# The Nature of Philosophy and the Philosophy of Nature

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Peter Godfrey-Smith (2014) *Philosophy of Biology*. Princeton, NJ: Princeton University press.

## Abstract

Peter Godfrey-Smith's introduction to the philosophy of biology is excellent. This review questions one implication of that book, namely that Darwin's case for the efficacy of natural selection was hampered by his ignorance of the particulate nature of inheritance. I suggest, instead, that Darwin was handicapped by an inability to effectively engage in quantitative population thinking. I also question Godfrey-Smith's understanding of the role that Malthusian struggle plays in linking natural selection to the origination of new adaptive traits, and I raise problems for his defence of apparently unproblematic conceptions of human nature. Finally, I highlight the welcome conception of a 'philosophy of nature' developed by Godfrey-Smith.

## Keywords

William Bateson; R. A. Fisher; Peter Godfrey-Smith; Human Nature; Natural Selection

Peter Godfrey-Smith has written a lovely introduction to our subject. There is a great deal to like about his new book, and far less to complain about than a reviewer might hope for. *Philosophy of Biology* is limpid. Not only is it beautifully written, it also manages to convey the basic structure of several technical issues in an unfussy and intuitive manner that will engage even the least mathematically minded of readers. It is simultaneously balanced and opinionated: it allows a newcomer to the subject to appreciate the strengths of differing positions, while offering refreshing and decisive answers to philosophical questions. It is short enough to offer a swift overview of the sorts of questions that concern us, but it takes enough time to develop (albeit in skeletal form) claims that will intrigue seasoned professionals. It gives a sense of the historical development of biology and of philosophical responses to it, and yet its concerns are up-to-date.

Let me now develop some more critical lines of reflection. As an introduction, it is inevitable that *Philosophy of Biology* leaves out, or slides over, details that one might linger over in a treatment aimed at a different audience. So the worries that follow do not detract from the quality of a book of this kind. I begin with qualms about Godfrey-Smith's understanding of natural selection, before briefly making trouble for his comments about human nature. I will return to gushing praise at the end of the review.

**What Darwin Didn't Know**

There is a general consensus among historians that while Darwin quickly convinced his scientific peers that the history of life could be represented as a great tree, he was much less successful in persuading them that natural selection was the primary explanation for this pattern, or indeed that natural selection was an important process at all. But what sort of facts, or what sort of reasoning, might have enabled Darwin to convince his sceptical audience? What was it, precisely, that Darwin didn't know?

Very early in his book Godfrey-Smith gives a précis of this historical orthodoxy, before offering a quick remark about Darwin's failings:[1]

> Most biologists were fairly quickly convinced that evolution (as we now call it) had occurred, and that common ancestry connects much or all of life on earth. There was more controversy about how the process had happened, especially about natural selection and Darwin's insistence on gradual change. One of the weaker points in Darwin's work was his understanding of reproduction and inheritance… (p. 9)

We are then treated to a swift canter through Mendel's work, the modern synthesis, and the discovery of the structure of DNA. Ninety pages later, Godfrey-Smith offers a brief description of one of the major landmarks of the modern synthesis. R.A. Fisher, he tells us, 'argued in 1930 that inheritance *had* to operate in a "particulate" manner, with discrete and stable genes, in order for sustained Darwinian evolution to be possible' (p. 97).

Putting all of these comments together, the reader might come away with the impression that Mendel, Watson and Crick helped biologists to understand—dimly at first, but later with a much clearer material basis—that inheritance was a matter of the passing on of discrete material particles. This conception of inheritance as particulate would then turn out to explain how, at Fisher's hands, a case could finally be made for the efficacy of selection. Darwin's shaky understanding of 'reproduction and inheritance' can then be understood as his ignorance of the material nature of the inheritance process. Godfrey-Smith may not intend to convey this message, but many readers will conclude from these remarks that what Darwin didn't know was that inheritance was particulate.

