

Biological Explanations, Realism, Ontology, and Categories

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Published in *Studies in History and Philosophy of Biological and Biomedical Sciences* 44: 617-622, 2013.

Extended review of:

**Processes of Life: Essays in the philosophy of biology** John Dupré, Oxford University Press, Oxford, 2012, pp. 350, Price £30.00, CA\$57.00, US\$55.00, Hardback, ISBN-13: 9780199691982

## 1. Introduction

There is much of interest in this collection of sixteen of John Dupré's most recently published papers, three of them with Maureen O'Malley. In it, Dupré continues to fruitfully shape the field by paving two-way streets between philosophy, microbial biology, genomics, systems biology, epigenetics, and other empirical literatures. But rather than focus on the book's many virtues, I will focus in detail on problems for arguments that Dupré gives for some of the book's more philosophical conclusions, with the aim of helping the book advance us even more than it already does. Section 2 of this essay shows how a reductionist could defend against Dupré. This helps, in section 3, to cast doubt on Dupré's rejection of monism and improve his constructivism. Section 4 sharpens and challenges Dupré's argument for his recent view that biological processes are more fundamental than biological entities. Finally, section 5 raises difficult questions for old and new versions of Dupré's pluralism about biological categories and entities. This plan excludes any focus on the more applied papers in the final section of Dupré's book, but rest assured they are well worth reading.

## 2. Reductionism

You are studying a lynx. Suppose you know everything about its:

*Components*, e.g., all of its cells.

*Narrow relations*, e.g., how all the cells interact and are assembled into organs.

Would this information suffice for you to in principle know everything about the lynx (aside from certain extrinsic facts, such as whether there is a tree to its left)? Dupré suggests that reductionists would answer "Yes" (p. 130) and then argues repeatedly that the correct answer is "No". Despite having no allegiance to reductionism, I think two general problems afflict Dupré's discussions here. First, the discussions do not sufficiently argue that anti-reductionist attacks should target the version of reductionism exemplified by a "Yes" answer to the lynx question. This is an important deficiency, now that readers widely appreciate that there are many versions of reductionism. Moreover, it is difficult to see why critiques should focus on the version of reductionism that the lynx case exemplifies. Consider the following:

*Wide relations*, e.g., the relations between the lynx's cells and things *outside* of it, such as ambient mean kinetic energy (measured by temperature), external microbes it interacts with.

Most reductionists reading *Processes of Life* would insist that in many contexts there are many facts about the lynx (and of the sort that interest Dupré) that could not be "fully understood" (p. 129) without knowing about its relevant wide relations.<sup>1</sup> So those reductionists would answer "No" rather than "Yes" to the lynx question. Being able to explain higher-level phenomena in lower-level terms is important to the typical reductionist. But that is a *vertical* commitment. Reductionists needn't restrict how far out, *horizontally*, an explanation travels along lower levels. A *wide reductionism* is often available.

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<sup>1</sup> Regarding those who continue to privilege the intrinsic features of things at the explanatory expense of *all* their extrinsic features or context, I agree with Dupré's criticisms (e.g., Barker, 2010; Wilson, Barker, & Brigandt, 2007).

The second general problem is that many reductionists could reasonably resist or absorb Dupré's chief counterexamples, which are of two main sorts. The first appeal to downward causation, which I read as threatening the following reductionist principle:

*Reductionist occurrence principle:* If an occurrence is one that a higher-level biological science can explain in principle, then some lower-level science (biological or otherwise) can also, in principle, explain it.

Consider, for example, mRNA in one of your cells. Just before midnight on a Monday it is translated into an amino acid sequence. The sequence folds into a specialized shape, resulting in a functional protein early on Tuesday. What explains the appearance of this protein on Tuesday? Dupré is correct to say that we must look beyond the amino acid sequence itself. But he insists we "must" (p. 140) additionally look to *the cell*, described as a thing at a higher level than the appearing protein it contains. This requirement on adequate explanation would falsify the reductionist occurrence principle. But Dupré asserts rather than supports the requirement. Why *must* we appeal to the cell in higher-level terms to explain the protein's appearance on Tuesday? I don't think we could finesse any of Dupré's examples to infer such a requirement. The reason is that if higher-level features (such as the cell) are causes that figure in adequate explanations of associated effects, then the effects are also adequately and diachronically explained by the lower-level features that determine those higher-level explanatory causes. Consider a more familiar case from Sober (1999). If the higher-level event of smoking at some time causes cancer at a later time, this is because the lower-level determination of cigarette smoke at the earlier time "is doing the work" (p. 548).<sup>2</sup> An even less ambitious claim will do in the mRNA case: If a higher-level event involving the cell late on Monday causes the protein's appearance on Tuesday, this is

