Life in a Physical World: The Place of the Life Sciences

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1. Physicalism, Biology, and Reductionism: State of the Art

Debate about the place of the life sciences within the empirical sciences has often centered around the issues of physicalism and reductionism.[1] Given that some form of physicalism is correct, why is biological science not physical science? Why do biological theories appear to be autonomous and irreducible to physical theories? And what is the nature of biological laws or regularities, assuming that the fundamental interactions that govern the physical world also are at work in living organisms? These are some of the oldest and most extensively discussed questions concerning the biological sciences. While philosophers of science of a Logical Empiricist bent first tried to defend the view that biological theories such as those of classical Mendelian genetics are in principle reducible to physical-chemical theories[2], an anti-reductionist consensus emerged during the 1970s.[3] This consensus was mainly based on the

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[1] See, e.g., Ernst Mayr, *This is Biology.* Cambridge, Mass.: Harvard University Press 1997.

[2] Kenneth F. Schaffner, "Approaches to Reduction", in: *Philosophy of Science* 34, 1967, pp. 137-147; Kenneth F. Schaffner, "The Watson-Crick Model and Reductionism", in: *British Journal for the Philosophy of Science* 20, 4, 1969, pp. 325-48.

[3] David L. Hull, *Philosophy of Biological Science.* Englewood Cliffs: Prentice Hall 1974; Philip Kitcher, "1953 and All That. A Tale of Two Sciences", in: *The Philosophical Review*

argument that genetic concepts such as dominance or the gene concept itself cannot be redefined in an extensionally equivalent way in terms of molecular concepts. The reason for this is thought to lie in the functional character of biological concepts. This means that certain theoretically significant properties in biology are individuated by their causal role, not some intrinsic structural property. But the molecular realizers of these causal roles are highly heterogeneous at the molecular level; in others words, the realizers don't have a theoretically significant molecular property in common that could be used eliminate the higher-level terms. Therefore, higher-level concepts in biology remain explanatorily indispensable; they have autonomous explanatory value that cannot be reproduced by molecular theories alone. Thus, on this by now received view in philosophy of biology, biological theories are irreducible for basically the same reason that most philosophers accept as the definitive refutation of mind-brain reductions.

Thus, philosophers of biology have reached similar conclusions as philosophers of mind have in regard of the issue of mind/brain-reductionism.[4] However, Jaegwon Kim[5] has argued that, in the philosophy of mind, this consensus is based on an inadequate model of reduction,

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93, 3, 1984, pp. 335-373; Alexander Rosenberg, *The Structure of Biological Science*. Cambridge: Cambridge University Press 1985; cf. C. Kenneth Waters, "Why the Anti-Reductionist Consensus Won't Survive the Case of Classical Mendelian Genetics", in: *PSA 1990*, East Lansing, Mich.: Philosophy of Science Association 1990, pp. 125-139.

[4] Donald Davidson, "Mental Events", in: L. Foster/J. Swanson (eds.), *Experience and Theory*. Amherst, Mass.: University of Massachusetts Press 1970; Jerry A. Fodor, "Special Sciences or the Disunity of Science as a Working Hypothesis", in: *Synthese* 28, 1974, pp. 97-115.

[5] Jaegwon Kim, *Mind in a Physical World: An Essay on the Mind-Body Problem and Mental Causation*. Cambridge, Mass.: MIT Press 1998.

namely Ernest Nagel's.[6] He proposed an alternative scheme according to which reduction does not consist in first connecting the terms of the theory to be reduced to those of the reducing theory by way of biconditional bridge principles (as Nagel's model assumes or is widely taken to assume), followed by the derivation of the laws of the theory to be reduced from the laws of the reducing theory. Instead, Kim argues that successful reductions must first give a functional characterization of the referents of the terms of the theory to be reduced. Such a characterization specifies the set of things that come under a concept by stating the causes and/or the effects that these things have in their containing system. Next, scientists must identify the things that play these causal roles at the lower level. For example, Kim thinks that the case of genetics provides a paradigm for this kind of reduction. Genes were first identified by the causal roles they play in living organisms, namely causing heritable character differences, being segregated and assorted in accordance with Mendel's laws, etc. etc. Later, it was discovered that these causal roles are actually fulfilled by DNA sequences that code for protein and/or RNA molecules.[7] This *is* a reduction; nothing more is required. I take it that Kim does not require that scientist be able to state necessary and sufficient *physical* (molecular) conditions for some thing to instantiate a theoretically significant higher-level property, for if he did, his model would basically collapse into Nagel's (or what is usually taken to be Nagel's). All that he requires is that the realizers of the causal role that defines the higher-level property be somehow describable from the physical level (he admits that this may not always be possible, for example, he things it is not possible for qualia).