There are reasons to doubt the history of biology as reconstructed in the preceding paragraph. Although Fisher did indeed claim that 'one of the main difficulties felt by Darwin is resolved by the particulate theory' (1930, p. 12), he

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[1] All page references are to *Philosophy of Biology* unless otherwise indicated.

did not argue that natural selection could not possibly work if it took place in a context of blending inheritance. Instead, Fisher argued that:

> The important consequence of the blending is that, if not safeguarded by intense marital correlation, the heritable variance is approximately halved in every generation…If variability persists, as Darwin rightly inferred, causes of new variability must continually be at work. Almost every individual of each generation must be a mutant… (1930, p. 5)

In other words, if inheritance follows a blending pattern, then for selection to be efficacious it must also be the case that like organisms mate with like, or that new variations are constantly arising, or both. Darwin seemed to think that assumptions much like these two were in fact satisfied (Lewens 2010). He thought that the struggle for existence was frequently so intense that only the very best adapted individuals would survive, hence they would end up mating with each other. And he held the view that 'sports'—that is, rare variations, of large magnitude—were of little significance for evolutionary change when compared with 'individual differences'. These were the 'many slight differences' which Darwin thought regularly appeared in populations (Lewens 2010, Vorzimmer 1963).

How did Darwin go about explaining inheritance? His hypothesis of pangenesis, first published in his 1868 work *The Variation of Animals and Plants under Domestication*, laid out a provisional mechanism. He took the view that all the cells in the body: 'throw off minute granules which are dispersed throughout the whole system…They are collected from all parts of the system to constitute the sexual elements, and their development in the next generation forms a new being' (Darwin 1868). So Darwin did think that inheritance involved the transmission of particles, yet this conviction did not help him to convert sceptics about natural selection. Pangenesis was barely mentioned in the editions of the *Origin* that appeared after 1868, indicating that Darwin himself seemed to think that his views about the particulate basis of inheritance were irrelevant to the case he tried to make for natural selection (Peckham 1959).

What Darwin needed to make a case for selection's efficacy was not a hypothesis about the material processes underpinning inheritance. It was a way of linking a set of claims about patterns of resemblance between offspring and their parents to a set of claims about the changing constitution of populations. Darwin gestured to one way of doing this—perhaps selection can work if variation is constantly springing up, and the struggle for existence is supremely intense—but he never developed a rigorous way to make this argument. Fisher developed a rigorous approach, and he concluded on that basis that Darwin's scheme for selection had implausible empirical consequences. But Darwin's failings were not so much a matter of ignorance that inheritance works in a certain way, as a matter of ignorance of how to engage in a certain form of quantitative 'population thinking'.

We have seen that, on Fisher's view, a particulate conception of inheritance could solve problems faced by blending views. Fisher wrote that it was 'universally admitted' that 'Darwin accepted the fusion or blending theory of inheritance' (1930, p. 1). But we have also seen that Darwin thought that offspring acquired a set of particles from their parents, passed on at conception, which matured in such a way as to explain trans-generational resemblance. There is evidently no contradiction in thinking that offspring traits are usually intermediate between those of their parents, while adding that the transmission of particles is what explains this. This raises the question of what one might mean by contrasting 'blending' inheritance with 'particulate' inheritance as Fisher did.

In Fisher's case the nature of the difference is clear. When he demonstrated 'the great contrast between the blending and the particulate theories of inheritance' (1930, p. 4), his exploration of the consequences of blending focused purely on what we might think of as phenomenal patterns of inheritance. Fisher entertained no theory of the mechanism that might underlie such an inheritance system. Instead, he showed us how quickly variance will disappear in a system whereby offspring trait values are always intermediate between the values of the parents. He then contrasted this purely phenomenal model with a system whereby offspring trait values follow what he called ‘the modern scheme of Mendelian or factorial inheritance’ (p. 7).

Evidently a population characterised by Fisher’s simple blending model will behave differently to a population that follows a Mendelian scheme. In a simple blending model, for example, the offspring of a given pair of parents are always identical, and reversion is impossible. In the Mendelian case, phenotypic values of siblings can differ, and phenotypes can disappear for a generation before reappearing again. Fisher argued that ‘the mechanism of particulate inheritance’ results in ‘no inherent tendency for the variability to diminish’ (p. 9). But if a phenomenology of blending inheritance can be articulated without spelling out the material mechanism that underlies it, we must ask whether a contrasting phenomenology of Mendelian inheritance can also be neutral regarding the underlying mechanism that sustains it. Of course, the mere claim that inheritance follows a Mendelian pattern tells us nothing about the precise material constitution of the particles transmitted from parents to offspring. But we can also ask whether Mendelian inheritance requires that we think of inheritance in terms of the transmission of particles at all.

