Explanatory Pluralism and Philosophy of Language: Explications and Concepts

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Take *explanatory pluralism* to be the view that we cannot give a unified account of explanation as such, on the grounds that explanations come in many different varieties.[1] Pluralists often associate different varieties of explanation with different domains. Sometimes these domains are conceived of as divisions within science (sociology, psychology, biology, physics, etc.), and sometimes they are conceived of as divisions separating science from other areas of inquiry (mathematics, ethics, etc.).[2]

Explanatory monism is then roughly the view that all explanations are substantively similar enough that we can give an adequate general account of explanation as such. Explanatory monism might well seem unwarranted. For every monistic or general theory of explanation in the literature, there are problem cases. Sometimes these problem cases amount to decisive counterexamples, as the example of the flagpole and its shadow is for Hempel's deductive-nomological theory. But though such cases might decisively refute one or another monistic theory, they do not refute monism altogether:

[1] Recent books defending explanatory pluralism include Weber et al. (2013) and Mantzavinos (2016).

[2] The books mentioned in fn. 1 primarily conceive of pluralism in the first way. Díez et al. (2013) invoke the latter taxonomy, and both Austin (1998) and the distinction between scientific and intuitive explanations in Shtulman and Lombrozo (ming) suggest a similar conceptualization of kinds of explanation.

maybe we just haven't yet found the right monistic theory of explanation. Successive theories certainly have improved insofar as they have accounted for the counterexamples that defeated earlier theories without introducing new weaknesses. But the fact remains that even the best and most general monistic theories in the literature face difficulties in being extended from familiar toy cases to explanations in the special sciences.[3]

In this paper, I want to begin by focusing on the criticisms of monistic theories of explanation presented in Weber et al. (2013), which advances what it calls the "pragmatic approach to scientific explanation," though I will call it *Ghentian explanatory pluralism*.[4] The Ghentians' case does not rest merely on the whack-a-mole game of producing counterexamples to monistic theories. It proceeds, rather, by exploiting a particular understanding of explications to attribute ambitious descriptive and normative claims to monistic theorists, for which claims those theorists nowhere argue. Weber et al. (2013) are correct that the monistic theorists nowhere argue for the claims in question, for the simple reason that they are not committed to them. One of two main goals in this paper is to undermine Weber et al.'s pluralist criticisms of monistic theories by getting clear on this point.

The other main goal is to defend my own approach against a certain kind of pluralist objection. I focus on concepts rather than explications, and I hold a version of the monistic thesis. Namely, I hold that what all explanations have in common, across scientific domains and even throughout non-scientific areas of inquiry, is that they all employ the same explanatory terminology to express the same general explanatory concepts.[5] One

[3] As a case in point, see the discussion in Weatherson (2012) of the difficulties attending the extension of the kairetic model of Strevens (2008) to equilibrium explanations in economics.

[4] For an explanation of the label, see §4.9 of Weber et al. (2013), the final section of the book, which ends by explicitly recommending work on explanation done at Ghent and cites, as examples, ten papers written by various combinations of seven different Ghent-affiliated authors.

[5] It would be a little bit disingenuous, outside of the context of this debate, to call

common pluralist objection to this thesis is that, insofar as it can be true, explanatory concepts are entirely empty.[6] Thus Díez et al. (2013) argue against Nickel (2010) that there are "no substantive and context-invariant constraints on explanatory information" (379). Díez et al. (2013) press their case by arguing that not even asymmetry is required by the semantics of 'because'. I push back against this claim, in defense of my approach to explanation, by showing how their arguments fail to stand up to linguistic scrutiny.[7]

Before turning to a brief discussion of explication, one important qualification should be mentioned. This paper does not attempt to show that there is no way of thinking about explanation on which the pluralist project deserves to be pursued. I am skeptical whether it can be done in a way that establishes a pluralist thesis that actually conflicts with explanatory monism, but even if it can be done, it is a job for pluralists to do themselves.[8] All I want to do here is show that the criticisms of monistic theories put forward in the Ghentian explanatory pluralist literature fail, and show how there is hope for a productive, monistic conceptual analysis.

