BIOLOGICAL LAWHOOD: A CRITIQUE OF SOME CURRENT THOUGHT

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INTRODUCTION

If you've read anything lately on the subject of laws in biology, chances are it hasn't painted a very rosy picture. Restricting our attention to the most influential views (the ones I'll be looking at in this paper), either biological laws don't exist (Beatty 1995), or they exist along a continuum of generalizations which includes the fact that all the coins in Goodman's pocket are copper (Mitchell 2000), or they exist but are not particularly interesting from a philosophical perspective (Woodward 2001), or, whether they do or not, it "is of little relevance for the working biologist" (Mayr 1982: 32).

This paper is an attempt to clear some brush in the hopes of providing a comfortable space for an account of biological lawhood.[1] Contrary to Ernst Mayr's oft-cited quip, whether there are laws in biology *is* relevant for the working biologist, so long as that work includes things like hypothesis formation, prediction, explanation, inductive confirmation, and counterfactual reasoning (which it does, if it's like most scientific work.) But before biological laws can have a fair hearing, we need to combat some of the nay-saying and marginalizing that has poisoned the jury. In this regard, I hope to remove the central motivations for the positions defended above. Having done so, we can move towards a positive account of biological lawhood, one that (a) sets biological laws apart from accidents, (b) is philosophically interesting in terms of its ability to enhance our understanding of scientific practice, and (c) is something in which the working biologist should have a vested interest.

1. BEATTY'S "EVOLUTIONARY CONTINGENCY THESIS"

Beatty (1995) has argued that to the extent that a generalization is "distinctly biological" (i.e., not a truth about biota that can be expressed purely in the language of physics or chemistry [or any other discipline]), that generalization describes a state of affairs which is the product of the vicissitudes of evolutionary processes — an "evolutionarily contingent" outcome.

> What does it mean to say that such generalizations describe evolutionarily *contingent* states of affairs? This has to do with the *rule-breaking*

[1] Developed in my companion piece, "From Scientific Possibility to Natural Necessity"

> capabilities of the agents of evolutionary change: the agents of evolution not only make, but also break the rules that pertain to the living world. More formally, to say that biological generalizations are evolutionarily contingent is to say that they are not laws of nature — they do not express any *natural necessity*; they may be true, but nothing in nature necessitates their truth (Beatty 1995: 51-52; his emphasis).

Since "nature fails to necessitate" the truth of biological generalizations, Beatty continues, those generalizations fail to express laws of nature (*ibid*.: 53).

The remainder of this section will be devoted to establishing two points against Beatty. The first point will be that Beatty's conception of what makes a truth naturally necessary is too underdescribed to undermine the idea that there could be distinctly biological laws (hereafter simply "biological laws"). To the extent that it is clear, it is too demanding. In particular, what I will argue is that, for all Beatty has said, what precludes biological generalizations from achieving lawhood may similarly strip the laws of fundamental physics of their status as laws. The resulting dilemma is that either (a) there are no such things as laws, or (b) failing to meet Beatty's standards for being naturally necessary does not itself preclude a certain (biological or physical) fact's being a law. Beatty would presumably be happy to embrace (a). But for people who think laws are important to science, (b) provides some protection from Beatty's formidable and influential argument.

The second point relates to Beatty's worry that the possibility that some biological rules might be broken — that some true biological generalizations might stop being true — shows that there could be nothing necessary about the truth of biological generalizations. Here I'll argue along lines similar to those mentioned above — namely, that we regularly dole out labels of "natural necessity" to generalizations in other domains of science that can stop being true. Thus we have little cause for arguing against the possibility of biological laws on these grounds.

1.1. Necessities and Contingencies

Evolutionary and Cosmic

Let us begin with Reichenbach's famous contrasting generalizations:

(1) All solid spheres of enriched uranium (U_{235}) have a diameter of less than one hundred meters.

(2) All solid spheres of gold (Au) have a diameter of less than one hundred meters.

Assume both generalizations are true. Beatty suggests that "there seems to be more to the former than the latter, by virtue of which we might accord the former but not the latter the status of law…That something more has to do with what nature necessitates or precludes" (Beatty 1995: 52-53). Specifically, what nature precludes is that there could be a solid sphere of enriched uranium whose diameter exceeded one hundred meters. By contrast, nature does not preclude there being a solid sphere of gold with a diameter of more than one hundred meters. Had the people of Earth all conspired to build a giant blob of gold exceeding one hundred meters in diameter, they might have done so. But had they conspired to build a giant blob of enriched uranium of similar proportions, they would have failed—i.e., failed *necessarily*. This feature of the fact described by (1) is why we might call *it*—but not that described by (2)—a law of nature.

