purposes of functional biology: ‘‘we can predictively rely on’’ them in physiology, medicine, cellular biology, and so forth ‘‘because nature is acting on a time-scale slow enough for us safely to neglect the chance of ‘arms race’ changes in biological processes local to us’’ (2001b: 741, n.6). These ‘‘temporary empirical regularities,’’ Rosenberg (2001c: 368) recognizes, might hold for millions of years, far longer than the period that interests medical science. Could reliable ‘‘The S is T’’ generalizations form a set that is stable for the purposes of functional biology? If so, then as necessities of functional biology, these generalizations could ground scientific explanations. As with the putative IB laws, these ‘‘The S is T’’ generalizations might exhibit a range of stability under counterfactual suppositions that extends in some respects beyond the range of stability exhibited by the laws of physics. In that case, the explanations supplied by functional biology would be irreducible to the sort of micro level explanations Rosenberg favors.

How, in functional biology, do counterfactuals come to be entertained in the first place? Discussion of a particular case might provoke one: a physician might say that the shooting victim would not have survived even if he had been brought to the hospital sooner, since the bullet punctured his aorta and the human aorta carries all of the body’s oxygenated blood from the heart to the systemic circulation. (This fact about the human aorta would still have held, had the victim been brought to the hospital sooner.) Counterfactuals may also arise in connection with functional explanations, such as that the human trachea has cartilaginous rings in order to make it rigid and so keep it from collapsing between breaths.[1] This explanation depends on the counterfactual ‘‘There would be no such rings if they didn’t make the trachea rigid.’’ Likewise, that the rings’ effect of making the trachea’s outer surface white does not explain the rings’ presence is bound up with the counterfactual ‘‘Were cartilage bright blue instead of white, the human trachea would still have had cartilaginous rings.’’

Notoriously, counterfactuals are context dependent. In Quine’s famous example, the counterfactual ‘‘Had Caesar been in command in Korea, he would have used the atomic bomb’’ is correct in some contexts, whereas in others, ‘‘ . . . he would have used catapults’’ is correct. What is preserved under a counterfactual supposition, and what is allowed to vary, depends on our interests in entertaining the supposition. Physiology, embryology, and other branches of ‘‘functional biology’’ involve a limited range of interests, and these limits are reflected in which counterfactuals are

[1] Here the explanandum itself takes the form of ‘‘The S is T.’’ A functional explanation could also be given of the fact that Jane’s trachea has cartilaginous rings: Jane is a human being, and the human trachea has cartilaginous rings in order to keep it from collapsing between breaths. Contrary to Rosenberg (2001a: 155), the functional explanation of the fact that a given individual buckeye butterfly’s wings possess eyespots is not that this individual’s wings have eyespots because all members of its species do. There’s no reference to function in that. Rather, this individual has eyespots because it is a butterfly of the buckeye species and this species uses eyespots to fool predators. Explanations of this sort are quite ordinary; at the zoo, a child might point to a bird and ask, ‘‘Why did he do that?’’ and an adult might properly reply, ‘‘That’s how pelicans eat.’’

correct in a functional biology context. Consider ''Were cartilage bright blue instead of white.'' In a context concerned with evolutionary history, it is *incorrect* to say (as we should in a functional biology context) '' . . . then the human trachea would still have had cartilaginous rings.'' For if cartilage had been bright blue instead of white, different selection pressures might have acted upon various creatures of eons past with cartilaginous parts that are visible to predators. Evolutionary history might thus have taken a different path, and so human anatomy might have been different; the human trachea might have sported no cartilaginous rings, or the human being might have possessed no trachea at all. Similarly, in a context concerned with molecular structure and the laws of physics, the counterfactual supposition ''Were cartilage bright blue instead of white'' demands changes of some sort either in the chemical structure of cartilage or in the laws governing light's interaction with molecules. All bets are off as to what the human trachea (if any) would then have been like. In functional biology contexts, though, it is correct to say that were cartilage blue instead of white, the human trachea would still have had cartilaginous rings. The counterfactual supposition, entertained in this context, should not lead us to contemplate how cartilage could have managed to be blue.

Likewise, it is of medical interest to know whether a given heart attack might have been less serious had epinephrine been administered sooner, or had the patient long been engaged in a vigorous exercise regimen, or had she been wearing a red shirt, or had the Moon been waxing. But it is not of medical interest to know whether the heart attack might have been less serious had human beings evolved under some different selection pressure. A physician might blame a patient's untimely death on her smoking, but not on human evolutionary history. Just as IB does not take Earth's basic physical constitution as a variable, so medicine does not take human evolutionary history as a variable.