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[6] Ernest Nagel, *The Structure of Science. Problems in the Logic of Scientific Explanation*. London: Routledge and Kegan Paul 1961.

[7] C. Kenneth Waters, "Genes Made Molecular", *Philosophy of Science* 61, 1994, pp. 163-185.

Kim's suggestion has not succeeded in displacing the anti-reductionist consensus in the philosophy of biology; in fact, it was hardly noticed by philosophers of biology. However, it is clear what their response would be: Even if Kim's new model of reduction is accepted, *that* the molecular realizers of some functionally individuated biological concept can be described at the molecular level *alone* is exactly what is not possible according to the anti-reductionist consensus. On the standard argument from multiple realizability, such a description would involve an ungainly disjunctive predicate without any explanatory force. This is why the higher-level theories are explanatorily indispensable.

To this reply, a Kim-style reductionist could retort that the identification of classical genes with protein- and RNA-coding DNA sequences is not ungainly at all. Understanding what genes are at the molecular level is precisely what molecular biology has done for genetics, and if this does not account as a reduction, then nothing does. However, this reductionist response misses that reduction is supposed to at least conserve the explanatory achievements of the theory to be reduced, in addition to providing explanations that exceed those of the theory to be reduced. But this is not the case in the genetics/molecular biology case according to antireductionists.[8] Classical transmission genetics offers explanations of inheritance patters that basically cite the pairing and separation of chromosomes. These explanations abstract away from the "gory" molecular details that constitute these processes at bottom. Kitcher[9] draws an analogy here to Putnam's well known square peg-in a round hole-argument. According to this argument, there is a perfectly fine explanation of why square pegs don't fit in round holes that appeals only to these objects' geometrical shape. This explanation abstracts away from the composition of the objects and from any physical laws that these may

[8] Kitcher, *op. cit.*

[9] Kitcher, *op. cit.,* p. 350

obey. In fact, this explanation is more general than any explanation that appeals to the objects' composition. Kitcher suggests that this is analogous to the explanations that classical genetics give of inheritance patterns.

There are different replies that a reductionist can give to this argument. First, it can be argued that the theoretical content of classical genetics is not exhausted by patterns of gene transmission. Classical geneticists described genetic structures with the help of elaborate maps long before molecular techniques such as DNA-sequencing became available. For instance, it was possible to show that genes must be linear structures, a finding which was confirmed by the discovery of the way in which DNA encodes genetic information.[10] This fits nicely with Kim's model of reduction.

A second possible response is that Kitcher's argument—just like Putnam's—is a manifestation of a theoretically unfounded "explanatory Protagoreanism",[11] according to which "some human or other is the measure of all putative explanations, of those which do explain and of those which do not." While Kitcher's chromosomal mechanics explanations or Putnam's square peg-in-a-round-hole-explanation may seem perfectly satisfactory to some people, perhaps relative to certain pragmatic contexts, it is not an explanation that would satisfy a physicist or a molecular biologist. Science ought to do better than that, for example, by showing exactly what forces pull the chromosomes apart before a cell divides, or what

[10] Marcel Weber, "Representing Genes: Classical Mapping Techniques and the Growth of Genetical Knowledge", in: *Studies in History and Philosophy of Biological and Biomedical Sciences* 29, 2, 1998, pp. 295-315.

