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Feelings in Guts and Bones: Reply to Lewis, Magnus, and Strevens

Anjan Chakravartty: Scientific ontology: integrating naturalized metaphysics and voluntarist epistemology. New York: Oxford University Press, 2017, 296pp, US$74.00 HB

Anjan Chakravartty[1]

In *Scientific Ontology*, I attempt to describe the nature of our investigations into what there is and associated theorizing in a way that respects the massive contributions of the sciences to this endeavor, and yet does not shy away from the fact that the endeavor itself is inescapably permeated by philosophical commitments. While my interest is first and foremost in what we can learn from the sciences about ontology, it quickly extends to issues that go well beyond scientific practices themselves, for two reasons. For one thing, it is not merely the case that philosophical considerations are relevant to ontological judgments even in the sciences; additionally, there are good philosophical reasons to believe that *different assessments* of these considerations are rationally permissible, which entails that rational agents may well come to different conclusions about scientific ontology in ways that admit of no ultimate resolution, in principle. Secondly, given this defensible variability of assessment, we have good reason to regard some disputes about whether particular patches of ontological theorizing deserve the label "scientific," as opposed to "non-" or "un-scientific," as ultimately irresolvable as well. All of this may be controversial, but I take it to be a true description of our precariously human epistemic condition in the realm of ontology.

In the following, I discuss some rich, penetrating thoughts about these ideas by Peter Lewis, P.D. Magnus, and Michael Strevens. Given their unwitting but nonetheless commendable decency in engaging with largely separate issues, I will discuss their challenges in sequence, in a direction of increasing skepticism. Lewis lends a sympathetic ear to much of the account, but feels that something very important—an appropriately deflationary approach to ontology, as may best fit one of the major case studies of the book—has been left out. Magnus likewise feels that

✉ Anjan Chakravartty
chakravartty@miami.edu

[1] Department of Philosophy, University of Miami, Coral Gables, FL, USA

vital things are missing, namely a proper consideration of the role of values in ontological judgments as well as entire ranges of ontological concern, with potentially serious consequences. In both cases, it will be my contention that the framework for thinking about ontology I present is, in fact, inclusive of what they take to be missing (or in one case, potentially so). Strevens is less concerned with what may be left out and more concerned with the correctness of what is there, arguing that the framework is inherently flawed in its description of criteria relevant to ontological judgments, as well as in the theme that some disagreements about ontology are unassailable. I will contend, however, that neither concern is compelling.

Lewis' deflationism: realism versus pragmatism about scientific ontology

There is a sense in which Lewis' worries are the most highly focused, for he illustrates them with an extended examination of a specific example: attempts to understand the nature of subatomic particles, as described by the Standard Model in contemporary particle physics, in terms of group-theoretic structures. In the book I consider a recently fertile, philosophical research program whose goal is to understand how these mathematical descriptions should be interpreted—forms of so-called ontic structuralism—so as to yield a scientifically respectable account of the ontology of this domain. The upshot of Lewis' pithy reconstruction of the history of the relevant scientific theorizing, however, is a much more general concern about how best to characterize the very project of scientific ontology. Throughout the book, I employ what I describe as an "earnest idiom" in which ontological claims are presented and taken at face value as putative descriptions of what things and kinds of things exist, and what these things are like. I contrast this with deflationary views of ontology, which add qualifications that yield various non-literal interpretations of otherwise earnest talk, according to which these claims are ultimately elliptical for something else such as historical, social, or pragmatic conventions of description. Lewis thinks that deflationism is a better approach to the ontology of particles and deserves more play more generally.

I think that there is a great deal of agreement between the moral I aim to derive from my discussion of structuralism and Lewis' advocacy of deflationism. He explains that the form of deflationism he has in mind is one according to which our empirical knowledge of the world can be expressed using different conceptual schemes, none of which is uniquely "true" or "correct" or "fundamental"—in the spirit of Carnap, "different conceptual schemes have different pragmatic virtues, and might be useful for different purposes." With the caveat that talk of different descriptions being "non-uniquely true," etc., is itself a minefield of issues requiring elaboration, the ultimate moral of my own discussion of particles is that as one considers finer and finer-grained questions about the metaphysical nature of something, one often comes to a point at which, even if one is tempted by an earnest realism up to that point, one now finds oneself unable to commit in the same sort of way to finer-grained claims. It is one thing to believe that things have properties, but

something finer-grained to believe that properties are universals. In the language of the book, at a certain point the magnitude of the metaphysical inferences involved may become sufficiently great that one is no longer willing to assert the simple truth or falsity of a claim. At that point, one might say something like this instead:

> different ontological pictures are often useful in different contexts of scientific work, especially when modeling the behaviors of systems of scientific interest under different conditions or with different explanatory goals in mind. With a helping of pragmatism regarding descriptions of objects, events, processes, and properties that fail to cross an agent's threshold for belief, these descriptions may be useful all the same. (162–163)

This is surely compatible with the pragmatic deflationism Lewis recommends, so where, then, do we disagree?