There is evidence that Mendel himself seems not to have understood the ‘elements’, which determined the makeup of his peas, as particles. Our modern notation represents homozygotes as *AA* or *aa*, and heterozygotes as *Aa*. Each token ‘*A*’ or ‘*a*’ represents some sort of discrete material token, which must be matched with a similar or dissimilar token. Mendel’s notation was different: what we would now call the heterozygote was still represented as *Aa*, but the pure-breeding offspring of hybrids—what we would now call the homozygotes—were represented as *A* or *a* simpliciter (Olby 1979). Perhaps Mendel thought of yellow-making factor in pea plants as a kind of fluid, which might be present in an unadulterated form—*A*—or in combination with an alternative immiscible green-making fluid—*Aa*.

William Bateson offers a more intriguing case. Bateson was, of course, Mendel’s great early champion. And yet, in spite of Bateson’s enthusiastic claims that plant breeders can 'take out greenness and put in yellowness; you can take out hairiness and put in dwarfness', it is not clear that he thought of genes as discrete material particles (Bateson 1904). Like some John Dupré *avant la lettre*, Bateson took the view that organisms were best understood in dynamic, processual terms (Dupré 2012). 'A living creature', wrote Bateson, 'is a vortex of chemical and molecular change…We commonly think of plants and animals as matter, but they are really systems through which matter is continuously passing.'[2]

This stress on the power of processes, rather than the power of materials, was carried over to genes themselves. He denied that genes were located on chromosomes, suggesting instead that they might be some sort of 'gel'. The constitution of these gels would then be understood by the physics of vortices. This physics would explain in dynamic terms the potential of genes to undergo abrupt shifts, as well as their stability over generations: 'I incline to the expectation that the heterogeneity of the determining elements as factors lies rather in forces, of which the materials are the vehicle, than in the nature of the material itself.'

This excursion into the arcana of Mendel and Bateson shows that there have been Mendelians—i.e. persons who stress the importance of Mendelian ratios for understanding the fluctuating makeups of populations over generations—who have denied that inheritance is a matter of the passing on of particles. That reinforces the thought that if Darwin was held back by anything, it was a failure to develop a rigorous analysis of the changing composition of traits in populations, rather than a failure to link inheritance with the transmission of particles. Even Fisher, in stressing the importance for selection of the preservation of variance in a population, places weight on the first populational task rather than the second mechanical one.

[2] This and the proceeding quotations from Bateson are all taken from Rushton (2014). For further detail on Bateson see Radick (2011; 2013).

These points might seem like pedantic corrections to Godfrey-Smith's casual historical asides. But they underline an important claim Godfrey-Smith makes immediately after his comment about Fisher's insistence that inheritance '*had* to operate in a "particulate" manner'. He argues that modern genomics suggests that there may be no particulate genes of the sort that Fisher imagined:

> The point is not merely that genes are more indefinite and blurry entities than had been supposed; it has to do with *why* they are less particle-like. Genomes, at least in organisms like us, are more organized entities, with large proportions of an organism's DNA engaged in subtle processes of regulation of the expression of "coding" regions. New genomes are made by combining large chunks of this genetic material from the genomes of each parent, and this is not much like shuffling a collection of alleles and stringing some together on a line. (p. 97)

In stressing our increasing understanding of the genome as a resource for inheritance that can be variably reemployed and reconfigured, Godfrey-Smith points to ways in which, when viewed close up, genes do not look much like particles. He also reminds us that 'objects that look indefinite and vague up close can become usably sharp once you are looking from further away', so that 'when we look at change over a long period in the entire species, genes come into focus and evolution does look like change in allele frequencies' (98-9). I have tried to suggest here that this combination of scepticism about the particulate nature of genes when viewed in their fine material details, along with an enthusiastic endorsement of trans-generational change as a matter of shifting combinations of alleles, could already be discerned in the work of early Mendelians like Bateson.