1 Explications and Concepts

Weber et al. (2013) introduce explications by quoting the following passage from Carnap:

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my view monistic, insofar as I think that we actually have two sets of explanatory concepts: narrative or causal concepts, on the one hand, and what I claim are metaphorically causal concepts, on the other, which we use to express (among other things) metaphysical explanations. See Shaheen (2017b, 201X).

[6] Cf. the common allegation, going back at least to Kitcher and Salmon (1987), that the account of explanations as answers to 'why' questions given by van Fraassen (1980) is an "anything goes" account of explanation.

[7] For a paper devoted entirely to the asymmetry of 'because', see Schnieder (2015).

[8] See Bokulich (2016a,b) for a representational conception of explanation that is supposed to support the pluralist project.

> If a concept is given as explicandum, the task [of explication] consists in finding another concept as its explicatum which fulfills the following requirements to a sufficient degree.
>
> 1. The explicatum is to be *similar to the explicandum* in such a way that, in most cases in which the explicandum has so far been used, the explicatum can be used; however, close similarity is not required, and considerable differences are permitted.
> 2. The characterization of the explicatum, that is, the rules of its use (for instance, in the form of a definition), is to be given in an *exact* form, so as to introduce the explicatum into a well-connected system of scientific concepts.
> 3. The explicatum is to be a *fruitful* concept, that is, useful for the formulation of many universal statements (empirical laws in the case of a nonlogical concept, logical theorems in the case of a logical concept).
> 4. The explicatum should be a *simple* as possible; this means as simple as the more important requirements (1), (2), and (3) permit.
>
> (Carnap (1950), 7)

Before addressing what Weber et al. (2013) make of this passage, a couple of comments are in order, one to set up a rejoinder to Weber et al. (2013), and one to set up my own approach to explanation. So, first, as the first two criteria for explicanda suggest, explicanda are more precise than explicata: explications involve replacing explicanda with something more precise. So an explicatum is a *precisification* of its explicandum, which only needs to be similar to it in a certain respect. Producing an explication thus requires balancing similarity to the explicandum against other criteria. David Lewis,

in his reply to the incredulous stare objection to modal realism, comments on this kind of balancing constraint on philosophical theorizing, as follows:

> In trying to improve the unity and economy of our total theory by providing resources that will afford analyses, for instance of modality as quantification over worlds, I am trying to accomplish two things that somewhat conflict. I am trying to *improve* that theory, that is to change it. But I am trying to improve *that* theory, that is to leave it recognisably the same theory we had before. (Lewis (1986), 134)

So in general we should expect an explication of explanation to depart from our ordinary concept of explanation in perhaps significant ways, so long as it is close enough to the target explicandum and satisfies the remaining criteria well enough.

Second, turning specifically to the exactness criterion, note that exact and non-wildly-disjunctive explicata of any ordinary explicandum are more or less certain to differ from the explicandum. Tarski recognized a parallel point very early on, with respect to his account of 'true':

> I clearly realize (as I already indicated) that the common meaning of the word "*true*"—as that of any other word of everyday language—is to some extent vague, and that its usage more or less fluctuates. (Tarski (1944), 359-360)

While I take Tarski to be right that words in natural languages are generally "to some extent vague," the last clause here betrays a picture of meaning that I reject. I take concepts to be expressed by words, i.e., I take concepts to be word-meanings. To individuate concepts, we have to individuate word-meanings. But there is no guarantee that word-meanings will be precise enough to qualify as explicata in the Carnapian sense, even if (as the phrase "finding another concept" in Carnap's definition suggests) we have or can coin words with meanings precise enough to be explicata. Here I depart

from Tarski, who apparently holds that concepts or word-meanings cannot be imprecise.