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<sup>2</sup> That lower-level determination will doubtlessly include the smoke's lower-level components, lower-level narrow relations, and lower-level wide relations.

because lower-level events in and outside the cell late on Monday are doing the explanatory work.

Dupré's second main sort of counterexample targets something like this reductionist principle:

*Reductionist category principle:* If phenomena at a higher level are of a kind, i.e., constitute a higher-level category, then there is some lower-level category from which we could, in principle, explanatorily infer those phenomena comprising the higher-level category.

For example, many people suffer conditions that constitute the higher-level category *cystic fibrosis*. The above principle then implies that there is some associated lower-level category, such as *molecular gene for cystic fibrosis*. To belong to the associated lower category is to have some cluster of intrinsic or extrinsic properties, such as particular DNA structures. Further, from any person's having this cluster of lower-level properties we can supposedly explanatorily infer that the person has a cluster of conditions that constitute cystic fibrosis. In the past this was often ambitiously presented (and even today often interpreted) as implying commitment to a one-one mapping from the lower-level category in question to the higher-level category. On that additional commitment, having, say, the DNA structures would be necessary and sufficient for having cystic fibrosis.

Dupré counters that many vertical relations between categories are not one-one. He says, for instance, that the cystic fibrosis example has a *many-one* aspect, meaning cystic fibrosis is *multiply realizable*. Rather than any particular molecular gene being necessary and sufficient for having cystic fibrosis, some 1000 different DNA mutations are each, in their respective contexts, sufficient for having it (p. 137). The mention of respective contexts points to a one-*many* aspect

as well: Each mutation generates cystic fibrosis in some associated contexts, but other effects in other contexts. This aspect amounts to each kind of mutation's *context sensitivity*.

But reductionists can reasonably absorb context sensitivity, by articulating two of the forms that can be taken by the wide reductionism introduced in the lynx case. One form involves recognizing *wide categories*, i.e., the (partially) extrinsic individuation of relevant lower-level categories such as relevant kinds of mutation. Being one of the 1000 mutations that can help produce cystic fibrosis is not *just* to be some particular DNA structure. It is to be some such structure in certain explanatory patterns of systematic relations with other specific kinds of cellular and extra-cellular relations extrinsic to that structure. Alternatively, a reductionist can resort to kinds of *wide systems*, claiming the relevant lower-level category is a broader lower-level system that includes the mutation and other cellular and extra-cellular features as interacting parts.

What about multiple realizability? Reductionists can reasonably resist that objection by drawing on influential dilemmas that challenge the coherence of the very notion of multiple realizability (Shapiro, 2000, p. 647). Alternatively, reductionists can absorb examples of multiple realizability. While it may upset one-one mappings, multiple realizability certainly does not upset all forms of explanatory inference available for the reductionist category principle (Sober, 1999). Take a Nagel-style reductionism, on which to explanatorily infer a higher-level category from a lower-level one is to show that belonging to the lower-level one deductively entails – with appropriate empirical statements and bridge principles – belonging to the higher. Present day reductionists do not need to commit to this dated entailment view and the problematic theories of explanation it rests on. But even if they do, multiple realizability does nothing to block the sought-after entailment when the multiple lower-level categories in question are finite in number

(e.g., 1000) (Sober, 1999, pp. 552-554). Less ambitious reductionisms have even more options for absorbing multiple realizability.