I believe the disagreement here may have nothing to do with the openness of my view to deflationism after all, but rather concerns the question of what *motivates* such an attitude in the first place. Among the motivations one might have to think about ontology pragmatically, I take one very important factor to be the degrees of metaphysical inference one accepts as genuinely amenable to a non-deflationary attitude—if any. Conversely, Lewis thinks that in the case of subatomic particles, a deflationary approach is motivated by the science itself. But this, as I will now suggest, is not the case in any very direct way: the Standard Model may be *interpreted* in such a way, but it does not provide a motivation for deflationism all by itself.

The heart of Lewis' advocacy of deflationism stems from his contention that physicists themselves do not seem to find the distinctions between the finer-grained ontological characterizations of particles I consider (for aficionados: in terms of purely relational structures, or in terms of intrinsic properties) important. Though these and other descriptions conflict, they seem happy to jump from one to another with no apparent concern. What better motivation for deflationism if not scientific practice itself! But here I fear a conflation has crept in—a conflation of two aspects of ontology: one concerning questions of *what* things there are, and another concerning further questions about the *natures* of these things. This conflation is certainly understandable and generally harmless. Typically, determining what there is requires further consideration of the ways in which things are different from one another, which often amounts to a consideration of their natures; and contemplating the different natures of things immediately yields distinctions among what there is. In the present context, however, conflating these different aspects of ontology threatens to mislead, because believing that something exists, in a non-deflationary way, is entirely compatible with believing that finer-grained descriptions of its nature should be regarded pragmatically.

With this in mind, I have doubts that physics or the practice of physicists furnishes any direct motivation for deflationism about particles. Describing or conceptualizing the nature of something in different ways need not, but certainly may involve believing it to exist in a non-deflationary manner, precisely because an ontological commitment to the existence of something does not by itself entail a commitment to the literal truth of all of the descriptions we employ in connection

with it. Indeed, this is a familiar theme in many contexts of scientific practice where we employ *idealized* descriptions of things we take to exist, all the while knowing that, as idealizations, they are not literally true. On the basis of the descriptions physicists employ, Lewis doubts that they "would find the existence of entities whose kinds are constituted exclusively by extrinsic properties [for example] at all problematic," but here I submit that the facts of scientific practice simply underdetermine what physicists or, for that matter, philosophers may think about the natures of the relevant kinds. Perhaps the description quoted above is literally true. Perhaps it is instead a pragmatically useful device. Which of these interpretations one accepts is a function of philosophical commitments regarding whether non-deflationary ontological knowledge is on the cards, and to what extent, in this case.

Thus, ultimately, I believe that Lewis and I should agree about the promise of pragmatic deflationism as an approach to scientific ontology. Where we diverge is in our reading of what the sciences can tell us about whether this is, in fact, the right approach to take, or whether it should be taken in this particular domain of physics. On my view, the choice to regard certain ontological claims in a deflationary or non-deflationary manner, whether they are coarser-grained claims about what exists ("there goes a lepton") or finer-grained claims about what these things are like ("which, by the way, is simply a node in a relational structure"), is something that is informed by philosophical presuppositions about what we can achieve when engaging in certain kinds of ontological theorizing. To the extent that one is inclined toward a non-deflationary realism, one's judgments about which scientific claims amount to knowledge will be informed by one's assessment of the evidence for those claims. If one is not so inclined to any extent, or if one reaches a point at which the inferences to such claims are, by one's own lights, too weak to support a non-deflationary commitment—a point at which the claims are held to be too epistemically risky—adopting a pragmatic attitude may well be an attractive way to go. In this way, what Lewis identifies as an alternative approach to scientific ontology is, I think, part and parcel of a larger story I want to tell.