> [Disputes over which meaning is the "right" one] occur in all domains where—instead of an exact, scientific terminology—common language with its vagueness and ambiguity is used; and they are always meaningless, and therefore in vain.
>
> It seems to me obvious that the only rational approach to such problems would be the following: We should reconcile ourselves with the fact that we are confronted, not with one concept, but with several different concepts which are denoted by one word; we should try to make these concepts as clear as possible (by means of definition, or of an axiomatic procedure, or in some other way); to avoid further confusions, we should agree to use different terms for different concepts; and then we may proceed to a quiet and systematic study of all concepts involved, which will exhibit their main properties and mutual relations. (Tarski (1944), 355)

Tarski here writes as if there is a collection of precise concepts, between which the word 'true' is ambiguous. He thereby runs together conceptions, i.e., theories, which may well be expected to be precise, on the one hand, and concepts, or word-meanings, on the other.[9] This conflation is clearly visible in Tarski's slipping from 'concept' talk to 'conception' talk:

> The word "true," like other words from our everyday language, is certainly not unambiguous. And it does not seem to me that the philosophers who have discussed this concept have helped

[9] Gallow (2014), §1.3 also distinguishes between conceptions and concepts along something like these lines. He introduces a conception of concepts as a certain kind of mental representation, rather than simply as word-meanings, whatever those are, but it is not entirely implausible to think that word-meanings might turn out to be the kinds of mental representations Gallow has in mind.

> to diminish its ambiguity. In works and discussions of philosophers we meet many different conceptions of truth and falsity, and we must indicate which conception will be the basis of our discussion. (Tarski (1944), 342)

On my view, concepts may be and often are imprecise, so the conceptions to which Tarski here refers may well be precisifications of the ordinary, imprecise concept expressed by 'true'. But not every such precisification is a disambiguation.[10] In the particular case of explications of explanation, then, it will be no surprise if potential explicata differ from the explicandum in being more precise than the general concept. Because of this difference in precision, moreover, there will be many explications that might be offered for a given concept, because an imprecise concept may be precisified in different ways. But the existence of many candidate precisifications of explanation will not be, by itself, evidence against explanatory monism.

These comments on the record, we can turn to Weber et al. (2013)'s interest in explications.

2 Fruitfulness and the Empirical Criticism

Weber et al. (2013) quote Carnap with revisionary purposes. The main revision they propose is to Carnap's notion of *fruitfulness*. For Carnap, the fruitfulness of an explication of a logical or nonlogical concept is a matter of its utility in formulating logical theorems or empirical laws, respectively. Explanation is a nonlogical concept, so Carnap would have us aim for an explication that allows us to formulate empirical laws. But Weber et al. (2013) replace the distinction between logical and nonlogical concepts by a distinction between "scientific explications" (including explications of nonlogical concepts like Carnap's examples of *fish* and *warmer*) and "mathematical explications" (including explications of numbers) (26). The distinction be-

[10] Cf. Godfrey-Smith (2009), 330 and Shaheen (2017b), §2.

tween the scientific and the mathematical is of course not exhaustive in the way that Carnap's distinction between the logical and the nonlogical is. So Weber et al. have room, which they take, to distinguish "philosophical explications" from both of these. They then suggest that the fruitfulness of philosophical explications might lie "in that they allow us to offer clear guidelines for scientists (i.e. we formulate norms with them, not empirical generalisations or logical theorems)" (27).

The kinds of norms they have in mind are revealed by their criticisms of the accounts of explanation represented by Hempel (1965a), Kitcher (1989), and Salmon (1984a). Without loss of generality, I will discuss their remarks about Hempel (1965a). Weber et al. (2013) read Hempel as committed to the following two claims:

(1) All scientists who have understanding as an aim really seek DN [i.e., Deductive Nomological] or IS [i.e., Inductive Statistical] explanations, so that the phenomenon they want to understand becomes expectable (28).

(2) All scientists that are engaged in understanding the world should construct DN or IS explanations (and not necessarily something more specific, such as DN explanations citing causes) (29).