Beatty is spectacularly noncommittal when it comes to whether some generalization expresses a law of nature (biological or other). He's less reluctant to commit to some generalization's being or failing to be naturally necessary. For example, Beatty sees (1) as being necessitated by nature, and notes that natural necessity is what justifies inferences from counterfactual antecedent conditions (such as our inference that the people of Earth would have succeeded in building a giant blob of gold, but failed in their attempt to build similarly proportioned blob of uranium) (*ibid*: 53fn9). Later on, however, he grants that physical truths might turn out to be "contingent, at least in the sense of 'cosmologically' contingent" (*ibid*: 63). By "cosmologically contingent" he presumably means that the true physical generalizations which are naturally necessary might not have naturally necessary—they might not have even been true. Had the initial conditions of the universe been different, for example, it might have only been an accident that no particle had exceeded the speed of light (rather than a law that no particle *could have* exceeded the speed of light), on par with the accidental truth that there's no blob of gold whose diameter exceeds one hundred meters.

What Beatty has alluded to (albeit indirectly) is the curious ability of laws to be both necessary and contingent. By (some species of) necessity, all solid spheres of uranium have a diameter of less than 100 meters. But this particular necessity is itself contingent (in some sense), depending as it does on some fact or set of facts deep in the history of the universe that presumably could have been otherwise. Had those facts been different, the fundamental physical laws whose consequences preclude such a large blob of uranium might have been different. But now here's the rub: if the "cosmological" contingency of true physical generalizations does not itself prevent some of those generalizations from being necessarily true, why must true biological generalizations fail to achieve some type of necessity simply on account of their being "evolutionarily" contingent? What qualitative difference is there between cosmological and evolutionary contingencies that allows for necessities in the former case but not the latter? In the absence of any explanation for treating cosmological contingencies differently from evolutionary contingencies, and in the absence of any clear difference (after all, what are cosmological contingencies but evolutionary contingencies on a cosmic scale?), I see no reason to deny some true biological generalizations the monicker "necessary" solely because those truths are contingent in some sense.[2]

Predictable and Unpredictable

But Beatty is not beaten. He distinguishes the species of contingency mentioned above from another, "stronger sense" in which biological generalizations are only contingently true. The sense he has in mind derives from the tendency of true biological generalizations to have their truth undermined by various and sundry chance events (in particular, random mutation and functional equivalence) that might arise during the evolution of a lineage. Thus, any attempt to formulate true biological generalizations with respect to functional categories will be blocked by two fundamental facts about the evolutionary process: (a) that whichever particular variants happen to arise in a population is entirely a matter of chance[3]; and (b) that whichever specific forms are favored by selection is equally chancey, in the sense that the same selection regime might just as easily have favored a form that was equally good for performing some task relevant to selection but which was, by chance, not favored (Beatty 1995: 57-59).

This species threatens the necessity of any possible biological generalization in a qualitatively different way from that of the cosmic sort of contingency mentioned above. Whereas the cosmic sort of evolutionary contingency purports to undermine a biological generalization's necessity by suggesting that it *might not have been true*, these unpredictable features of the evolutionary process suggest that any given true biological generalization might *stop* being true. However inexorable a biological generalization's march through evolutionary history might seem, random events can stop it dead in its tracks at any time. Presumably, goes the argument, we ought to view as merely contingent any generalization whose truth is this easily revoked.

I'm not going to deny the intuitive pull of this argument. It certainly is easy to appreciate the sentiment that there is no necessity attached to the truth of a generalization that can stop being true. But there are other intuitive considerations that indicate that timelessness is not a property that a truth needs to possess in order to be naturally necessary. I think I can show that the view that naturally necessary generalizations are eternally true derives its support from the erroneous idea that laws of nature cannot refer to indexicals. Once this prejudice is removed, it becomes easy accept the proposition that naturally necessary generalizations can, in principle, be true for a finite amount of time. Were I to establish this, I will have removed the last of Beatty's motivations for denying that biological generalizations can

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[2] Beatty (2006) refers to the cosmic sort of evolutionary contingency as "causal dependence" contingency, in contrast with the "unpredictability" sort of evolutionary contingency discussed below.

[3] See Darwin (1862) on orchids, and Beatty (2004) on Darwin on orchids.

be true as a matter of natural necessity. That a biological generalization might stop being true in no way impinges on its ability to be naturally necessary.

There are widely accepted and uncontroversial examples both of cases in which (i) generalizations expressing laws of nature refer to indexicals, particulars, demonstratives, or their like, and in which (ii) generalizations expressing natural necessities stop being true. A stock example of (i) is Tooley's (1977: 686) description of a garden in which

> [a]ll the fruit…at any time are apples. When 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. Cherry trees planted in the garden bear apples, or they bear nothing at all.

What should we say in a case like this other than that it is a law of nature that all the fruit in the garden (which, incidentally, belongs to a character named "Smith") are apples? Indeed, Tooley is so impressed by the thought experiment that he is compelled to give up his commitment to the view that laws of nature cannot refer to specific particulars. Marc Lange (2000) mentions an even more convincing case: Paul Dirac's conjecture that the gravitational-force "constant" is inversely proportional to a specific time — viz., the Big Bang. As Lange stresses, it seems wrong to say that Dirac's conjecture was a *logical* error (although it does seem to have been a factual one). If we accept that there are laws of nature whose reign does not extend beyond a particular portion of space, there would seem to be no motivation for denying that there could be laws of nature whose reign does not extend beyond a particular stretch of time.