**Malthus and Selection**

Darwin introduced the concept of natural selection in order to explain the phenomena of adaptation. It is essential for this task that Darwin's mechanism does not merely explain why beneficial traits, once they arise, might become widely distributed in a population. Darwin also took it that natural selection could explain why beneficial adaptations come to exist in the first place. If natural selection cannot discharge this role, it is unclear how natural selection is supposed to be an alternative to special creation. For, on the face of things, if natural selection can only tell us why wonderful adaptations like eyes spread through populations if they happen to arise as novel variants, we are still faced with the question of how something as intricate as an eye appears in the first place. Natural selection is supposed to answer demands for origin explanations, as well as demands for distribution explanations.

If natural selection is a force that can increase trait frequencies, how can selection also explain the origination of beneficial adaptations? It must turn out, as Godfrey-Smith explains, that by increasing trait frequencies, selection makes the emergence of further adaptations more likely. Several writers have shown how this can happen, and Godfrey-Smith endorses the basic schema (see, for example, Neander 1995, Lewens 2004). Suppose that genomic bases X, Y and Z produce eyes of increasing functionality. Suppose, also, that it is more likely that Z will be produced by mutation from Y, than it is that Z will be produced by mutation from X. Now, suppose that Y appears by chance mutation in a population of organisms with X. And suppose that natural selection favours Y, and increases the number of organisms with Y in the population. Selection has now made it more likely that Z will appear, by increasing the number of organisms with Y. Selection has explained the origination of a more functional eye.

Godfrey-Smith spots an important ambiguity in the way this sort of story is sometimes presented. One reads that selection makes 'eye precursors more common. But "common" is ambiguous—a trait might become more common in relative terms or in absolute terms' (p. 41). Selection is usually thought of as increasing the *frequency* of one trait over another. If the chances of Z appearing are to be increased by selection, then it must turn out that selection increases the *absolute* number of organisms with Y. Selection need not act in this way, even though it sometimes might do, because Y might increase its frequency over X even when the absolute numbers of both are declining.

What I find puzzling in Godfrey-Smith's account is his appeal to the Malthusian struggle over scarce resources in the context of origin explanations (see also Godfrey-Smith 2009). I have already given my own account of the role intense struggle has for Darwin. If resources are scarce then (Darwin proposes) the intense competition that results means that very slight anatomical differences can give advantages that determine who lives and who dies. And if all but the very best adapted perish, then selection will not be so retarded by unconstrained mating, for less adapted organisms will not survive to sexual maturity. In these respects, struggle accentuates the acuity and the efficacy of selection in promoting adaptation (Lewens 2010).

Godfrey-Smith suggests something different, namely that 'the fact of scarce resources—when it is a fact—ties relative reproductive success and absolute reproductive success together' (p. 42). What he appears to be saying is that when resources are scarce the type whose frequency is increasing is necessarily increasing in absolute numbers, too. But if resources are exceptionally scarce, all types might be decreasing in absolute numbers, some more quickly than others. We will still find that the more successful type increases its frequency over the others (until all become extinct). So scarce resources do not tie relative reproductive success to absolute success.

In earlier work on this issue I suggested that we might do better to think of selection as increasing the efficiency of search in a population (Lewens 2004). Let us return to our example of eyes. In circumstances of exceptionally scarce resources we might find that the overall chances that a Z variant will appear in a population are decreasing, because the absolute number of Y variants is also decreasing, albeit less quickly than the number of X variants. In these circumstances, selection increases the frequency of Y even as Y's numbers decline. But we must remember that our population—even if it is shrinking—is more likely to produce a Z variant if it is composed primarily of Y variants than if it is composed primarily of X variants. Selection explains adaptation in circumstances of exceptionally scarce resources, in the sense that the chances of Z arising are higher if Y increases its frequency than they would have been if X had increased its frequency. This is compatible with the fact that the chances of Z arising are decreasing over time, because of extremely scarce of resources. Understood in one way, selection increases the chances of adaptation in this scenario; understood in another way, selection decreases those chances.