But there is, according to Weber et al., a rub. Hempel does not argue for these claims. Rather than taking the absence of any argument for these claims as a sign that Hempel is not committed to them, Weber et al. read Hempel as failing to establish them for not having tried. Hempel should have tried to establish his descriptive claim, Weber et al. suggest, "by systematically investigating the opinions of a large representative sample of scientists (this could be done by interviewing them or analysing their writings)" (29). That is, Hempel should have engaged in a certain empirical project of trying to find out what sorts of things are sought by scientists who have understanding as an aim, by investigating empirically what scientists

regard as explanations and what are presented as explanations in scientific work. Call this *the empirical criticism*.

As their gloss of fruitfulness for philosophical explications suggests, Weber et al. are more interested in the normative claims they attribute to monistic theorists like Hempel than they are in the descriptive claim. Granting for the moment—a temporary concession soon to be revoked—that Hempel is committed to (1), (2) certainly doesn't follow from it. Even if all scientists want to contribute to the discovery of, say, DN explanations, it doesn't follow that all of them should spend their time constructing them. They might rather better contribute to their joint project by designing experiments and gathering raw data. Hempel doesn't, as a matter of fact, say anything—at least, not in the six block quotes presented by Weber et al.—about what scientists should do. This should be no surprise, since the idea that the fruitfulness of a class of philosophical explications should consist in its allowing us to offer guidelines to scientists is, for present purposes, an innovation of Ghentian explanatory pluralism.

Forget (2) and return to the question of Hempel's commitment to (1). The idea that philosophers, insofar as they are giving explications of scientific explanation, are committed to claims that could be empirically investigated by interviewing scientists or examining the writings of scientists does not stand up to scrutiny. For one thing, in passages that Weber et al. quote, Hempel repeatedly explicitly says that he is not attempting to describe actual scientific practice.[11] So it is unclear to what extent the kind of empirical investigations they recommend would be needed to vindicate whatever descriptive claims Hempel really made. For another, recall that, on Carnap's definition of an explication, on which Weber et al. largely rely,

[11] Hempel writes that "these models are not meant to describe how working scientists actually formulate their explanatory accounts" (Hempel (1965a), 412, qtd. at Weber et al. (2013), 28). "This construal," he later adds, "does not claim simply to be descriptive of the explanations actually offered in empirical science" (Hempel (1965a), 488-489, qtd. at Weber et al. (2013), 27).

"close similarity" between explicandum and explicatum is not even required. It might be enough if some central cases clearly fit the DN model well, and indeed monistic theorists of explanation typically elaborate their accounts with reference to such examples. Hempel, again in a passage that Weber et al. quote, says he is giving "an *explication*, which is intended to replace a familiar but vague and ambiguous notion by a more precisely characterized and systematically fruitful and illuminating one" (489, qtd. at 27). The pluralists offer us no reason to think that scientists are particularly well-positioned to evaluate explications—precise replacements for ordinary concepts meeting a number of technical criteria—such that we ought to interview them to find out what they think explanations are. This point seems especially important given that Ghentian explanatory pluralism proceeds by investigating explanation within the context of one or another particular sharply limited domains, provided we remember Wittgenstein's remark on the etiology of confusion:

> A main cause of philosophical disease – a one-sided diet: one nourishes one's thinking with only one kind of example. (Wittgenstein (2001), §539)

Pluralists might attempt to diagnose monists with such a disease—after all, the history of monistic analyses is the history of finding previously unconsidered counterexamples to them—except that Ghentian explanatory pluralism itself embraces the symptoms, by limiting analyses to "context-dependent" descriptive and normative claims, which turns out to mean claims restricted in scope to clusters of contexts individuated on the basis of being part of "a certain discipline or research tradition" (Weber et al. (2013), 33-35).