The case is even stronger for naturally necessary generalizations which stop being true. Consider the generalization "All objects on Earth experience a downward acceleration of $9.8m/s^2$." If this generalization is true, its truth is no accident. It is not simply a coincidence that all objects on Earth happen to accelerate towards the Earth at $9.8m/s^2$. Suppose we drained the planet of its molten core. The generalization "All objects on Earth experience a downward acceleration of $9.8m/s^2$" would no longer be true. Does it then follow that, when it *was* true, it was only accidentally true? Of course not. Far more plausible is the view that certain facts necessitated by nature may simply cease to be facts when other facts about nature cease to be facts. The same pattern of reasoning applies to Dirac's conjecture mentioned above. Had Dirac's conjecture been true, a true generalization describing gravitational force would not have been true for very long (since it was supposed to have changed with time). But whichever generalization *was* true at a given time was naturally necessary.

A more sophisticated objection focuses on what might be taken to be an untenable change in our epistemic situation which would accompany the change in truth value of a naturally necessary generalization. The objection goes like this: Natural necessities ground inferences from counterfactual (or otherwise subjunctive) antecedents (Beatty 1995: 53n9). If naturally necessary generalizations can stop being true, and this possibility was well known, we would have no grounds for relying on counterfactually-based inferences. But we (scientists included) *do* rely quite heavily on counterfactually-based inferences. Therefore changes in truth-value are not something that our concept of natural necessity can sustain.

I grant that counterfactually-based inferences play a pervasive and important role in science and in everyday life, but I deny that the knowledge that naturally necessary generalizations might change their truth values would significantly impact our ability to use counterfactuals in the way we currently do. In fact, physicists are somewhat accustomed to changes in the truth value of naturally necessary generalizations, and they (like all other scientists) continue to routinely employ counterfactuals in their attempts to deduce what might have been the case had things been different. Consider the case of Earth's globe-trotting magnetic north pole. The map below [insert at bottom of page] shows changes in the pole since 1831 (when it was first discovered by James Ross, on an arctic crusade in which his ship was ice-locked for four years!).[4] Any true generalization that is made regarding magnetic north, I submit, is naturally necessary. Yet ostensibly any particular generalization does not stay true for very long.

Do magnetic north's shifts in location undermine our ability to make counterfactual inferences which rely upon it? Consider the counterfactual, "Had we been standing at 71.3ºN, 110.8ºW in 2001, we would have been 10 degrees south of magnetic north."[5] The truth of this counterfactual is not in doubt, nor, I would argue, is the natural necessity which "supports" it.[6] This may seem like a cheat, since I have specified in the antecedent a time at which we knew where the magnetic pole was. In fact, this nicely illustrates an important point — viz., that our ability to use counterfactuals as tools of inference does not depend on natural necessities remaining *constant*. Rather, it merely depends on us *knowing* which putatively naturally necessary generalizations are *true*. Some naturally necessary generalizations may require that occasionally we perform direct checks on their truth. Others may require that we perform some calculation which our theory suggests will tell us whether the generalization is true (or, rather, which generalization(s) are true), as might have been the case had Dirac's conjecture turned out to be true. Others may remain true for the duration of the universe. The point is that whether a generalization's truth is a matter of natural necessity cannot turn simply on whether it remains true. For all we know, the day on which all true distinctly physical generalizations stop being true is just around the corner. Were that day to come, would we conclude that there was something else that made a certain set of counterfactuals

[4] NASA, "Earth's Inconstant Magnetic Field" (http://www.nasa.gov/vision/earth/lookingatearth/29dec_magneticfield.html)

[5] The position of magnetic north in 2001 was observed at 81.3ºN, 110.8ºW (values since then are estimated).

[6] I leave unspecified for the time being the way in which laws (or natural necessities) support counterfactuals. For those who cannot wait for an exposition, I direct you to Lange (2000, forthcoming), where the relation of between laws and counterfactuals is cashed out with remarkable clarity and rigor.

true, some truth that wasn't naturally necessary? I don't see why we would have any immediate reason to conclude that, some reason that followed directly from our concept of natural necessity. More reasonably, we might say something like, "It used to be the case that All *F*s were *necessarily G*. Had there been an *F*, it would have *necessarily* been *G*. But now some *F*'s being *G* would be merely accidental." To my ear, there is nothing even remotely incoherent going on there.