Magnus' dilemma: the import of values and non-fundamental scientific ontology

Turning now from the broader, framework question of what general approaches to ontology are compatible with the schema I describe in the book to more specific concerns about possible incompleteness, Magnus worries that the absence of two important aspects of scientific ontology threaten to undermine the account. The first stems from a possible restriction of my discussion to matters of *fundamental* ontology, which would leave ontological questions about everything else untouched, thereby drastically limiting the significance of the view. The second is a lack of engagement with what is often called the argument from inductive risk, or perhaps even more generally, the notion of pragmatic encroachment, both of which suggest that values beyond those traditionally conceived as epistemic values may be constitutively relevant to determinations of belief and knowledge—in this case, regarding ontology—and associated action. Magnus derives from this a dilemma,

and though neither horn is obviously fatal, he notes that going one way or the other will produce very different understandings of scientific ontology. As it happens, I believe that my account is committed to neither horn of Magnus' dilemma, but even so, as I will now elaborate in my own way, he is surely right that interesting consequences await those who would embrace or reject one or the other.

Let me begin with the impression that my treatment of scientific ontology applies only to issues of fundamental metaphysics. Perhaps some of the examples I consider at greatest length give that impression, but many of the examples I invoke along the way—fluids, biological species, etc.—are not usually described as instances of fundamental ontological categories, whatever one's preferred definition of "fundamental." I certainly could not blame anyone for missing the one place where I do, to my own recollection, say anything very explicit about whether the scope of my investigation is restricted to any particular "level" of reality: "I will simply assume, as is common among those who are interested in scientific inquiry in fields other than quantum gravity, that societies, aardvarks, proteins, and electrons are no less real than whatever more fundamental theorizing may reveal, and no less worthy of ontological scrutiny" (139). Now, an assumption is not an argument, but until and unless the mereological nihilists succeed in convincing the rest of us that only the fundamental, whatever that means, is real, a wide-ranging, inclusive attitude toward many possible scales of ontology is, to my mind, the only attitude that does justice to the wide-ranging forms of inquiry we find exemplified across the sciences. I intend what I say to apply across the board, and thus we evade the first horn of Magnus' dilemma.

Evading one horn of a dilemma is supposed to result in a skewering by the second, but I admit to not feeling the point of the latter one here. Unlike the case of the first horn, this insensitivity on my part is not due to a denial of Magnus' characterization of my view: he is right that I do not engage with the argument from inductive risk; neither do I engage with the possibility of pragmatic encroachment, which he does not mention, but which I raise now because I take its potential import in this context to be much the same. The upshot of the argument from inductive risk is that given the ampliative and hence uncertain nature of inductive inference, values that are traditionally considered non-epistemic, such as ethical and social values, should play a role in belief formation with respect to how we set, characterize, and assess standards of evidence and interpret data, for example, in contexts where having false beliefs would be ethically, socially, or otherwise detrimental. Pragmatic encroachment is the thesis that pragmatic issues may determine whether or not one should hold a belief in a given circumstance, or whether such a belief would count as knowledge. Pragmatic factors, much like ethical and social values, are not traditionally held to be epistemically relevant in any context. Though I hint at this constellation of issues obliquely at one point in passing (249–250), it is entirely fair to say that I do not engage with it in the book.

It would be a mistake, though, to conclude from this that my account of scientific ontology is married to a traditional conception of the separation of epistemic values from certain other values, as Magnus suggests. My use of the term "epistemic values" is neutral: it is compatible with traditional views according to which virtues such as consistency, coherence, and unification are epistemically relevant while

others such as social or pragmatic virtues are not; it is likewise compatible with views according to which the latter are, in fact, constitutively relevant to belief and knowledge, in which case these latter virtues will qualify as epistemic as well. Indeed, my conception of values that play a role in scientific ontology extends beyond these kinds of "virtues" to more general considerations of the sorts of answers and explanations in contexts of ontological theorizing that individuals find valuable, or not, as the case may be. It is central to my view that values of various kinds play a role in determining where, for any given person, lines are drawn between domains of ontological theorizing in which belief is appropriate and domains in which one should suspend belief instead. A determination of whether the argument from inductive risk or the possibility of pragmatic encroachment rules in certain factors that more traditional accounts of epistemic virtues would exclude is simply not the sort of thing my view of scientific ontology is intended to furnish. It is amenable to being spelled out in different ways.

