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Book review: do microbes question standard thinking in the philosophy of biology?

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Do Microbes Question Standard Thinking in the Philosophy of Biology?

Critical Notice of John Dupré
"Processes of Life – Essays in the Philosophy of Biology"

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1. Introduction

This is a highly welcome book that offers a fresh perspective on the philosophy of biology. It is of interest to both philosophers and biologists and to experienced readers as well as novices. The book is structured into four sections 'Science', 'Biology', 'Microbes' and 'Humans' and consists of a collection of articles written by John Dupré over the past few years. A very wide range of topics are discussed. Among other things, Dupré defends a pluralism that emphasises that while there is only physical stuff, the kind of things composed of this stuff are fundamentally diverse, a modest form of social constructivism, the inseparability of science and values, the thesis of promiscuous individualism that there are various ways of dividing living systems into organisms, and an anti-reductionist position about biology stating that complex objects possess properties that are autonomous from properties of their constituent parts. Dupré also argues that the success in understanding the chemical basis of genetics has undermined simplistic view of inheritance, that the New Synthesis is flawed and instead evolutionary theory is a theory in flux, that biological individuals are typically symbiotic wholes involving many organisms of radically different kinds and genomes, that cooperation is a characteristic of life, that medicine has to take seriously the microbial symbiotic relationships, that contrary to evolutionary psychology human beings are in fact pretty well adapted to modern life, that in no sense of the word 'gene' can biological legitimacy be given to the concept of human race, that human nature as something fixed and constant throughout the human species is an illusion, and that a denial of genetic determinism gives way to a robust sense of human autonomy.

In the remainder of the review, I will focus on the section on *microbes*. This is motivated by Dupré's claims that 'this topic is furthest from the central agenda of the philosophy of biology' (2012: 11) and that attention to microbes demands reconceptualizations, which 'are not just a background development but a major transformation in understanding that needs to be reflected in the philosophy of biology' (2012: 186). The section consists of four papers, three of which are co-authored with Maureen O'Malley (Dupré 2011; O'Malley and Dupré 2007; Dupré and O'Malley 2007, 2009). Microbes are defined as organisms too small to be seen without a microscope. Dupré and O'Malley provide a manifesto for the importance of microbes for the philosophy of biology. They do not disappoint: It is true that the microbial world has long been neglected in the philosophy of biology and that attention to microbes offers new conceptual insights and demands revision of, or at least questions, some widely-held views. On the more critical side, the arguments sometimes suffer from a lack of clarity and some conclusions could have been better argued for. In what follows, I will discuss three main claims defended in the section on microbes, and in order to advance the discussion, I will focus on my objections. Yet this should not distract from the importance and innovativeness of Dupré and

O'Malley's work on microbes, which will hopefully stimulate further research in this area.

2. A new notion of organism and microbial communities as multicellular organisms

Traditional definitions of multicellular organisms demand that the *same* genome appears in differentiated but functionally interrelated cells, thus excluding microbial communities where there are many different genomes. Yet Dupré and O'Malley argue that a more encompassing definition is suggested by microbes. Because microbial communities exhibit well-defined organisation and interactions as functional units and are self-organising entities (Dupré 2012: 172 and 176), they are multicellular organisms. More generally, Dupré and O'Malley complain that the classical notion of an organism is a problematic biological category and that, in particular, it is an inferior or even inadequate conception of the medical individual. They demand a revision of this notion to include microbial communities. In their view, organisms are whichever cooperative systems of cells are most usefully recognized for exploring biological function (Dupré 2012: 203). This, they claim, implies that the microbes that occupy our bodies (such as gut microbes) form part of the human organism.

It is an important point that microbial communities show organism-like properties and that the biological entities of interest in certain scientific investigations are microbial communities or include microbial communities. For instance, one would surely want to agree that for many diseases the entity of concern is not the classical notion of the human organism but the human body including microbes. So here Dupré and O'Malley fulfil their promise and show that microbes lead to important conceptual insights in the philosophy of biology.