I'll now conclude my critique of Beatty by quickly summarizing the main points. I attempted to extinguish one of Beatty's reasons (causal dependence contingency) for thinking that there were no such things as biological laws by showing that we standardly accept the lawhood of physical generalizations the truth of which is contingent upon some event in history. Thus, I argued, there is no basis for denying lawhood to biological generalizations on account of their historical dependence. Second, I tried to show that the possibility that a true generalization (thought to express a law or natural necessity) might stop being true at some point in the future typically does nothing to impugn the ascription of *natural necessity* to physical generalizations. Likewise, we need not take the possibility — even the *likelihood* — of a true biological generalization to stop being true as putting that generalization beyond the realm of natural necessity. If there are no laws in biology, it will have to be because of reasons other than those offered by Beatty.

2. Mitchell's Pragmatic Laws

I've just argued the possibility of naturally necessary biological generalizations survives Beatty's critique, and thus so too does the possibility of laws in biology. Sandra Mitchell (Mitchell 1997; Mitchell 2000) has also sought to preserve the possibility of biological laws in the wake of Beatty's (and others') arguments, but sees the sort of strategy I've pursued so far as misguided:

> By focusing discussion on laws vs. accidental generalizations, natural necessity vs. contingency, one is saddled with a dichotomous conceptual framework that fails to display important differences between the kinds of causal structure found in our world and differences in the corresponding scientific representations of those structures. (Mitchell 2000: 255)

In contrast to my approach, she has argued in favor of a "pragmatic" strategy that "focuses on the function of laws and holds that there are a variety of forms of scientific claims that provide us with usable knowledge" and "leads us to develop a multidimensional framework of features that characterize useful scientific generalizations" (*ibid*.: 244). Since the function of laws in science is to "explain, predict, and allow successful intervention," and since there are lots of generalizations in biology which allow us to do those important scientific tasks, there are plenty of distinctly biological generalizations that function as laws in biology (*ibid*.: 249). The only thing standing in the way of their lawhood, she argues, is a "normativist" approach to laws of nature which requires that, in addition to playing their scientific roles, laws be "universal, exceptionless, and necessary" (*ibid*.: 249). However we rarely if ever find any such generalizations in science. If we want to hold on to the view that laws are important to science, then, we should jettison the normativist criteria for lawhood and restrict our attention to principles that play the roles in science that laws have always been thought to play.

The central criticism I will develop in this section is that, in contrast to Beatty's overly *restrictive* criteria for lawhood, Mitchell's "pragmatic" approach is far too *permissive*. Her decision to transcend the law vs. accident/necessary vs. contingent dichotomy in favor of a continuum of usefulness is ill-advised, and generates counterintuitive results about what qualifies as a law. Laws are certainly useful for science. But certain features of our concept of natural law, I will suggest, require that a generalization be more than merely useful.

2.1. "The Continuum of Contingency"

Mitchell compares three familiar generalizations:

(1) All solid spheres of enriched uranium (U_{235}) have a diameter of less than one hundred meters.

(2) All solid spheres of gold (Au) have a diameter of less than one hundred meters.

(3) All the coins in Goodman's pocket are copper.

In contrast to Beatty's observation that "there seems to be more to [(1) than to (2) or (3): CH], by virtue of which we might accord the former but not the latter the status of law…That something more has to do with what nature necessitates or precludes," Mitchell argues that the difference between the generalizations "appears to be in the nature and degree of contingency they display" (Mitchell 1997: 254).

According to Mitchell, under the misguided law vs. accident paradigm, (2) and (3) are accidental generalizations, whereas (1) is a bona fide law of nature. Not only do these category assignments fail to do justice to what scientists find useful, they misrepresent the degree of contingency possessed by each generalization. She is, of course, happy to grant that (3) is entirely contingent:

> Recall the counterfactual. If a coin were to be placed in Goodman's pocket, would it be copper? No. A quarter could easily be put in the pocket and falsify the generalization (*ibid*.: 252)

But the fact that all solid spheres of gold have a diameter of less than one hundred meters seems to her less contingent:

> That there is not sufficient gold in the desired configuration is something quite deep about the history of the universe and the distribution of matter. It is the result of the processes of stellar fusion and solar system formation. At the origin of the universe in the Big Bang sixteen billion years ago it is believed that only helium and deuterium and ^{7}Li were formed within the first few minutes after that event. All the other elements, including the gold and uranium in our examples have been produced in the subsequent evolution and development of the stars. (Beryllium and boron developed later as well, but by a different process). The spectra of very old stars, which formed over 10 billion years ago, show deficiencies in all elements except hydrogen and helium, and so it is believed other elements have been synthesized since that time [cite]. Thus, there is a sense in which it is impossible, given the history of the universe, that the so-called accidental generalization about gold would be false.

I'm not competent to pronounce on how probable (or possible) the existence of a hundred-meter wide blob of gold would be given certain cosmological facts. Luckily that doesn't matter, because Mitchell has used two different sorts of counterfactuals here: one sort to evaluate the Goodman example, and another sort to evaluate the gold example.