[11] Alexander Rosenberg, *Darwinian Reductionism*. *Or, How to Stop Worrying and Love Molecular Biology.* Chicago: The University of Chicago Press 2006, p. 35.

forces repel the peg from the hole, taking into account their composition, of course. Here, the reductionism/antireductionism debate turns on divergent assumptions as to what constitutes a good explanation of a phenomenon—a matter on which, naturally, reductionists and antireductionists have different intuitions.[12]

These arguments and counter-arguments are well known and have been discussed in the literature *ad nauseam*. It is beyond the scope of this paper to present all the twists and turns of the reductionism/antireductionism debate, or even to lay out the various positions that have been defended, from strong forms of reductionism to non-reductive physicalism, emergentism, scientific pluralism, and so on.[13] Instead, what I would like to do here is to examine some novel arguments, which have received little attention. I think that both attempts, while perhaps not successful, contain some genuine insights with respect to the place of the life sciences in the conceptual landscape of the natural sciences.

The first view I want to critically review is an attempt to defend of a strong form of reductionism about biology that can be found in Alex Rosenberg's recent book.[14] I will show why Rosenberg's account fails, even though it contains a valuable insight concerning the role of the concept of function in biology, namely in the individuation of traits. Rosenberg thinks that this makes all of biology conceptually dependent on evolutionary theory, which is not

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[12] Paul Hoyningen-Huene, "Epistemological Reductionism in Biology: Intuitions, Explications and Objections", in: P. Hoyningen-Huene/F. M. Wuketits (eds.), *Reductionism and Systems Theory in the Life Sciences*. Dordrecht: Kluwer Academic 1989: pp. 29-44.

[13] An important strand of this debate is critically reviewed in Thomas Reydon's contribution in this volume, namely the issue of natural kinds and its implications for reduction.

[14] Rosenberg 2006, *op. cit.*

generally thought to be reducible to more fundamental theories. As a result, an unbridgeable gap threatens between biology and physical theories. Rosenberg tries to close this gap by trying to show that evolutionary theory, at least natural selection theory, *is* fundamental. I shall criticize Rosenberg's position on two counts: First, I will show that the idea that natural selection theory is fundamental is problematic (Section 2). Second, I will argue that there was no problem for the reductionist in the first place, because there are ways of individuating organismic traits that do not depend on the concept of natural selection (.ection 3).

The second view I will discuss here comes from outside the philosophy of biology, namely from general metaphysics and is it is not very recent, but it has been hardly noticed by philosophers of biology and of science: the view of biological laws that has been developed by Michael Thompson.[15] He thinks that biological laws differ fundamentally from physical laws. While this claim is hardly new, the specific differences that Thompson sees between the two classes of laws have, to my knowledge, not been noticed in the philosophy of biology. Even though I disagree with some parts of Thompson's account, I believe that it merits serious discussion, which I shall attempt in Section 4.

2. Rosenberg's Defense of Reductionism and Why it Fails

[15] Michael Thompson, "The Representation of Life", in: R. Hursthouse/G. Lawrence/W. Quinn (eds.), *Virtues and Reasons. Philippa Foot and Moral Theory*. Oxford: Clarendon 1995, pp. 247-296.

In his recent book,[16] Rosenberg firmly adheres to the view that “nothing in biology makes sense except in the light of evolution.” Evolutionary biologists such as Ernst Mayr[17] or Theodosius Dobzhansky,[18] who have defended this view, based their arguments on the assumption that a full understanding of organisms requires the identification of the *ultimate* causes of their characteristic properties. To use Mayr’s favorite example, even if we fully understand the physiological mechanisms that induce migratory birds to flock together and embark on a long journey towards a warmer climate zone—i.e., the proximate cause—a full understanding of this behavior requires an account of what it was selected for in the birds’ evolutionary past—i.e., the ultimate cause. On this received view, proximate and ultimate explanations are *complementary* and *conceptually independent*. This conceptual independence allows for the possibility of endorsing both reductionism about proximate biology and antireductionism about evolutionary biology. The latter kind of antireductionism is usually justified on grounds of the multiple realizability of fitness.[19]

However, according to Rosenberg, ultimate and proximate biology are *not* conceptually independent. How could this be? Why can’t biologists pinpoint an organism’s molecular, physiological, developmental etc. mechanisms independently of its evolutionary history? For Rosenberg, this has to do with the way in which biologists pick the *explananda*, in other words, that which they want to explain by discovering the underlying mechanisms. Let us say,

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[16] Rosenberg 2006, *op. cit.*

[17] Ernst Mayr, "Cause and Effect in Biology", in: *Science* 134, 1961, pp. 1501-1506.