However, I would have liked to see more careful reflection on what is required from a definition of an organism. For instance, Dupré and O'Malley write:

> If communities are self-organizing entities that operate as functional units and are more than simply aggregations of individuals (Andrews 1998; Ben-Jacob et al. 2000; Kolenbrander 2000), they can only be excluded from multicellular status if the definition of multi-cellularity is closely based on knowledge of multicellular eukaryotes. (Dupré 2012: 176)

Why is it that self-organisation, being more than simply aggregations of individuals and the operation as functional units is a sufficient condition for a multicellular organism? Similarly, why is it, as the authors argue, that communities which exhibit well-defined organisation and interactions as functional units should be classified as organisms (Dupré 2012: 172)? Also, O'Malley and Dupré claim:

> Many of the characteristics that are used to define multicellularity do not exclude unicellular life when proper attention is paid to bodies of research that illuminate cellular cooperation, developmental processes, competition, and communication strategies amongst unicellular organisms. (Dupré 2012: 189)

While they convincingly argue that microbial communities possess many characteristics of multi-cellular organisms, it is not clear to me why this suggests that microbial communities are multi-cellular organisms. It could well be that only many, but *not all*, of the characteristics of multi-cellular organisms are shown by these communities.

My view on this matter is that the notion of an organism is a vague concept. Whether communities that show organism-like properties are called organisms or not is a matter of convention and is not very interesting. What *is* interesting are the biological

facts and what entities are most fruitfully employed in certain scientific investigations. Similar views have been expounded by Griffiths (2012) and, in particular, by Haber (2013). Haber (2013) in his paper about colonies such as ant or bee colonies also reminds us that debates about the meaning of 'organisms' have already been around for a long while. So I think that there are two important insights to be gained from O'Malley and Dupré's discussion. First, microbial communities show surprisingly complex properties and can perform a wide range of functions. Second, in certain scientific investigations it is most fruitful to employ as basic biological entity a collective including microbes and not the classical notion of an organism.

Moreover, recall Dupré's (2012: 241) doctrine of *promiscuous individualism* – that there are various ways of dividing living systems into organisms driven by particular human goals. In my view, promiscuous individualism suggests that both the classical notion of an organism and the new notion are legitimate concepts, which are useful in different contexts. However, Dupré and O'Malley's discussion gives the impression that the classical notion of an organism is inferior or even inadequate, leaving me wonder how this fits to promiscuous individualism. Also, the claim '[it] is strongly suggested by the several lines of research outlined above, that the individual microbe is not the fundamental ontological unit in microbiology' (Dupré 2012: 184) is puzzling because it seems to suggest that there are fundamental ontological units in microbiology, which is not in the spirit of promiscuous individualism.

I mentioned above that while Dupré and O'Malley's work on microbes is highly important and inspirational, the arguments could be sometimes clearer. An example is their response to doubts about conferring the status of an organism to microbial communities because they lack clear spatial boundaries:

> A first response to these doubts might be that clear boundaries are not necessarily connected to ontological fundamentality. […] Second, the biofilms that are the preferred lifestyle of prokaryotes make possible their study as bounded multicellular entities as well as contradicting common conceptions of bacteria as free-floating individuals in occasional and highly impermanent contact. Finally, there is a large body of empirical work which challenges standard views of boundaries because it reverses expectations about organismal integrity and microbial ubiquity. In regard to the former, the omnipresence of genetic exchange in microbial communities shows organism boundaries to be much more permeable than might have been thought. For the latter […] recent studies taking a more extensive and finely resolved genomic perspective have found that communities of bacteria and archaea in hot springs and soils, for example, do actually have geographic limits. (Dupré 2012: 176-77).

It does not seem clear from this response what position is defended. Is it that clear boundaries are not important for organismality, as the first point and the point about organismal integrity seem to suggest? Or is it rather that clear boundaries are important but that, contrary to what one might think, there are reasonably clear boundaries also for microbial communities, as the second point and the point about organismal ubiquity seem to suggest?

3. Doubts about the genetically isolated lineage in the microbial world

Dupré and O'Malley question that the genetically isolated lineage, which is often seen as the fundamental unit of evolutionary theory, has a real analogue in the microbial world. Their arguments lead to the important insight that there are challenges for the concept of a genetically isolated lineage that are not present for eukaryotes. In this sense Dupré and O'Malley are right that the microbial world offers challenges to standard thinking in the philosophy of biology. However, in my view, the claim that

the genetically isolated lineage has no real analogue in the microbial world is still up to debate (and it might well be that Dupre and O'Malley do not disagree with this sentiment).