Having said this, I should admit to being puzzled about why Magnus takes the issues he raises to comprise a *dilemma*. The preamble here is that in order to avoid the first horn—the restriction of my discussion to issues of fundamental ontology, which would greatly circumscribe its relevance—scientific ontology must be extended to include non-fundamental theorizing as well. Then, having broadened one's ontological horizons, one must accept the morals of the argument from inductive risk. As I have explained, neither broad ontological horizons nor taking an expansive position regarding the scope of epistemic virtues is antithetical to my view, but a puzzlement remains. The argument for a dilemma here seems to require a suppressed premise to the effect that non-fundamental ontological theorizing, unlike fundamental theorizing, entails the morals of the argument from inductive risk. Let us grant this for the sake of argument. What concern follows? If ethical, social, or pragmatic factors are epistemic, then the voluntarism I espouse regarding the adoption of epistemic stances will apply to ethical, social, and pragmatic values as well as others. Perhaps the worry here is that this may be too much to swallow, given that my voluntarism about rational stances entails that they are not, ultimately, vulnerable to non-question-begging critique.

Of course, one should never swallow more than what is appropriate, but it is important to appreciate that satisfying the constraint of rationality when adopting an epistemic stance is hardly a trivial matter. The question of whether any particular stance succeeds in satisfying this constraint is hardly insulated from examination, evaluation, and debate. Granted, according to the voluntarism about stances I defend, the relevant conception of rationality is minimal, requiring only that stances harbor no logical inconsistency or probabilistic incoherence, and that they be pragmatically coherent by the lights of those who adopt them. An elaboration and defense of these criteria for the rationality of epistemic stances is one of the central undertakings of the book, and I will not attempt to distill all of this here. For present purposes let it suffice to say that *if* this undertaking is successful, and *if* one is convinced that ethical, social, and pragmatic values are constitutively relevant to justified belief or knowledge, then one should indeed be prepared to swallow the conclusion of the previous paragraph. Only the first of these ifs, however, is essential to my account of scientific ontology.

Strevens' explanations: telling arguments and the unbearable lightness of stances

From worries about possible incompleteness and potentially surprising consequences, let us turn now to Strevens' arguments to the effect that the news is substantially worse than you think. There are two major complaints, here. The first is that the way I describe how ontological judgments are made, and, more specifically, the account I give of criteria relevant to these judgments, are flawed in ways that undermine the central message of the book. Secondly, my contention that some disagreements about ontology are ultimately unassailable is not only defeatist but implausible. Regarding the first of these charges, I certainly agree that there is more to say about relationships between criteria relevant to theorizing about ontology, and Strevens points us in some promising directions. That said, I do not think that any of this promise undermines the view he targets. The second charge is symptomatic, I believe, of a lack of "depth" of consideration of the ultimate wellsprings of ontological and no doubt other philosophical judgments. Let me explicate these thoughts in turn.

In the book I label the principal factors relevant to deciding ontological commitment "empirical vulnerability" and "explanatory power." I contend that assessments of these factors determine the "epistemic risk" we associate with ontological claims, where risk is here understood as a feature of inferences and propositions, the appraisal of which determines how confident we are in judging their truth or falsity. Claims judged epistemically riskier are ones in which confidence is concomitantly lower; conversely, the lower the risk, the higher our confidence. Thus understood, measures of epistemic risk are inversely proportional to empirical vulnerability, which is a matter of susceptibility to empirical testing. Risk is also, arguably, inversely proportional to explanatory power, which is a matter of how well a claim satisfies criteria commonly associated with good explanations of the data of scientific investigation. Strevens prefers the term "testability" to "empirical vulnerability," but I will stick with my own terminology due to a nagging feeling that the suggested replacement is not discriminating enough: those who are especially impressed by explanatory power may regard manifesting impressive degrees of it as a test in itself, one that a theory or hypothesis might pass or fail. Terminology aside, Strevens' substantive worry is that I treat empirical vulnerability and explanatory power as though they are separable. In fact, he maintains, they are inextricable.

Surely it is correct to say that when we are putatively able to observe or otherwise detect some target of scientific investigation, explanations of how we are able to do this, linking the putative target to our data via the intermediaries of our senses and scientific instruments, are intimately connected to whatever confidence we have that the detection has been successful. Strevens explores a number of ways in which detection and explanation are thus connected—for example, if an ontological posit does no explanatory work, or a great deal of explanatory work, this will be factored into assessments of what has been detected. Let me indulge a quick digression: the fact that the connection between detection and explanation is uncontroversial gives

the lie to Magnus' suggestion, which I did not engage above, that detecting at least some unobservable entities "relies only on a simple projective inference and not on explanatory considerations," the idea being that once we are satisfied that a given form of detection is veridical, we may simply use this information to leverage incrementally more powerful detections and then iterate the process. The idea that this leveraging does not involve explanatory considerations is, I think, problematic, but I will not argue the case here. Let us instead return to our present focus: Strevens' observations about connections between empirical vulnerability and explanatory power.