At any rate, given Hempel's explicit denial that he is attempting to describe actual practice, the only way that we construe the descriptive claim (1) such that Hempel might believe it is to understand 'really seek' as creating an opaque context such that scientists can be unaware of what it is they are really seeking. For scientists might know they are trying to understand

something, and that some methods they have learned to pursue are usually effective for understanding things, without ever having thought (correctly or otherwise) about what form an explanation should take. To illustrate this point by way of contrast, I want to consider a case where a failure to have interviewed non-philosophers to gather their views about a concept makes sense, and then think about the differences between that case and the case of explanation.

2.1 A Disanalogous Analogy: Happiness and Understanding

In this section I consider something of a disanalogous case to explanation. I consider this case because Weber et al. are committed to thinking that it is analogous to the case of explanation, and it will be useful to put this on the table. Suppose some intrepid explicator named Tolstoy*, inspired by the opening line of *Anna Karenina*—"Happy families are all alike; every unhappy family is unhappy in its own way."—and proceeding in the way Weber et al. suggest, offers the following descriptive and normative claims about happiness:

(3) Everyone who has happiness as an aim really seeks a stable job, a marriage, two children, and a dog.

(4) Everyone who is engaged in being happy should find a stable job, marry someone, have two children, and get a dog (and not necessarily something more specific, like a borzoi).

Both claims are obviously false, but that's beside the point. The point here is that Tolstoy* really would be making a mistake if he committed to (3) without conducting any kind of empirical investigation of what pursuers of happiness take themselves to be doing or any systematic investigation of what the actual pursuit of happiness looks like.

Tolstoy* would be making a mistake in so doing because people have some insight into their own happiness. Whatever that level of insight is, the

existence of people who are sure they would not be made happy by a stable job, marriage, two children, and a dog is a serious challenge to (3). An even stronger observation is warranted. If it turns out that the existence of people who are sure they would not be made happy by a stable job, marriage, two children, and a dog does not suffice to establish the falsity of (3)—presumably because of some funny business about the semantics of 'having X as an aim' or 'really seeks'—then there is just no point in Tolstoy*'s interviewing people about what they take their happiness to consist in or depend on. The relevant happiness-related facts would be epistemically inaccessible to them.

There would be similarly little point in investigating what the actual pursuit of happiness looks like. Whether or not the Humean theory of motivation is ultimately correct, people do something close enough to acting on the basis of their beliefs that, if their beliefs about happiness are totally misguided, so too will their pursuit of happiness be. So, insofar as Tolstoy* asserts (3) without having done empirical work of the kind envisioned by Weber et al., a parallel version of their empirical criticism applies to him justly. Whatever concept he is explicating, in the course of which he commits to (3) and its normative partner, he has not done the work he ought to have done.

But understanding and happiness are different in ways that suggest that Weber et al.'s empirical criticism does not justly apply to Hempel or other monistic theorists of explanation. For example, we plausibly have less insight into whether we understand something than we have into whether we are happy. Understanding may be mind-dependent, but that doesn't mean it's epistemically accessible in the same way as happiness.[12]

What Hempel and other theorists of explanation give us, insofar as they are committed to the link between understanding and explanation suggested by (1), is a story about what it is that scientists do that actually yields

[12]On the psychology of scientific understanding, see Trout (2002, 2007).

understanding. Consider Michael Strevens's account of the link between explanation and understanding: he holds that to understand something is to have an explanation why it is the case.

> I take scientific understanding to be that state produced, and only produced, by grasping a true explanation. (Strevens (2008), 3)[13]

Monistic theorists, in giving explications of explanation, are hoping to say what it is that scientists produce that produces understanding in turn, whether this thing they produce is their conscious goal, or utterly misrepresented in their writings, or entirely off their radar. Here the disanalogy between our epistemic access to our own understanding and our epistemic access to our own happiness rears its head. Salmon (1984b), elucidating his ontic conception of explanation, focuses on "mechanisms that actually operate" (299). Strevens (2008) conceives of explanation as "something out in the world, a set of facts to be discovered" (6). The question of monism, for these authors, is the question whether those mechanisms, or those sets of facts, or whatever share a common explanatory nature, or structure, or whatever. But there is no reason to think that interviewing scientists or systematically perusing their writings is the best way to get closer to an answer to that question. So the empirical criticism of the Hempelian and other monistic theories of explanation is no real criticism of monism at all.