Let us look at their arguments in more detail:

> It might be possible in principle to construct evolutionary models in which microbial clones play a similar role to the familiar macrobial lineages. But […] there are some serious difficulties with such models. The most obvious is timescale. Microbial clones have lifespans of hours or days rather than the thousands of year typical of macrobial lineages. This suggests a need for higher level models if any sense it to be made of long-term evolutionary change. It further needs to be decided how the beginning and end of a clone are to be defined for this purpose, especially in the light of a large body of evidence that shows little true or enduring clonality in most bacterial populations (Maynard Smith et al. 1993, 2000). The prevalence of mobile genetic elements moving between microbial units again points to a focus on larger units within which these movements take place. (Dupré 2012: 181)

First of all, their objection about lifespans does not seem to show that for microbes lineages 'have no real analogue in the world'. Rather, it suggests that other concepts will be more suitable for longer timespans; for shorter timespans the notion of a lineage can still be useful. Second, while it is indeed an intricate issue how to define clones, it is not unreasonable that definitions of clones can be found which are useful for certain biological investigations, even if none of these definitions is without its flaws or there is no 'best' definition of a clone. Their third point is about lateral gene transfer – the transfer of genetic material from one microbe to another. Lateral gene transfer indeed poses some problems for the concept of a genetically isolated lineage. However, it is still up to debate how prevalent such gene transfer is. This is also emphasised by Sterenly:

> In one view, successful gene migration is a rare event. It has happened many times, and many genes in the genome of bacterial cell lineages have their original ancestors in other lineages. But that is true only because we are looking at ancient lineages – at the summed results of rare events over 3.5 billion years. Almost all the genes in any given prokaryote clone-line have been replicating together for millions of generations (Lerat et al. 2005). In another view, lateral gene transfer is pervasive on evolutionary significant time scales, but it is very rare for core "informational genes" those responsible for the machinery of replication, transcription and translation (Brown 2003, Woese 2008). In the most extreme view, it is such a pervasive feature of prokaryote evolution as to undermine the very idea that prokaryote evolution is best thought of a branching tree (Bapteste and Boucher 2008; Doolittle and Bapteste 2007; Martin and Embley 2004). (Sterenly 2011: 90-91)

So only if lateral gene transfer is a pervasive feature of the evolution of microbes does it seem justified to claim that the lineage has no real analogue in the microbial world. What Dupre and O'Malley's discussion reminds us is that one should be wary of insisting that lateral gene transfer is sufficiently rare just to prevent serious damage to lineage-based biology.

4. Cooperation as a criterion for life

After providing an insightful survey about the very different forms of life that exist on Earth, Dupré and O'Malley turn to the question of what constitutes life. The life forms that exist on Earth are all of the same kind. Life on other planets or based on other chemicals might be altogether different from what we know and can imagine. Because of this, one may argue that our theorising about life will be severely limited. While this is a legitimate worry, we have to work within our constraints. Thus, like Dupré and O'Malley, we should still try our best in understanding life based on the forms of life we know on Earth.

Among the most widely agreed features of life are the capacity to form lineages by replication and to exist as metabolically self-sustaining wholes, and Dupré and O'Malley also endorse them as necessary conditions of life (Dupré 2012: 223). In addition to this, they advance collaboration as highly characteristic of life (2012: 235). The characterisation they arrive at is that life 'occurs at the intersection of lineage formation and (typically collaborative) involvement in metabolism' (2012: 227). According to their analysis, not only cellular entities but also non-cellular entities are living. For instance, viruses are non-cellular but they should be classified as living when metabolically active (and as non-living when not metabolically active) (2012: 216). Moreover, Dupré and O'Malley complain that the extant literature on life fails to acknowledge that the entities that form lineages by replication are not the same as those that form metabolic wholes. For example, human organisms, as standardly conceived, are lineage-forming entities but are not metabolic wholes because microbes in our gut form part of the metabolic wholes (Dupré 2012: 207).