[18] Theodosius Dobzhansky, "Biology, Molecular and Organismic", in: *American Zoologist* 4, 4, 1964, pp. 443-452.

[19] Elliott Sober, *The Nature of Selection. Evolutionary Theory in Philosophical Focus.* Cambridge Mass.: MIT Press 1984.

for example, that biologists want to understand how chick embryos form wings. 'Wing' is a functional concept. In other words, the classification of some structure as a wing, including its exact delimitation from neighboring structures, involves an appeal to function (flight in this case). Rosenberg argues that the salient concept of function here must be that of *proper* function,[20] that is, function as selected effect. A wing is a structure that was selected because it confers the ability to fly. It is a functional type, and "function" means proper function according to Rosenberg. The realizers of this functional type are *heterogeneous* because different structures with different evolutionary origins can confer the ability to fly. This is why there are also no natural kinds (essences) in the traditional sense in biology, Rosenberg argues. For selection is blind to essences (intrinsic structure).[21] The upshot is that the way in which an organism is divided into parts crucially depends on the theory of natural selection. Since proximate biology takes its requests for explanation from such divisions ("what mechanisms control the development of the chick wing?"), it is conceptually dependent on evolutionary biology.

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[20] Ruth G. Millikan, "In Defense of Proper Functions", in: *Philosophy of Science* 56, 1989, pp. 288-302.

[21] Thomas Reydon (in this volume) argues that selected effect functions are *not* multiply realizable, because they require that the function bearers stand in an appropriate historical (genealogical) relationship, which means that even something which plays the exact same causal role today would not count as an instance of the function if it evolved independently. To this, it could be replied that nothing prevents a certain organ to change its internal structure (its essence) in evolution while it continues to benefit from natural selection, so the set of things that has the same activity and stands in the appropriate genealogical relations would count as instances of the function. This would count as multiple realization. However, Reydon's point does seem to limit the multiple realizability of selected effect functions.

This position with respect to functions and proximate biology seems to put Rosenberg in the difficult position that, in order to maintain his reductionism, he must show either that the theory of natural selection is reducible to more fundamental theories or that it is *itself* a fundamental theory. He chooses the second path: He argues that what he calls the “principle of natural selection” is itself a fundamental law. Here is one formulation of this alleged “principle”:[22]

> $\forall x \forall y \forall E$ [If *x* and *y* are competing organisms in generation *n*, and *x* is fitter than *y* in *E*, then probably (there is some generation *n'*, at which *x* has more descendants than *y*)]

There are alternative formulations, and Rosenberg is aware that this may not be the most general way of stating the principle. Rosenberg takes this to be an empirical law (in contrast to Sober,[23] who thinks that the principle of natural selection is *a priori*) and he understands fitness in terms of a probabilistic propensity.

Now for what is probably Rosenberg’s boldest claim: He argues that the principle of natural selection is a *physical* law, or perhaps a *chemical* law (or both). In support of this claim, he argues that even things that are not considered t be alive obey this principle, for example, self-replicating molecules. He also offers a story why textbooks of physical chemistry do not

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[22] Rosenberg (2006), *op. cit.,* p. 160

[23] Elliott Sober, "Two Outbreaks of Lawlessness in Recent Philosophy of Biology", in: *Philosophy of Science (Proceedings)* 64, 1998, pp. S458-S467.

normally cite this law, namely, because physical chemists normally ask different questions. But this doesn't prove that this isn't a fundamental law of nature according to Rosenberg.

Rosenberg needs this claim in order to make "natural selection safe for reductionism." The reason is, as I have already shown, is that Rosenberg thinks that natural selection via the concept of proper function provides the explananda for biological explanations, even outside of evolutionary biology.

I would like to address two critical points at Rosenberg's argument. The first concerns his claim that there exists a "principle of natural selection" which is a physical law. The second point challenges the claim that natural selection theory is needed for identifying the explananda for biological explanations.