### 3. The Nature and Limits of Science

When aiming for conclusions about the nature and limits of science in the early chapters of the book, Dupré leans heavily on his recurring critique of reductionism to argue that naturalistic monism is not credible. This monism is a metaphysical thesis (p. 23) about the fundamental nature of reality, a thesis more ambitious than the mere claim that all stuff is material (p. 37). Because this monism can associate with reductionism rather than eliminativism, it allows that there are many material kinds of thing. But it adds that these kinds reduce to *one* fundamental kind of thing (p. 34). So Dupré seems to think it is this tally of *fundamental kinds* that monism gets wrong. I don't know whom Dupré sees as committing to such monism, but I think Robert Boyle's 1666 corpuscular theory of matter instantiates it. That theory says that all kinds of material bodies are constituted at their most elemental level by this or that arrangement of corpuscles.

Here is Dupré's main argument against naturalistic monism, the *monism myth argument*:

1. If naturalistic monism is credible, then the unity of science is credible (p. 23).
2. But the unity of science is not credible (pp. 23-34).
3. So naturalistic monism is not credible.

To be relevant as the first premise suggests, the unity-of-science view in question will have to be unity of what Dupré calls the *contents* of science, which he primarily treats as reductionism. You might then object that the premise is incorrect because reductionisms are not the kinds of views that could possibly transmit credibility to more metaphysical views such as naturalistic monism.

But suppose premise 1 survives your objection. Then, in the second premise, at least one of the reductionisms that transmits credibility to naturalistic monism is likely to be one of the reductionisms that, we saw, Dupré fails to defeat. As evidence for this, consider that the wide reductionism that was left standing is just the sort that Robert Boyle paired with his monistic corpuscularianism. For Boyle, such relational properties of corpuscles as relative position and location, together with wide interactions between differently textured aggregates of corpuscles, help constitute higher-level explanatory kinds (Anstey, 2000; Banchetti-Robino, 2012; Kaufman, 2006).<sup>3</sup>

Moving on from monism, Dupré defends agreeable balances between realisms and constructivisms about sciences, including the most recent articulation of his promiscuous realism. I like to express this as a combination of realism about some scientific *categories* (or kinds) and constructivism about scientific *classification*. Consider the category *interbreeding species*. Roughly speaking, a group of populations belongs to it if and only if genes can flow between those populations but not from them to other populations. This category has one sort of reality if the groups of populations belonging to it *really exist* and are distinguished by the gene flow relations mentioned. The category has yet another and more scientifically interesting sort of reality if such gene flow relations tend to causally *explain* the relatively high degrees of phenotypic similarity found between populations of a typical group belonging to the category. Dupré is frequently clear that both sorts of realism are probably enjoyed by many categories (pp. 7, 49, 70, 77).<sup>4</sup>

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<sup>3</sup> Boyle often claimed to explain his experimental findings by appealing to such higher-level kinds, without actualizing the possibility of lower-level explanation that he also recognized (e.g., Clericuzio, 1990). In more recent physics, when any fundamental particles or forces putatively explain higher-level phenomena, they again do so partly by virtue of their dispositional relational properties, by reference to which they are defined (Nichols, 2010).

<sup>4</sup> Though occasionally he is cagey about this issue (e.g. p. 45).

The constructivism enters when we represent the relationships between multiple categories within classification schemes (p. 49). It is *not* that these schemes must or typically do fail to accurately (really) represent categories and relations between them (p. 41). Rather, when developing any classification scheme that may turn out relatively accurate, we necessarily have constructively chosen to develop that scheme rather than others that also could have turned out to accurately represent different categories or relations. For example, while developing a classification based around the *interbreeding species* category, we necessarily opt against developing one based around the *ecological species* category (Ereshefsky, 1992). There are many distinct and relatively accurate possible classifications that we could develop, but practically speaking we cannot develop them all. An appeal to our values often helps make the necessary choices.