In this light, reconsider Rosenberg's point that had a certain mutation occurred or a certain environmental influence been present sometime ago, then by now, ''The S is T'' would have been overtaken by evolutionary events. By the argument I have just given, this is not the sort of counterfactual supposition with which functional biology is concerned. Therefore, the failure of reliable ''The S is T'' generalizations to be preserved under such suppositions does not prevent their forming a set that is stable for the purposes of functional biology. Recall from section Laws and their stabilities that a closed set of generalizations that are of interest to the scientific field and reliable for its purposes qualifies as *stable for the field's purposes* if and only if its members would still have been reliable, for the field's purposes, under every counterfactual supposition *of interest to the field* and consistent with the set.

Rosenberg (2001a: 158) correctly points out that many different structures could have performed the same function. We might want to explain why the buckeye butterfly has eyespots rather than (say) tasting foul to birds. To answer this why-question, it does not suffice to say ''The eyespot discourages predation by birds.'' Rather, we would need to discover the sort of explanation that Rosenberg favors. Likewise, IB fails to explain why one particular combination of *n* species rather than some other inhabits Mauritius. Nevertheless, IB explains why *n* species rather than many more or fewer inhabit Mauritius. Likewise, that the eyespot discourages predation explains why the butterfly has the eyespot rather than having no eyespot but otherwise being exactly as it actually is.

Rosenberg might be willing to concede this point but to deem such an explanation marginal to real biology:

> The reductionist may admit that there are contexts of inquiry in which how-possible answers to questions satisfy explanatory needs. But the reductionist will insist that in the context of advanced biological inquiry, as opposed say to secondary school biology instruction, for example, the how-possibly question either does not arise, or having arisen in a past stage of inquiry, no longer does. (2001a: 154)

But I see no grounds for regarding one contrast class (having no eyespot but otherwise being the same) as less befitting ''advanced biological inquiry'' than the other contrast class (having a different defense mechanism). Indeed, it is only in light of a particular answer to the question ''Why does the butterfly have an eyespot rather than having no eyespot but being otherwise the same?,'' namely, ''Because the eyespot is a defense mechanism,'' that it makes sense to ask why the butterfly employs this particular defense mechanism instead of some other. So even when biologists are pursuing an answer to the latter question, they do not dismiss the former question as outdated or naïve; it remains fully relevant.

Had the buckeye butterfly tasted foul to birds, then it might not have sported eyespots. Here we have a counterfactual supposition of interest to functional biology, but under which a reliable ''The S is T'' generalization is not preserved. However, this result does not undermine the stability (for functional biology) of the set of reliable ''The S is T'' generalizations. That is because the counterfactual supposition (''Had the buckeye butterfly tasted foul to birds'') is itself logically inconsistent with some member of the set (namely, that the buckeye butterfly does not taste foul to birds). Again, a closed set of reliable generalizations is ''stable'' for the purposes of a given scientific field if and only if its members would still have been reliable, for the field's purposes, under every counterfactual supposition of interest to the field and *consistent with the set*.

In a functional explanation, one ''The S is T'' generalization (such as ''The human trachea has cartilaginous rings'') is explained by appeal to others (such as ''The human trachea does not collapse between breaths''). The autonomy of functional explanations, on my view, reflects the fact that the range of stability exhibited in functional biology by ''The S is T'' generalizations extends in some respects beyond the range of stability exhibited in physics by the laws grounding the sorts of explanations that Rosenberg favors. Thus, ''The S is T'' generalizations possess a distinct kind of *necessity* in functional biology.

Take the explanation that the vulture has no feathers on its head and neck because the vulture feeds by sticking its head and neck deep inside the bodies of carrion, so any feathers there would become matted and dirty. This explanation is independent of the details of the laws of physics. Putnam uses a similar example to defend the irreducibility of macro explanations: why a cubical peg, 15/16'' on a side, cannot fit into a round hole 1'' in diameter. Putnam writes:

> The explanation is that the board is rigid, the peg is rigid, and as a matter of geometric fact, the round hole is smaller than the peg. ... That is a correct explanation whether the peg consists of molecules, or continuous rigid substance, or whatever. (1975: 296)

A peg (or vulture) made of continuous rigid substance would violate laws of physics. But the same functional explanation would apply to it. That distinctive range of invariance reflects the irreducibility of this kind of explanation to anything that could be supplied, even in principle, by the laws of physics.[2]

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[2] The interpretation I have offered of Putnam's remarks is somewhat different from what is found in the literature – see, e.g., Sober 1993: 75. Putnam (personal communication) has endorsed my interpretation of his argument, pointing out that in a recent work (1999: 150), he has characterized different explanations of the same phenomenon as generalizing to different classes of cases. My notion of different explanatory principles remaining invariant under different classes of counterfactual perturbations is a version of this idea.