The counterfactual associated with the Goodman example asks us what would happen, given the history of the universe (or any other sorts of facts), if we now attempted to insert a quarter into Goodman's pocket. Our intuitions about the lack of necessity involved in (3) are revealed by the sorts of inferences we are precluded from making given the counterfactual antecedent,

(3*) "Had we attempted to put a quarter in Goodman's pocket".

The analogous counterfactual antecedent for (2), then, is

(2*) "Had we attempted to build a hundred-meter wide blob of gold,"

not, as Mitchell suggets,

(2†) "Had we 'played the tape' of the origin of the universe again" (*ibid*.: 254)

I grant that (2†) suggests that there are more possible worlds in which we might find a non-copper coin in Goodman's pocket than in which we might find a gigantic blob of gold. But so what, since (2†) is not designed to reveal our intuitions about whether there is a law of nature that would prevent us from successfully amalgamating enough gold to form the hundred meter-wide sphere? ($2^{\dagger}$) is designed to reveal our intuitions about the possibility of there having come to be at some point a hundred-meter-wide blob of gold. But what matters is not whether such a blob of gold *could have formed* given the history of our universe, but rather whether there *could be* such a blob of gold given what the universe is like — that is, whether certain features of the universe *preclude* there from being a hundred-meter-wide blob of gold. Reichenbach's gold-sphere example has traditionally been used to show that there is nothing about the universe that prevents such a blob of gold from forming. And watch what happens when we employ the appropriate analogue to the Goodman antecedent: Had the people of Earth conspired to construct the hundred-meter-wide blob of gold [i.e., (2*)], they might have done so; they shouldn't be discouraged by the absence of such a blob of gold in the universe's history. Mitchell is able to achieve the appearance of a difference between generalizations (2) and (3) by using counterfactual antecedents designed for very different purposes. Once the required adjustments are made, we see that the accidental nature of (2) is preserved right along side that of (3). It's obvious to Mitchell how we might endeavor to falsify the Goodman generalization: just try putting a non-copper coin in Goodman's pocket. It seems equally obvious that a similar attempt (*mutatis mutandis*) could be made to falsify the gold generalization.

Mitchell uses a similar device to score points in favor of the idea that there is an accidental component to the fact that (1) all spheres of uranium are less than one hundred meters in diameter:

> there may be conditions under which a [hundred-meter-wide: CH] sphere of uranium would be less likely to reach criticality, and thus it is not inconceivable that there could have been a history of our planet in which it would have been stable. The reasoning here is by analogy (*ibid*.: 254).

This time it sounds like we're invited to consider a different history of the universe, one in which it makes sense to entertain the counterfactual antecedent,

(1*) Had the universe been such that it was possible for a hundred-meter-wide sphere of uranium to avoid criticality

But once again, Mitchell has changed the rules in the middle of the game. Supposing that it is a law of nature that particles cannot exceed the speed of light, consider the counterfactual antecedent,

(L*) "Had the universe been such that it was possible for a particle to exceed the speed of light"

(L*) invites us to entertain a *counterlegal* scenario, one that is impossible given the actual laws of nature. The reason that (L*) and (1*) sound similar is because they both direct us to a counterfactual universe that has a fundamentally different nomic structure than the actual universe. This is accomplished via the phrase "Had the universe been such that…,"— in other words, had the structure of the universe been able to tolerate things that the actual universe cannot tolerate. But an invitation to consider the nature of a universe in which certain nomic generalizations fail to hold cannot achieve the effect of testing our intuitions about which generalizations our naturally necessary in *our* world, for the simple reason that it is the place of those generalizations *in our world* that makes them naturally necessary. No one expects a generalization which is naturally necessary in our world to be naturally necessary in every possible world, most of all possible worlds for which it is explicitly stated that those generalizations might fail to hold. On most understandings of the concept of natural law, laws are not metaphysically necessary.[7]

Watch again what happens when we formulate a counterfactual antecedent for (1) which is appropriately analogous to the Goodman antecedent provided by Mitchell (as well as (2*) above):

(1*) "Had we attempted to build a hundred-meter-wide blob of uranium U_{235}…"

As Beatty (1995) observes, we would have failed in our attempt to build this blob of uranium for reasons having "to do with what nature necessitates or precludes."

If what I've said above is right, then the law vs. accident dichotomy survives Mitchell's attempt to demolish and replace it with a continuum along which generalizations vary only in how contingent they are. Once we employ the appropriate sort of counterfactual antecedent, the sort that tests our intuitions about what would happen if we were to attempt to falsify a particular generalization, the laws come out on one side and the accidents come out on the other. There will always be a story to tell in which it is possible to emphasize the contingency of some putative law relative to a particular history of the world. But such story-telling is misguided, if for no other reason than that laws have never been thought to be necessary in *every* sense — e.g., laws have never been claimed to be logically necessary, and rarely to be metaphysically necessary.