Although this classification constructivism and attempts to support it by appeal to species pluralism are increasingly popular, two of the ism's features are often overlooked. First, this constructivism is so tame that all realists in audiences like Dupré's have probably long believed it. True, this constructivism opposes a radical sort of uniqueness thesis. That thesis says we aren't forced to choose between classifications because there is *just one* uniquely true classification of everything—not just a place where a great many different and accurate classifications are brought together, but a lone, integrated, accurate, master classification of everything. Dupré sometimes seems to have this uniqueness thesis in his cross-hairs, describing it as the hope for “the” true story (p. 44), converging on “the same story” (p. 44), and achieving “a unique and privileged” classification (p. 49). I agree that we should reject such a uniqueness thesis. It concerns horizontal unity of categories rather than or in addition to any vertical unity



that is suggested by some of the reductionisms that survive Dupré's attacks. But what realist in Dupré's audience would dare believe in the uniqueness thesis anyway?<sup>5</sup>

Second, once we appreciate the tameness of classification constructivism, we can give more convincing arguments for it. Rather than appeal to species pluralism, which authors have challenged since Dupré's earlier work favoring it (Barker & Wilson, 2010; de Queiroz, 1999; Wilson, Barker, & Brigandt, 2007), the classification constructivist can simply note that any researcher must implicitly or explicitly make the following sort of choice when developing a classification: Should I aim for an accurate ecological classification involving categories such as *predator*, or an accurate evolutionary classifications involving, say, *free rider*?

#### **4. Process Ontology for Biology**

Chapters 4 and 5 narrow the focus to biology. A main contrast here is between entities (or things) and processes. Entities can endure across time. But they can also exist statically at a moment. Processes cannot. They are essentially dynamic and extend across time. Dupré suggests that we typically accept an entity ontology for biology, and he argues that we should revise this in favor of a process ontology. On both theses, both entities and processes exist. But the theses propose opposite directions of dependency relations, or opposite views about what is ontologically fundamental, as clarified in my interpretation and labeling of them:

*Processism*: processes are ontologically fundamental to entities, with entities deriving what existence and explanatory causal power they have from processes in which they are nodes.

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<sup>5</sup> More precisely, who would believe *both* that there is just one master classification to uncover *and* that this would license the view that our classificatory activities could be free of our choices or decisions?

*Entityism*: entities are ontologically fundamental to processes, with processes deriving what existence and explanatory causal power they have from entities and interactions between them.

In favoring processism, Dupré says “the things we distinguish in our descriptions of life, at least, are always to some extent abstractions from the dynamic processes that *ultimately constitute* life” (p. 72; emphasis added).

Dupré’s most promising argument for processism concerns familiar biological categories, e.g., *species*, *gene*, and *organism*. The basic idea (p. 71, p. 85) is that processism best explains our category difficulties—the difficulties that have confronted our attempts to “provide philosophically adequate accounts of these various categories” (p. 70). To ensure validity, we should interpret this as a *likelihood argument* that favors processism over entityism:

1. If the truth of processism would make the category difficulties more probable than would the truth of entityism, and the category difficulties in fact obtain, then the category difficulties evidentially favor the truth of processism over the truth of entityism.
2. The truth of processism would make the category difficulties more probable than would the truth of entityism.
3. The category difficulties in fact obtain.
4. The category difficulties evidentially favor the truth of processism over the truth of entityism.

Let’s accept premise 1 and start with premise 3. The category difficulties appear to consist primarily in two sorts of failures of *traditional essentialist* accounts of biological categories (Barker, 2013; Ereshefsky, 2010). One feature of such accounts is their *modal* requirements on the properties that make entities belong to certain categories:

*Modal requirements:* There is some set of properties associated with the category in question such that each property in that set is *necessary* for belonging to the category, and together the properties are *sufficient* for belonging to the category, and these properties are important parts of the *explanations* of many of the other properties typical of entities belonging to the category.

Some of the category difficulties are then modal failures, as it seems *variation* characteristic within the biological world ensures that paradigm biological categories do not meet the modal requirements of traditional essentialism. For instance, some traditional essentialist accounts of the *species* category imply that a group of populations is a species *only* if it has the property of being monophyletic, i.e., only if the group includes all and only the descendants of a given ancestor. Yet biological complexities such as lateral gene transfer, combined with intuitions about which groups really are species, imply that this traditional essentialist necessity claim is false (pp. 76-77).

Another feature of traditional essentialist accounts is their location requirement on properties that make entities belong to categories:

*Location requirement:* Each of the properties in the set of properties that makes an entity belong to the category in question are intrinsic rather than extrinsic properties of those entities, i.e., those properties are realized by features entirely within the entities that have those properties, rather than partially or wholly realized by any features outside the entities.