First, let us consider Rosenberg's alleged "principle of natural selection". As stated, it is only applicable to populations with discrete generations. Evolutionary theorists use different fitness measures for populations with discrete generations and for age-structured populations with overlapping generations. In one of my own works, I argue that if there is a general principle of natural selection, then it is highly abstract and needs to be instantiated by specific models.[24] On this view, the theory of natural selection is a family of models ("semantic view" of theories) and its content is not appropriately expressed by a universally quantified claim. Universally quantified claims only come in when it comes to stating classes of natural systems to which the models *apply*. The general theory is merely some sort of a guideline for building specific models.

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[24] Marcel Weber, *Die Architektur der Synthese. Entstehung und Philosophie der modernen Evolutionstheorie.* Berlin: Walter de Gruyter 1998, Ch. 6.

Rosenberg could reply that, perhaps, he has not correctly stated the fundamental principle of natural selection, but that his point that there exists such a principle and that it is a fundamental law of nature stands. However, I don't think that he can sustain this view. The reason is that there are no reasons to believe that there is a fundamental measure of evolutionary fitness. "Fitness" means different things, depending on the evolutionary problem that biologists are trying to solve. Sometimes, fitness is an absolute growth rate. Sometimes it is an absolute number for the surviving offspring. Sometimes it is a coefficient in a population genetic model that makes explicit assumption the genetic system (e.g., Mendelian inheritance). Fitness is predicated of genes, genotypes, individuals, and groups. So far, there is no unifying framework for evolutionary theory, and there are no reasons to think why there should be one. There are different evolutionary processes and different questions that one can ask about them. Any fitness measure can be useful for answering one kind of question, but not another.

If there is no fundamental fitness measure, it follows that there is no general "principle of natural selection". And *a fortiori* there is also no fundamental law of nature about natural selection.[25]

[25] Daniel Sirtes (personal communication) objects that this argument at best proves that there is no *single* fundamental principle; there still could be one for every type of evolutionary process. However, it seems to me that such a hodgepodge of principle—and there would have to quite a lot of them—would not deserve the status of "fundamental" principles, because they would all only be applicable to some restricted number of cases.

I think that this failure of Rosenberg's attempt is exemplary for the whole of biology. Biology is not concerned with identifying laws of nature in the traditional sense. Its goal is rather to answer specific why-questions by using various conceptual tools, including in some cases mathematical models. The answers to such why-questions cannot generally be incorporated into some unified framework.[26]

As we have seen, the ultimate motivation for Rosenberg's account of biological laws was his goal of showing that biological traits could be both functional, in the proper role sense, and yet physical. In the following section, I shall examine if there are no other ways of how biological traits can be individuated.

3. An Alternative Account of Functions and Trait Individuation

As we have seen, Rosenberg based his defense of reductionism on the view that biological traits are individuated *functionally*, where "function" is understood in the sense of selected effect function or "proper" function. I think the first part of this claim is correct, however, there is a problem with the second.

On this view, some item X has a function F in organism S exactly if X does F and the fact that some earlier tokens of X have done X is a cause of X's presence in S. The way in which earlier tokens can cause the presence of some item in later generations, of course, is natural

[26] This claim is generally known as scientific pluralism, see Stephen H. Kellert/Helen E. Longino/C. Kenneth Waters (eds.), *Scientific Pluralism*. Minnesota Studies in Philosophy of Science, Vol. XIX. Minneapolis: University of Minnesota Press 2006.

selection. Thus, Rosenberg's view is that natural selection is not only needed to explain why some organism S came to have a part X, but to speak of X as having some kind of *unity* in the first place. It is for this reason that Rosenberg thinks that the theory of natural selection is fundamental for the whole of biology. This, of course, includes behavioral biology. According to Rosenberg, the description of behavioral traits is laden and/or ought to be laden by theoretical hypotheses about selection history. A trait such as a wing is individuated by the fact that it was selected for flying, no matter what other capacities it may have (for instance, it's capacity of being flapped so as to distract or attract some other animal). On this view, descriptions of an organism's traits are laden by the theory of natural selection and assumptions about the evolutionary past.