In addition to claiming that the law vs accident dichotomy is not *accurate*, Mitchell places equal emphasis on the worry that the dichotomy renders useless most of the generalizations used in science. Since most scientific generalizations "fall short of the [normativist: CH] philosopher's ideal" of being "universal, exceptionless, [and: CH] necessarily true," those of us who want to understand the structure of scientific knowledge would be better advised to "explicate the way in which causal knowledge of this sort is usable" rather "than to just relegate those claims to the heap of 'accidents'" (Mitchell 1997: 249-250).

My response to her worry here is to point out that describing a generalization as "accidental" does not thereby make it scientifically useless, nor does it preclude us from explicating its role in scientific practice. Take an example from biology. Facts about the certain geographical distributions of phenotypes are typically taken to be entirely accidental. Ages ago, a Floridian pelican picked up a fish in the

[7] A notable exception to this near-consensus is scientific essentialism.

Indian River and accidentally dropped it into Lake Kissimmee. By chance, there happened to have been other fish in Kissimmee with whom the dropped fish could successfully mate. Pretty soon, there was a new species (call them "dropfish".) For one lucky Floridian phylogeneticist, this accidental event is of enormous scientific importance, for it answers the question "Why are there dropfish?". For a Floridian biogeographer, it offers an explanation for why there are dropfish in Lake Kissimmee. Suppose further research leads him to formulate the true but accidental generalization,

(D) All dropfish are in Lake Kissimmee.

The Floridian conservation biologist can use this generalization to explain the failed attempt to relocate a species of bird from its native Kissimmee to Lake Okeechobee. This bird feeds exclusively on dropfish, and since all dropfish are in Lake Kissimmee, Lake Okeechobee has nothing for the bird to eat. I have trouble seeing how this generalization is not an accident *par excellence*. I also have trouble seeing how it is not scientifically useful. Plausibly, it even grounds the truth of the counterfactual,

(D*) Had I caught a dropfish today, I would have to have been fishing in Lake Kissimmee.

Along similar lines, we could infer that a pelican whose stomach contents contained a dropfish must have been near Lake Kissimmee recently. Notice, however, that the following counterfactual is false:

($D^\dagger$) Had we attempted to put a dropfish in Lake Okeechobee, we would, by necessity, have failed.

In particular, it's false for the same reasons that would have allowed us to succeed in putting a quarter into Goodman's pocket or in (given our very best efforts) building a hundred-meter-wide blob of gold — viz., because there is no natural necessity precluding our success.

To the extent that the type of analysis I've been doing here generalizes, I think there is little motivation left for replacing the law vs. accident dichotomy with a continuum of contingency. To quote Beatty again, there just *is* something more to the nomic generalizations than there is to the non-nomic ones.

What, then, for the "pragmatic approach" to laws? Does it die along with the attempt to eradicate the law/accident dichotomy? I don't think so. Mitchell's suspicion that a continuum of contingency would reveal laws to be pragmatic entities was based on the mistaken idea that generalizations cannot be scientifically useful and still be thought of as accidents — they cannot allow us to predict, explain, and successfully intervene. The dropfish story shows that generalizations *can* play these important scientific roles and still be thought of as accidental. But I don't think this spells "Doom" for the idea that examining scientific practice is the best way of understanding what a law of nature is. In order to pursue the pragmatic approach under the assumption of the law/accident dichotomy, we will need to find some scientific role which laws play but which accidents (in virtue of their nature as accidents) cannot play.

3. Woodward on Explanation and Invariance

In a recent series of papers (Woodward 1997; Woodward 2001), Jim Woodward has argued for a position which gives primacy to a generalization's *invariance* across counterfactual scenarios. Like Mitchell, Woodward wants to jettison the laws/accidents dichotomy in favor of a continuum of invariance, where generalizations differ not in kind but in the range of counterfactual scenarios over which they would have still been true.

There are two factors motivating Woodward's interest replacing the laws/accidents dichotomy with a continuum of invariance. The first is his dissatisfaction with what he suspects is driving the philosophical interest in laws — viz., the "nomothetic thesis," the idea that laws are required for explanations. As Woodward rightly observes, there is a whole lot of explaining going on in the special sciences, most of it done without reference to laws (Woodward 1997: S32-S33). The second factor is his belief that (a particular kind of) invariance is what allows generalizations to be explanatory. Since non-lawful generalizations can possess the same kind of invariance which allows putative laws to explain, a generalization can be explanatory without being a law. If what was motivating our interest in laws was the worry that we could not explain phenomena without them, we would then seem to no longer have a reason for caring about whether there are laws in biology (or anywhere else, apparently).

I certainly share Woodward's qualms with the nomothetic thesis, but I think there's by now a near-consensus that the nomothetic thesis is false. Lots of philosophers are interested in laws of nature, but the fact that laws are not necessary for scientific explanation has been known for some time now. Something else must be behind philosophers' interest in laws. Perhaps that "something else" is the sort of invariance Woodward has in mind. Does Woodward's account of invariance successfully refute the view that so-called "laws of nature" possess something special that so-called "accidents" do not, collapsing all explanatory generalizations into one category whose members differ quantitatively? We shall see. I intend to argue that, while Woodward is correct in his observation that generalizations other than the so-called laws are invariant and therefore explanatory, we have good reasons to believe that the type of invariance possessed by putative laws differs from the type of invariance possessed by generalizations which do not express laws. If this argument is successful, I will have preserved the laws/accidents dichotomy as well as provided a potential means by which we might identify the role played by laws in science which accidents cannot play.