Interestingly, the illustrations that Strevens supplies are ones in which there is theory change: cases in which something posited previously in connection with a now rejected theory has been replaced by something else. In such cases, he says, the former posits have no explanatory power. With the benefit of hindsight one might come to such a conclusion, but at any given time, with only our best theories and models in play, assessments of the explanatory power of current ontological posits are all that matter, so let us fix our attention here. Does the close connection between detection and explanation in determining whether a given empirical test has been successful, and to what degree, undermine my account of the principal factors relevant to ontological commitment? It is difficult to see why it should, not least because while there are clearly ways in which these factors may come together, not all of the explanatory virtues of a posit may be tied to putative detections. Indeed, there are many cases—not surprisingly, the most controversial ones—in which detection and explanation simply come apart, because putative detections are highly indirect, or impracticable, or impossible. In these cases, "explanatory power can serve as a massive counterweight to a lack of empirical vulnerability in dissolving epistemic risk" (88).

For all his attention to the nuances of explanation, I suspect that a vaguer unease with the overarching moral of the book is what fuels Strevens' strongest comments. My account of scientific ontology is one according to which, underlying different assessments of the presence and degrees of empirical vulnerability and explanatory power, and thus epistemic risk, there are stances comprising collections of attitudes and values that lead different agents to draw lines between belief and suspension of belief in different ways. These underlying commitments, so long as they satisfy certain constraints of rationality, are indefeasible. Strevens doubts this, but the grounds of his skepticism are, I think, unconvincing. For instance, he observes that some arguments about ontology are "full-dress philosophical arguments," like David Hume's against necessary connections, and contends that these can be assessed quite independently of stances. This, I submit, fails to penetrate the surface of the dialectic between Hume and his critics. Hume's psychological premises are naïve; is there no less naïve argument for the same conclusion in the vicinity? Bas van Fraassen's claim about explanation not being essential to science is not empirically refutable, as Strevens suggests, for it is not a claim about the surface appearances of scientific practice; it is part of a deeper proposal for understanding the aim of science. Stances underlie and often survive changes in specific arguments for conclusions that are in keeping with them.

That Strevens' skepticism rests on the surface is exemplified in some of his comments about explanation. When he claims that "multivalence"—explaining things in different ways—boosts the explanatory power of an ontological posit, I heartily agree. When he suggests that this requires highly determinate characterizations of the properties of posited entities, such that if they were to be replaced in future theorizing, the same properties would have to be attributed to their successors, I stand and applaud! I have defended this idea myself in elaborating a proposal for scientific realism, which I call "semirealism" (Chakravartty 2007). Strevens and I share an underlying stance, but it is a mistake to think that ours is the only rational option, precluding the idea that different agents may have different but nonetheless rational takes on the ontological import of what we view as the explanatory power of corroborating multivalence. Thus, I am unmoved by his rejection of the idea that disputes about ontology ultimately turn on different assessments of and tolerances for epistemic risk.

Strevens wonders, for *reductio*: "Was van Fraassen a more timid soul than David Lewis? Were the logical positivists wimps?" One may as well ask whether rock climbing is braver than riding trains. These questions are irrelevant to the issues before us. A constructive empiricist who judges the benefit of beliefs that go beyond the observable as illusory is hardly displaying timidity. The logical positivists struggled mightily, not wimpishly, to forge a semantic theory that would safeguard the sciences as the exemplary form of knowledge production, placing them on the firmest possible footing of observation and experience. Assessments of epistemic risk do not stem from courage or cowardice; they stem from what one values. Strevens characterizes stance voluntarism as defeatist, offering instead as engines of belief "the institutional pressure to diversify intellectually, along with the natural human predilection to believe whatever you are currently saying." One need not deny the presence of comparatively superficial pressures and predilections to quell the charge of defeatism: epistemic stances are by no means immune to scrutiny and critique, because their rationality cannot be taken for granted. Across our breadth, we believe and suspend belief regarding ontological claims in different ways. If "institutional pressures" are motivational, they merely reinforce our natural calling as philosophers to seek newer and better arguments for beliefs in keeping with stances that resonate within.

Reference

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