Many category difficulties amount to violations of this requirement, owing to *context sensitivity* in the biological world. Even if being monophyletic turned out necessary for being a species, this

would constitute a location failure because being monophyletic is an extrinsic property of groups, realized partly by a group's genealogical relations to groups outside of it.

Given all this, the most convincing trouble is for premise 2. Neither processism nor entityism on their own assign *any* probability values – low, high or otherwise – to the category difficulties. This is because both processism and entityism are very general metaphysical views that say nothing about the sort, or nature, of processes or entities in question. There are many sorts of possible processes, and many sorts of possible entities. Some sorts of possible processes would, if they existed and typified the biological world, be of an invariable sort, featuring perfectly predictable parts and interactions. Such processes would actually make the category difficulties for traditional essentialism quite *improbable*. Other sorts of possible processes would be of variable sorts with parts and interactions that preclude infallible prediction. These different processes might make the category difficulties for traditional essentialism quite *probable*. The point is that without appending to processism some auxiliary assumptions about whether the probability increasing or instead the probability decreasing sorts of processes typify the biological world, the category difficulties receive *no* probability value from processism, making premise 2 false.

We might try to mend premise 2 by adding the needed auxiliary assumptions. But as we will shortly see, we can also add such auxiliaries to entityism, in a way that allows Dupré's competitor to render the category difficulties maximally probable. In that case, processism could not make the difficulties more probable than the competitor, no matter which auxiliaries about sorts of processes we pair with processism. Therefore, premise 2 would remain false.

Addition of the auxiliaries to the competitor entityism is justified independently rather than *ad hoc*. There already exist well-developed alternatives to traditional essentialist accounts of

categories. Homeostatic property cluster (HPC) accounts, for instance, have been applied to Dupré's exemplar categories *species* (Boyd, 1999; Wilson et al., 2007), and *gene* (Wilson et al., 2007), and *organism* (Wilson & Barker, 2013). We can reasonably interpret these accounts as supposing both that entities are more fundamental than processes, and that the categories in question are variable and context sensitive. After all, these accounts were designed to accommodate variability and context sensitivity. We can, therefore, reasonably combine entityism with the independently attested auxiliary that says the biological categories to which entities belong are homeostatic property cluster categories. And this combination would render category difficulties for traditional essentialism maximally probable, making the truth of premise 2 impossible.

This points to a more constructive problem. Seeing the work that auxiliaries do may suggest they do *all* the work—that processism and entityism do none of the probability rendering when combined with auxiliaries. The rarefied metaphysical issue of whether it is processes or entities that are ontologically fundamental may be wholly independent of whether the categories that entities belong to are variable, context sensitive or accord with traditional essentialism. This would instruct us to *not* aim for impossible evidential contact between the category difficulties and the truth-value of processism. But it would also point Dupré's intriguing idea in the direction of *discovery* rather than *justification*. That is, aim to show instead that if researchers who already pay close attention to biological processes were to also sometimes or always work *as though* the additional metaphysical thesis of processism were true, then this would have net benefits in terms of discovery. If that were shown, processism would prove to be a practically *useful* thesis, even without light shining on its *truth*-value.

Questions abound, but they are progressive ones. If we are evidentially disconnected from the truth and justification of different ontological frameworks, presumably we should think that the same discoveries are, in principle, possible under each framework. But then in what ways would operationally adopting one ontological framework have a greater *benefit* over another in terms of discovery? Would it benefit by making it *practically easier* to discover things that are in principle accessible from both frameworks? Or would it benefit by *speeding up* actual discoveries that are in principle available under both? We need to answer these questions to measure which “discovery benefits” one framework might have over another. Similarly, we also need a clearer sense of what exactly such a “framework” is. Measurement then becomes important because the argument in favor of the comparative usefulness of processism in some cases must be empirical.