**Human Nature**

I now turn to some brief remarks on Godfrey-Smith's equally brief comments on the notion human nature. Evolution, as he puts it, is 'open-ended' (p. 141). Traits that are now rare might become common; traits that are now common might become rare. Moreover, as we understand developmental processes better we can learn how to quickly change widely distributed traits that until now have been thought inevitable. So whatever 'human nature' might be, we should not think of it as strongly fixed. Even so, Godfrey-Smith is relaxed about weaker approaches to the human nature concept. He points out that Martians could put together something like a field-guide to our planet's flora and fauna. It might feature an entry on *Homo sapiens*. 'In that sense, there is surely nothing mythical about the idea of human nature' (p. 140). A few sentences later he suggests that 'there is nothing problematic in talking of the "nature" of the human species, in a low-key way. As a result of our evolutionary history, there is a genetic profile that is characteristic of our species, which includes important causes of many of our distinctive traits.' (p. 140)

I agree that Martians could put together a field guide with a usable entry on humans. But field guides enable us to assign individual organisms to their proper species when we are out in the field. That is why real field guides include information about characteristic bird song even when that song is learnt, and that is why field guides do not include information about genes, because genes are hard to observe (Lewens 2012). So the entry on *Homo sapiens* in a Martian field guide might include pieces of information like 'wears removable clothes', ‘watches television’, ‘lives in large artificial dwellings’, and so forth, for these are very good (albeit imperfect) diagnostic features of our species members, and they are fairly easy to observe. This first diagnostic notion of human nature as ‘things one might see in a field guide entry’ is evidently not the same as the second notion of ‘a genetic profile that is characteristic of our species, which includes important causes of many of our distinctive traits’. Wearing removable clothing may be diagnostic of *Homo sapiens*, but it cannot be linked in any straightforward way to a characteristic genetic profile.

Godfrey-Smith does not flag to readers how different these two notions of human nature are, which suggests the concept may harbour ambiguities that are problematic by virtue of going unnoticed. It is also easy to raise worries for the second notion of human nature. Recall that he gestures to ‘a genetic profile that is characteristic of our species, which includes important causes of many of our distinctive traits.’ Our species might have a distinctive genetic profile that derives from non-coding regions of the genome; some of our distinctive phenotypic traits may have important causes that are not genetic but epigenetic; many very widely distributed emotional traits, which evolutionary psychologists would reckon part of ‘human nature’, may not be *distinctively* human, but they may instead be shared with related species. All of this should make us wonder what purpose is served by defining human nature along the lines sketched in Godfrey-Smith’s second account. I see more problems in these conceptions of human nature than Godfrey-Smith does.

**The Philosophy of Nature**

Let me close by accentuating the positive. A consistent and important feature of this book is that it is recognisably *philosophical*. One cannot decide what is to count as a major evolutionary transition, or what the explanatory value of Hamilton’s rule might be, or whether species are arbitrary groupings we impose for the sake of communicative convenience rather than evolutionary actors whose existence is independent of taxonomists’ practical concerns, merely by carrying out well designed experiments. One also needs to engage in some abstract reflection concerning what would count as evidence in favour of different answers to these questions, and whether the questions have intelligible presuppositions in the first place. Biologists themselves care about these questions, and it is greatly to the credit of our subject that philosophers have helped to answer them in constructive ways. But if we place too much stress on an image of the philosophy of biology as a discipline continuous with theoretical biology, and largely in the service of biology's own conceptual worries, we risk obliterating a more directly philosophical set of concerns that are thrown up by biological work.

The work of biologists can be usefully integrated into a variety of debates about (among other things) the relationship of humans to nature more generally; about the ability of contingent, fallible creatures to understand the fundamental structure of the universe; about the broad nature of causal relations, and of natural laws; and about how to place puzzling phenomena of morality and meaning in the world as we understand it. We should not be too concerned that many biologists worry only a little about these questions. If philosophers see their successes as fully measured by the degree to which they are participant in the active projects of biologists we also face the possibility—which some will see as a risk, others as a blessing—that philosophers of biology will become detached from the concerns of their mainstream philosophical colleagues, and that their conversations will increasingly target scientists, rather than workers in the humanities.

Godfrey-Smith spends much of this book addressing the ways in which philosophy can shed light on problems that trouble biologists. Thankfully, he also makes room for a project that he calls 'the philosophy of nature': an attempt to 'understand the universe and our place within it' (p. 4). The philosophy of biology, when undertaken in this mode, 'is not giving a philosophical report of what is going on in science, but working out what the raw science is really telling us, and using it to put together an overall picture of the world' (p. 4). This brings the philosophy of biology back into alignment with the concerns of our philosophical colleagues. His fine book gives encouragement to those of us who consider ourselves philosophers first and foremost.


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