3.1. Woodward's Invariance

For Woodward, a generalization [All *X*s are *Y*: CH] is *invariant* if and only if it

> continues to hold or is stable...under some class of interventions [a] that change the conditions described in its antecedent [its *X*-value: CH] and [b] that tells us how the conditions described in its consequent [its *Y*-value: CH] would change in response to these interventions (Woodward 1997: S31-S32).

where an "intervention" on *X* with respect to *Y* changes an object's *Y*-value (if at all) via a change in its *X*-value.

Let's look at these definitions in the context of generalizations (1), (2), and (3) discussed above. Regarding (3), the relevant variables are "coin in Goodman's pocket" (X) and "made of copper" (Y). Since changing whether a coin is in Goodman's pocket (changing its *X*-value) will not change whether the coin is copper (changing its *Y*-value), the generalization "All coins in Goodman's pocket are copper" fails to be invariant in Woodward's sense. That is, the generalization would *not* continue to hold in the case where we changed the *X*-value of a quarter, for example.

How does the lack of invariance exhibited by (3) result in its inability to explain, on Woodward's account? For Woodward, "to explain an explanandum [a change in *Y*-value: CH] is to show how changes in it counterfactually depend on changes in the factors cited in the explanans [an *X*-value: CH]" where the relevant counterfactuals are those with antecedents involving interventions on *X*-values (*ibid*.: 5). Since interventions that change whether a coin is in Goodman's pocket do not change whether it is copper (i.e., (3) is not invariant in the relevant sense), the counterfactual

(3†) Had we inserted a quarter into Goodman's pocket, it would have changed from silver to copper

is false. And since changes in whether a coin is copper therefore do not counterfactually depend on changes in whether it is in Goodman's pocket, we cannot use the fact that all coins in Goodman's pocket are copper to explain a *change* in whether some particular coin is copper.[8]

Similarly for (2), where *X* = "sphere made of gold" and *Y* = "thing less than one hundred meters in diameter." Since increasing the diameter of a thing to beyond one hundred meters does not counterfactually depend on changing that thing from a sphere made of gold to, for example, a sphere made of something other than gold, *Y* can vary independently of *X*, which means that (2) fails to be invariant under interventions on *X*. Were we to have constructed a solid sphere with a diameter of more than one hundred meters, it might still have been made of gold. So an object's being gold cannot *alone* explain why it is less than one hundred meters in diameter.

In contrast, (1) does seem to possess the relevant sort of invariance required for the generalization to explain changes in *Y*-values. When *X* = "sphere made of uranium" and *Y* = "sphere less than one hundred meters in diameter," (1) holds for interventions on *X*. Were we to have constructed a solid sphere

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[8] See (Woodward 1997: S32fn4) for similar remarks.

with a diameter of more than one hundred meters, it could not have been made of uranium. Were we to have constructed a solid sphere of those proportions, it would still have been true that all solid spheres of uranium are less than one hundred meters in diameter. If we are charged with the task of building a hundred-meter-wide sphere, and the only material we had available to us at the time was some enriched uranium, completing our task would require a change in what the sphere was to be made of (i.e., a change in *X*-value.) Thus, a sphere's being uranium can*, all by itself*, explain why that sphere is less than one hundred meters in diameter.

Let us now turn our attention to an intermediate case. Take the generalization,

(4) All my children have my facial features

(4) is not a law of nature. There is a possible world which is nomically identical to ours but in which my children's faces resemble their mother's. And yet some child's being mine clearly explains why it has my facial features. This, argues Woodward, is due to the fact that (4) possesses the same relevant sort of invariance possessed by (1). In other words, (4) continues to hold under some class of interventions that (a) makes a particular child someone else's problem — I mean, offspring — and (b) tells us how the child's resemblance to me would change in response to turning him into someone else's offspring. Had some child not been mine, it would not have possessed my facial features (or possessed them only *by accident*).

Our goal in this section was to see whether Woodward has shown that laws do not differ from non-laws in the *kind* of invariance they exhibit. I think Woodward *has* shown that a generalization's ability to explain is bound up with its being invariant under some class of interventions. And I think that the laws' ability to explain is bound up with their being invariant under some class of interventions. Nevertheless, I think there is something about the particular range of cases over which generalizations expressing laws remain invariant that sets them apart from generalizations which do not express laws. Note: I do *not* think this "something" about the range of invariance is what separates generalizations which are explanatory from those which are not; that some generalization does not possess this something does not mean it cannot be explanatory. Presumably this is neither here nor there for Woodward, since he is only worried about our interest in laws *qua* explanatory devices. But, as is, I think, widely recognized, laws are used to do more than explain.