Dupré offers ideas congenial to these remarks, showing philosophers can aid empirical work by clarifying ontological frameworks in systems biology. To build on this idea, we philosophers of biology need to take more seriously the idea that we cannot support our pragmatic theses without controlled empirical tests. We must explicitly test “ontological” frameworks against each other by controlled measure of discovery-related variables. To my knowledge, there is no precedent that exactly matches this sort of testing, which makes it difficult to helpfully elaborate on what, exactly, it and the practical steps to it would involve. Partly by drawing inspiration from work like Dupré’s, and from related sorts of testing, I could only offer some preliminary suggestions.<sup>6</sup> But the more general point is that more theses in

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<sup>6</sup> Drawing, for example, from Dupré’s criticisms of neo-Darwinism in chapter 9, it would be interesting for interdisciplinary teams involving members proficient in interviewing methodology, philosophy of biology, metaphysics, systems biology, and other relevant biological theory and empirical methods, to interview research teams who helped discover the processes that problematize neo-Darwinism. Interview teams could investigate the extent to which the research teams operated within something like a processism framework. To do so the interviewers could begin with certain understandings of what it is to work within such a framework, informed by what they antecedently know from other relevant work. Additionally, answers from those interviewed could inform

philosophy of biology are pragmatic and empirical than we often realize, and that if we wish to adequately defend them, then we must press into these relatively uncharted testing waters. For help, we can and ought to vastly enrich and extend the sort of interdisciplinary connections that Dupré has done much to build.

## 5. Pluralism about Biological Categories

Pluralism about biological categories is a recurring topic in the second half of Dupré's book. Any instance of pluralism concerns a broader category, e.g., *species*, and associated narrower ones, e.g., *interbreeding species*, *phylogenetic species*, and *ecological species*. The multiple narrow categories are said to be equally real and equally legitimate for science to recognize and classify, in their respective contexts. But in defending this position, Dupré and others often hastily dismiss *complementary* theoretical monisms. Pluralism about narrower categories is consistent with a theoretical monism that says the associated broader category is also real and theoretically relevant. Dupré opts against this pairing, preferring “total abandonment of...theoretical commitment” to broader categories *species* and *gene*. He wishes to retain only vague reference to such terms, and in those cases, only to facilitate communication (p. 111). But he bases this preference on the failure of those categories to accord with traditional essentialism. This strategy is incomplete because it does not address alternatives to traditional essentialism, e.g., the HPC accounts discussed above, which see there is good reason to not hold the broader categories to traditional essentialist standards. I wonder whether some of the countless successful communications facilitated by use of “species” and “gene” would have been miraculous if there were *no* significant theoretical overlap between various uses of those terms.

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modifications to the initial understandings of the interviewers. Although this would not allow the past biological discoveries now being discussed to serve as parts of tests of a pragmatic processism, it may improve the concepts and experimental design needed for future tests.

For the other worry I will raise about Dupré's pluralism, consider a special kind of pluralism. It multiplies the number of individual entities recognized, in addition to the number of categories. For instance, Dupré argues that the *organism* category associates with at least two narrower organism categories: *monogenomic organism* and *polygenomic organism* (p. 198, p. 241). Moving from recognizing just the former of these categories to both adds polygenomic individuals to the catalogue of individual entities.<sup>7</sup> I have nothing against polygenomic organisms. I may even be one, and I just happen to now exist in very nearly the exact chunk of space as a distinct monogenomic organism that follows me around. My follower consists in cells that each descended from a genomically identical or very similar ancestral zygote, created by sperm and egg from my parents. If I am a polygenomic organism, then I consist of these exact same monogenomic cells *plus*, say, the trillions of genomically distinct bacteria that reside around my gut. Dupré, often with Maureen O'Malley, argues that such cellular composites are every bit the organisms that my monogenomic follower is. The question is: why think this way? Well, the collaboration between the microbes and many of my other cells is astonishingly intricate. The collaborations are also vitally important to most parties involved and feature in many valuable explanations, predictions, and interventions—in predictions about cold adaptation (p. 195) for example, and explanations and potential interventions concerning colon cancer (p. 238).<sup>8</sup>

But the intricacies and importance do not alone answer the question. To justify recognizing any individual is to justify recognizing its boundary as a boundary, and justifying

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<sup>7</sup> In the species case, on the other hand, one could add the *ecological species* category to the list of recognized narrower *species* categories without influencing the number of individual organisms recognized.