Dupré and O'Malley's discussion about collaboration is welcome. It teaches us that collaboration is more widespread than usually acknowledged in the philosophy of biology. They are also right to stress that for life as we know it the lineage-forming entities are not the same as the metabolic wholes. Still, I am not sure everyone will be convinced that collaboration is a highly characteristic of life. Suppose we discover a distant planet which is inhabited by billions of different organisms that do not engage in any collaboration, but can reproduce, are metabolically self-sustaining and show all the other common criteria of life. It may be a judgement call, but some would probably still want to call such organisms life and thus would not endorse collaboration as highly characteristic of life. Moreover, when characterising life, Dupré and O'Malley use the notion of a lineage. As discussed above, they doubt that the notion of a lineage has a real analogue in the microbial world. Given this, it is somewhat surprising to see that here they use the notion of a lineage without any concerns.

Furthermore, I would have liked to see more reflection on what is at stake when assigning to an entity the status of living. What I have in mind are reflections on questions such as: Life is a vague term in every-day language. So might it not be that there is no single best characterisation of life but several equally plausible characterisations? In a similar fashion, when Dupré and O'Malley claim:

> Although we do not claim to have provided a definition of life, we do believe we have offered a view of living matter that offers a flexible resource for understanding the many ways in which life can be organized. (Dupré 2012: 227)

one would like to know more about what exactly they have provided if their aim was not to provide a definition of life. Related to this, Dupre and O'Malley defend collaboration as highly characteristic for life but *not* as a necessary condition of life (Dupré 2012: 225, 227). One might think that either a feature is necessary for life or it is contingent and thus not that important at all. Their defence of collaboration as highly characteristic of life does not fit into this scheme. Thus it would have been helpful to see a discussion about how features can be important for characterising life even if they are not necessary conditions of life. Also, Dupré and O'Malley repeatedly stress that there is no sharp dividing line between life and non-life (Dupré 2012: 227), but it is not clear to me that this follows from their characterisation of life.

Dupré and O'Malley highlight that their view on cooperation differs from the standard view in the philosophy of biology. It would be interesting to know more about what exactly is the difference. Granted, according to the picture painted by Dupré and O'Malley, cooperation is much more widespread than often thought and typically the units of selection are collections of organisms. Yet what philosophers of biology are bound to ask is whether their view differs from the widely held conviction that evolution is a selfish endeavour. Is it just that individual parts are selfish but they cooperate because this increases their fitness? So we see all this collaboration because it is (or at least was initially) advantageous for the individual parts. Or do Dupré and O'Malley endorse the more radical view that the individual parts or the cooperative units are not selfish. Some of their comments such as

> Many of these are activities that no individual microbe can accomplish on its own, and the collective behaviour is often achieved with a cost for individual "altruistic" micro-organisms (if they are perceived through the lens of selfishness) (Dupré 2012: 222)

or 'the unit of selection, the entity in which selfishness may *perhaps* be expected as norm' (Dupré 2012: 226, my emphasis) do not seem to rule out a more radical position.

Finally, I have mentioned above that some of the conclusions could have been better argued for. An example is the following paragraph:

> It is clear to us that leaving viruses out of evolutionary, ecological, physiological, or conceptual studies of living entities, would allow only an incomplete understanding of life at any level (Wilhelm and Suttle 1999; Weinbauer and Rassoulzadegan 2004; Suttle 2005). This deep and extensive interaction is too biologically important, from our perspective, to be considered as purely parasitic. Conceived of collaboratively, cellular life is constantly 'bathing in a virtual sea of viruses', within and without every cell, with evolutionarily significant consequences for the past, present, and future of all cellular life-forms (Bamford 2003: 232). In fact, says virologist Dennis Bamford (2003, 235) it is time to consider dividing life into two realms: the cellular realm and the viral one. He believes that only by dealing more thoroughly with a concept of life fully cognizant of the role of viruses will we be able to achieve an adequate view of life even as it applies to its cellular manifestations. (Dupré 2012: 216).

In this paragraph Dupré and O'Malley claim that viruses should be regarded as a form of life, but arguments as to why this should be the case could be more explicit.

Critical comments aside, this is a ground-breaking and inspiring book, which every philosopher of biology should have read. Among other important contributions, this book shows that attention to microbes demands revisions of, or at least questions, standard thinking in the philosophy of biology. Philosophers of biology can no longer ignore the world of microbes.

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