4. Lange's Counterfactually Stable Sets

In this section I'll briefly describe a recent discovery of Marc Lange's concerning the difference between laws and accidents with respect to their range of invariance over counterfactual situations. I will then provide a sketch of how this feature of the laws — something which accidents lack — accounts for their ability to perform a particular scientific task. Contrary to Woodward, then, I will have shown that there is a role other than explanation which the laws' invariance allows them and them alone to play.

Over the last decade, Marc Lange has forcefully argued that the set of truths expressing laws of nature bears a particular relation to subjunctive facts which is possessed by no other set of truths (save for the set of *all* truths and the set of *logical* truths). It is this: the set of truths expressing laws is stable across every subjunctive supposition which is logically consistent with the facts that are laws. For example, the fact that there is no hundred-meter-wide blob of uranium would still have obtained had I worn black socks instead of white, or had we packed as much uranium as possible into a hundred-and-one meter wide dome, or had Earth spontaneously imploded, or…and so on for every subjunctive antecedent logically consistent with that fact and the other facts that are laws. In contrast, a set of truths containing the facts that are laws along with one accident (say, that there is no hundred-meter-wide blob of gold) is not stable over every subjunctive supposition with which its members are logically consistent. That there is no hundred-meter-wide blob of gold is logically consistent with all the people in the world trying in ernest to assemble such a blob. Had they given it there best shot, then it might not have been true that there was no hundred-meter-wide blob of gold. So the stability of this set of truths does not extend to all the counterfactual situations with which it is logically consistent. Unlike the set containing the facts that are laws, any set containing an accident (aside from the set of all truths) will be unstable for some counterfactual situations with which it is logically consistent.

I'd like now to suggest that this feature of the laws — their unique brand of counterfactual stability — is what explains the laws' ability to play a hitherto unexplored scientific role. An important component of scientific practice involves making a judgment as to whether some hypothesis could possibly explain the focal phenomenon. Eventually, some candidate hypotheses are ruled out on the basis of their not being able to possibly account for the phenomena, and some are ruled in based on a judgment of their possibly explaining the phenomena. The principles governing these possibility judgments are, I argue, laws of nature, and it is in virtue of the laws' particular flavor of counterfactual stability that scientists are able to rationally make these judgments. In contrast, no accident is capable of ruling out the logically possible hypotheses.[9]

Consider again the fact that

(1) All uranium spheres are less than one hundred meters wide

Now consider a logically possible explanation for (1):

(E) Many centuries ago, a powerful king had all uranium spheres greater than one hundred meters wide destroyed.

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[9] Space prevents me from a complete development of this argument. See my companion paper, "From Scientific Possibility to Natural Necessity" for the whole story.

Why are we able to rule out (E) as an explanation for (1)? Plausibly, because of our basal commitment to the principle that (1) expresses a law, and so precludes the possibility of (E). Indeed, for each subjunctive supposition consistent with (1) (other than (1) itself), we are able to rule out that subjunctive supposition as a possible explanans for (1). In contrast, look again at the fact that

(2) All gold spheres are less than one hundred meters wide

Now consider a logically possible explanation for (2)

(F) Many centuries ago, a powerful king had all gold spheres greater than one hundred meters wide destroyed.

Are we able to rule out (F) as an explanation of (2)? No. We have no basal commitment to the principle that (2) expresses a law. So, for all we know, (F) might be true.

Finally, remember (4)?

(4) All my children's faces look like mine.

While clearly not a law of nature, that my children are mine rather than someone else's explains why their faces look like mine. But now consider a logically possible explanation for (4):

(H) All my children had pre-natal reconstructive surgery to look like me

Are we able to rule out (H) as an explanation of (4)? No. We have no basal commitment to the principle that (4) expresses a law. So, for all we know, (H) might be true. On the other hand, suppose, contrary to fact, that God had made it a law of nature that all children's faces resemble their fathers'. Then we *could* rule out (H) as an explanation for (4), for, had (H) been false, it still would have been true that all my children's faces look like mine.

I'm painfully sensitive to the fact that the sketch I've provided here will fail to convince most that laws are responsible for scientists' ability to rule out lots of logically possible explanations, and that the ability of laws to play this role is due to their unique subjunctive stability. But that's not the point of this paper. The point of this paper was to resist, on the one hand, objections to the possibility of biological laws, and, on the other hand, objections to the distinction between laws and accidents. I've shown that the objections to biological lawhood fail, and that a rejection of the law/accident dichotomy misses something special about laws — plausibly, something special about their place in scientific practice. While I think these objections fail, underlying them is the well-warranted expectation that the case for biological laws be made in terms of some biological facts which are naturally necessary in a way that other, scientifically significant facts are not. I attempt to fulfill this expectation in the companion essay mentioned above.

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