<sup>8</sup> In chapter 7 Dupré brings up further considerations in favor of recognizing polygenomic organisms, including genomic chimeras, intermixing of maternal and offspring genomes, and organ transplants.



such boundary claims additionally requires boundary *principles* that take us from facts about intricacy and importance to the boundary claims. An example principle:

If *Enterococcus faecalis* bacteria interact with intestinal cells of human organisms in intricate ways that benefit those cells under normal conditions, but under certain abnormal conditions the interactions change and this is an important explanatory cause of various effects in human organisms such as colon cancer, then we should recognize the typical human organism boundary as enveloping both intestinal cells *and* the bacteria, in addition to or in place of an organism boundary that excludes the bacteria as entities external to and which interact with intestinal cells inside it.

Even if Dupré were to deny that he presupposes this principle in his discussion of *Enterococcus faecalis* (pp. 237-238), he repeatedly and tacitly presupposes such principles elsewhere in *Processes of Life*. No matter how well these principles support certain boundary claims, they receive no thorough defense of their own in the book.<sup>9</sup> Moreover, they are notoriously difficult to defend. If I suffer several nasty conditions including colon cancer, and main parts of the explanatory cause of this involve interactions between misbehaving microbes and my intestinal cells, then why does the explanation improve by, or license, recognizing an organism boundary (perhaps mine) that envelopes the microbes? Being an important part of the causal explanation of something certainly seems relevant to boundary constitution. And obviously, things such as

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<sup>9</sup> Perhaps Dupré is willing to let them go undefended because he is a constructivist about the boundaries of individuals, dropping the realism that we saw him retain about category boundaries in the face of pluralism. In opposition to this exegetic interpretation of Dupré is his claim that his view of the narrower categories *polygenomic organism* and *monogenomic organism* is “parallel” with his realist view of narrower *species* categories, such as *interbreeding species* and *ecological species* (p. 241). But favoring the interpretation is Dupré’s claim that in the organism case, it is not that we appeal to our interests merely to constructively decide which *real and discovered boundaries* to recognize in classifications; rather the category boundaries themselves are in part “a matter of human decision” (p. 241, my emphasis; see also p. 153). But even if his constructivism about individual boundaries turns out more thoroughgoing than any about, say, *interbreeding species* boundaries, the boundaries are still recognized. The need for justified boundary principles then remains. One could sidestep this problem by only pragmatically operating *merely as though* real or constructivist boundaries are there. Although this option surfaces occasionally in *Processes of Life* (e.g., p. 232), it would require thorough contrastive empirical support of the sort discussed in connection with processism.

symbiotic interactions between intestinal cells and bacteria are theoretically interesting. But a vast range of things interact with parts of me in interesting causally explanatory ways, without our thinking that we should recognize a vast array of organism boundaries, and without jeopardizing the explanations when we do not. I suppose we could change this habit and recognize vast numbers of organism entities that my traditionally recognized body is a part of. But this threatens to land us upon a mere verbal change, where what were once called the limits of explanatorily salient causes are now called boundaries of various sorts of organisms.

To end, note that the two concerns I raised for Dupré's pluralism suggest that chapter 12 in *Processes of Life* is a tantalizing outlier in two respects. First, in it Dupré and O'Malley clearly and explicitly invite us to resist recognizing boundaries around living things. Instead they describe a boundary-resistant continuum that combines many degrees and distinct (probably incommensurable) kinds of metabolic collaborations that are important for understanding living things.<sup>10</sup> Second, dwelling on the continuum rather than boundaries allows them to uncharacteristically make the case for what I would recognize as a sort of theoretical monism about the broader category *living thing*. As other recent work also attests (Barker & Velasco, *in press*) perhaps other categories treated pluralistically in *Processes of Life* could benefit from this combination of boundary dissipation and theoretical integration.

## **Acknowledgments**

Thanks to Ellen Clarke, Michele Martin, Neeraja Sankaran, and Rob Wilson for helpful comments on a draft of this essay, and to Ingo Brigandt for some helpful leads.

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<sup>10</sup> Ultimately they *do* implicitly presuppose boundary claims, e.g., when implying that a virus *is not* presently alive if not collaborating (p. 227), and when they leave open the possibility that some mere chemical systems indeed *are* alive (p. 225). Nonetheless the wariness of boundary claims is more apparent here than in the rest of the book.

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