Paul Griffiths[27] has argued that this view puts the cart before the horse. The parts of organisms and their causal capacities must be understandable *independently* of natural selection. Otherwise, the following regress threatens:

1. Selected effect functions are ascribed by causal analysis of the capacities of the parts of ancestral organisms and a determination of their fitness contribution.
2. Thus, we must already be able to individuate the parts. This cannot be done on the basis of the ancestors to the ancestral organisms, because this would generate a regress
3. But if we are able to individuate parts for ancestral organisms independently of their selection history, then this is possible for living organisms

[27] Paul Griffiths, "Function, Homology, and Character Individuation", in: *Philosophy of Science* 73, 1, 2006, pp. 1-25.

So if natural selection is not fit for the individuation of organismic parts, what is? This turns out to be a very difficult question, and I can answer it only in outline.

In essence, I do not think that there is a *general* answer to this question. In other words, there is no unique principle of cutting up an organism into parts in the way that Plato suggested in the infamous passage of the *Phaedrus*, according to which a good scientist should carve nature at her joints. Clearly, Socrates's advice from the *Phaedrus* to proceed by trying not to splinter any parts, “as a bad butcher might do,”[28] is not helpful at all, for we have no theory-independent way of knowing when we have splintered something.

The explanandum is almost never neutral with respect to the explanans. So different theoretical models often come with different ways of classifying the phenomena. This has long been recognized for the physical sciences, for example, by Kuhn and Feyerabend, but few people (excepting Rosenberg) have noticed that the same holds for biology. Developmental biology, evolutionary biology, evo-devo, physiology, cell biology and so on have different ways of individuating phenomena.

However, I do want to argue that the concept of biological function is *often* involved when biologists cut up an organism into parts, including mechanisms. But the salient concept of function need not be that of selected effect functions. There are other concepts of function, and they can also fulfill the role that Rosenberg thinks only selected effect functions can play.

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[28] Plato, *Complete Works,* J. M. Cooper (ed.). Indianapolis: Hackett 1997, 265e (p. 542).

As an alternative, I suggest a modified version of causal role functions.[29] This account starts with Cummins's[30] analysis according to which functions are such capacities that are capable of explaining a capacity of some containing system. The paradigm is the heart's capacity to pump blood figuring in any adequate explanation of the circulatory system's capacity to transport nutrients, oxygen and blood cells through the body. According to Cummins, the pertinent capacity of the containing system is a matter of an interest-based choice to be made by the investigator. I have modified this account by suggesting that this systems capacity should be made dependent not on the investigator's interests, but on the role that the containing system itself plays in the self-reproduction of the *whole* organism. I argue that this is what turns Cummins-functions into *biological* functions. Cummins-functions can be applied to any kind of system. But only biological systems are capable of self-reproduction. In order for self-reproduction to occur, an organism's functions must work together. The specific contribution that some organ's causal capacities make to self-reproduction makes will depend on what other organs do. For example, if there were subsystems of an organism that would use the heart's heat production towards something that itself makes a contribution of self-reproduction, then the heart would (also) have the function of producing heat. It is the place that such a causal capacity plays in a whole network that gives it its function (perhaps much in the way in which a linguistic expression's meaning is given by the inferential role that the expression plays in a network of other expressions, as claimed by inferentialists and semantic holists).

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[29] Marcel Weber, *Philosophy of Experimental Biology*. Cambridge: Cambridge University Press 2005; Marcel Weber, "Holism, Coherence, and the Dispositional Concept of Functions", in: *Annals in the History and Philosophy of Biology* 10, 2005, pp. 189-201.

[30] Robert Cummins, "Functional Analysis", in: *Journal of Philosophy* 72, 1975, pp. 741-765.

I have argued that introducing such a global constraint on a system of functions might make the interest-dependence vanish, provided that there is exactly one way of laying a network of cooperating functions over an organism. Of course, this is hard to prove; but I suggest that it might be possible by using a notion of maximal explanatory coherence.[31]

Thus, contrary to what Rosenberg claims, dividing up an organism into different parts or traits can be done independently of its selection history. Whether there is one correct or natural way of doing this, however, is very difficult to say.[32] What seems clear is that functions have a holistic[33] character: Some thing only has a function if it is connected to many other things that also have functions and that conspire to maintain the organism's form. Furthermore, what some thing's function is can depend on what other things do to which it is connected. However, this holism need not necessarily be an obstacle to reductionism, unless the requirements for successful reduction are made excessively strong. For instance, it might still

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[31] Weber, "Holism, Coherence, and the Dispositional Concept of Functions"

[32] A lot hangs on the way in which the *explanadum* of such a network of functions is construed. It is tempting to suggest that it has to be "self-reproduction of the individual" (as I have done in my *Philosophy of Experiental Biology*), however, this notion suffers from a certain indeterminacy that is introduced by the reflexive term "self." What is that "self" that is being reproduced? And what does its "reproduction" or "maintenance" involve, in other words: what are its persistence conditions? Note that the answer "the individual" doesn't really help because of the notion of biological individual is notoriously difficult (see Jack Wilson, *Biological Individuality - The Identity and Persistence of Living Entities.* Cambridge: Cambridge University Press 1999). This could make some room for pluralism.

[33] See Michael Esfeld, *Holism in Philosophy of Mind and Philosophy of Physics*. Dordrecht: Kluwer 2001.

be possible that Kim's requirements (see Section 1) can be satisfied. Of course, on the view of functions that I have mentioned, some thing's function may not only depend on how this thing interacts with its immediate interaction partners (Kim's "causal role") but also on what the role of that thing is in the whole organism. But once this role is known, there are no obstacles to then identifying the realizers of these functions.

In the final section, I shall critically discuss an altogether different challenge to reduction in biology.

4. Michael Thompson's Account of Biological Regularities

After much debate on *ceteris paribus* laws and various "outbreaks of lawlessness"[34] in biology, many philosophers of biology including myself have found Jim Woodward's account of causation and explanation[35] very helpful to come to terms with causal regularities in biology. However, there is something that this account does not quite capture, and this is the question of what makes a certain causal generalization a *biological* generalization as opposed to merely a physical or chemical one. I think the following answer is not really satisfactory: "A causal generalization is biological if it concerns living organisms or parts thereof." For there are endlessly many causal generalizations about any part of an organism that could just

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[34] Sober (1998), *op. cit.*

[35] James Woodward, *Making Things Happen: A Theory of Causal Explanation*. Oxford: Oxford University Press 2003.

as well be described as physical or chemical, for example, “blood vessels with a high content of elastin expand as internal fluid pressure increases.”[36]

An interesting answer to the question of what characterizes biological generalizations can be found in the work of Michael Thompson.[37] It comes from general metaphysics and has therefore rarely been noted by philosophers of science. Thompson writes for example:

“Now suppose I say, 'Bobcats breed in spring': it is obvious that this isn't going to happen in any particular case unless certain conditions are satisfied. Perhaps a special hormone must be released in late winter. And perhaps the hormone will not be released if the bobcat is too close to sea level, or if it fails to pass through the shade of a certain sort of tall pine. But now, to articulate *these* conditions is to advance one's teaching about bobcats. [...] The thought that *certain hormones are released*, or that *they live in such-and-such altitudes and amid such-and-such vegetation*, is a thought of the same kind as the thought that thy breed in the spring. [...] These conditions are presupposed by the life-form itself.”[38]

Thompson thinks that there is an important difference between biological generalizations such as ‘bobcats breed in spring’ and purely physical generalizations such as ‘water boils at 100°C’. But the difference is not that one requires *ceteris paribus* clauses while the other doesn’t. They both do. i.e., both generalizations are subject to certain conditions that must obtain for the generalizations to be manifested. In the first example, it is necessary that certain

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[36] C. Kenneth Waters, "Causal Regularities in the Biological World of Contingent Distributions", *Biology and Philosophy* 13, 1998, pp. 5-36.

[37] Thompson, *op. cit.*

[38] *ibid.* p. 287

environmental cues that trigger mating behavior in bobcats occur (e.g., longer days, milder temperatures) and that nothing interferes (e.g., a shortage of prey). In the second example, it is necessary that normal atmospheric pressure obtains and that the water has not been salted. But according to Thompson, in the biological case it is itself a fact about this species *that* these conditions obtain. Bobcats will seek an environment where the conditions for breeding are favorable, such that the regularity will obtain. By contrast, there is no law about water that says that all water tends to occur under conditions such that the regularity "water boils at 100°C" or any other such regularity will obtain. In fact, the latter generalization has a purely *hypothetical* character: It only says, water boils *if* the temperature is 100°C or more. By contrast, the biological generalization is *categorical* in nature. It reads as it is written: bobcats breed in spring. *That* bobcats live in places where there is a seasonal change in temperature and day length that triggers their breeding is part of the nature of bobcats.

It is clear that Thompson has quite a different conception of regularities or laws than contemporary philosophy of science, in fact, it is closer to Aristotelian forms than to laws of nature in the modern sense. According to Thompson, each organism instantiates a certain "life-form" that is characterized by such caterogical laws as the ones about bobcats in his example. His notion of life-form seems to be one of a complex irreducible essence, much like Aristotle's concept of *eidos.* Of course, as such this conception is problematic, especially in light of all the arguments against biological essentialism that have been produced in recent years.[39] However, there might be some merit in Thompson's suggestion that what characterizes biological generalizations is in part the way in which different generalizations conspire to ensure each other's being manifested by individual organisms. There might

[39] John Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science.* Cambridge Mass.: Harvard University Press 1993.

perhaps even be an analogy to what some philosophers of science have said about natural kinds in biology, for instance, Richard Boyd's theory of homeostatic property clusters.[40]

According to Thompson's account, what makes certain regularities biologically salient is that they ensure that *other* regularities are instantiated, regularities that are themselves important for the survival of the individual, and so on. This is quite reminiscent of my tentative answer to the question of what makes certain activities in an organism functionally relevant (see the preceding section). On this account of functions as well as on Thompson's account of biological laws, there exist a highly complex relation between the different parts of an organism, a relation that obtains exactly if the parts are organized such that the system sustains itself. This kind of focus on self-reproduction is what distinguishes biology from other natural sciences.

Where I must part with Thompson is here: I see no principled way of drawing a line between essential and non-essential parts of an organism. Which laws are associated with the form and which ones aren't? Furthermore, I see no reason why Thompson's account of biological laws should be inconsistent with an adequate form of reductionism. Even if it is their relation to the instantiation conditions of other laws that makes certain laws about biological entities salient, there is no reason why these relations cannot be fully understood and expressed in physical-chemical language.

One final point: It should also be noted that Thompson's categorical laws are only valid for the living state and if the organisms over which they range live in their normal environment.

[40] Richard Boyd, "Homeostasis, Species, and Higher Taxa", in: Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge, Mass.: MIT Press, pp. 141-185.

They contain no information what would happen, for instance, if North American bobcats were transferred to the Tropics. Would they still breed in spring? To answer this kind of question requires good old-fashioned causal laws that range not only over a set of actual states, but over counterfactual situations as well. Biologists can discover such causal laws as well, but they will be of the ordinary, hypothetical sort. In this respect, biology is no different from other natural sciences.

5. Conclusions

There have been many attempts to show that biology occupies some special place in the natural sciences, and most of them have attempted to show that biological theories (or laws) are irreducible to physical-chemical theories. This is obviously correct if “reduction” is understood in a strong, derivational sense, but far less obvious if a weaker sense of reduction such as Kim’s is assumed. One of the most popular arguments against reduction, the argument from multiple realizability, is not convincing on such a weaker view. Additional arguments to the effect that some higher-level explanations do some explanatory work that cannot be recovered at the lower level rely strongly on intuitions as to what constitutes a good explanation and are not convincing to those who don’t share these intuitions, for the intuitions of reductionists and anti-reductionists notoriously differ.

Further, I have considered Rosenberg’s argument that (1) even proximate biology needs evolutionary concepts (proper function) to individuate the parts of an organism, this (2) is no problem for the reductionist because the salient evolutionary principles are *fundamental physical laws*. The latter claim fails because natural selection theory is not a unified theory; it consists of a wide variety of specific models that deploy different fitness measures. Furthermore, there are alternative ways of how biologists can individuate the parts of an organism, for example, by causal role functions. I how discussed a rich version of causal role functions that might yield a natural system of functions for each type of organism. Even though functions are in a sense holistic properties on this account, reductionists need not worry about this.

Finally, I have critically examined M. Thompson’s essentialistic account of biological laws according to which the latter develop an irreducible life-form for each species of organism. I argue that this account gives a good answer of what makes certain regularities biologically salient (or biological at all), this also provides no arguments against a suitably understood reductionism.

Notes