3 Conceptual Monism, Sort Of

The two comments about explications emphasized in §1 were that explicata may differ from explananda, and that the existence of multiple candidate explicata is not evidence the explicandum's being unified, if imprecise.

[13]Strevens (2013) presents further, slightly altered details and clears up some potential areas for confusion, but for present purposes the differences between the quoted passage and Strevens (2013) and the details available in the latter paper are irrelevant.

Whereas disambiguations have to do with distinct meanings of a word, precisifications can be more finely grained and more exact than any of the meanings of the word that expresses the concept that they make more precise. In the case at hand, it turns out that none of the Ghentian explanatory pluralists' precisifications of *explanation* correspond to distinct meanings of 'explanation'.[14] But to give a monistic account of explanation, we do need to be able to say something informative about the content of our explanatory talk that holds across domains. Díez et al. (2013), responding to Nickel (2010), argue that there turns out to be nothing substantive to say about this.[15] In particular, they join Nickel in treating the question as relating to the semantics of 'because', but argue against Nickel that 'because' does not even enforce so weak a condition as explanatory asymmetry. Now, on my view, the asymmetry of 'because' derives from its expressing explanatory relations that either are themselves to be identified with or are backed by the asymmetric relations of causation and metaphysical priority or grounding.[16] But Díez et al. train their fire on this very weak constraint on the semantics of 'because'. It is important to see that their argument fails in order to see that Nickel and others are right about the asymmetry of 'because', and so that there are at least some context-invariant constraints on explanations.

Nickel (2010) argues for a modest form of explanatory monism that I follow Díez et al. (2013) in calling Generalism.

> Generalism There are *substantive* and *domain-invariant* constraints on explanatory information.

Díez et al. offer a series of arguments that purport to defeat Nickel's argument for Generalism. They train their fire on a very weak constraint on the semantics of 'because': that 'because' is asymmetric.

[14] See Shaheen (2017b), §2, which makes the point about distinguishing precisifications from disambiguations with reference to Weber et al. (2005).

[15] In anticipation of a knowledgeable reader's skeptical eyebrow, I admit that only one of Díez et al. is literally a Ghentian, but their arguments are relevant to the larger debate.

[16] See also Schnieder (2015), §3c.

In particular, Díez et al. give two arguments that they think show that 'because' is not asymmetric. The first argument concerns temporal asymmetry.

(5) The moon appears there because it was at location l earlier.[17]

(6) The moon appears there because it will be at location l' later.[18]

(7) It is not the case that the moon appears there because it will be at location l' later.[19]

(8) It is not the case that the moon appears there because it was at location l earlier.[20]

Nickel alleges and Díez et al. grant that (5) and (6) differ in acceptability: only the former is acceptable. Nickel argues that the difference in acceptability is due to a difference in truth value. To establish this, he points out that (7) is "completely acceptable," and therefore true, and so its negand (6) must be false (Nickel (2010), 311). But Díez et al. argue that (8) is also completely acceptable, at least in a certain context. In particular, in the context of a comparison of "genuinely explanatory" Newtonian dynamics and "merely descriptive" Keplerian celestial kinematics, (8) is completely acceptable, and therefore true, and so its negand ((8)) must be false as well (385). If they are right, then there is, contra Nickel (2010), no difference in truth value between (5) and (6).

The problem with the Díez et al. argument here is that they illicitly shift the context. (5) isn't acceptable in the context of a comparison of the Newtonian and Keplerian theories. So the challenge of explaining the difference in acceptability between (5) and (6) cannot be profitably investigated by considering such contexts. (Note also that there is a reading of (8) on

[17] Nickel (2010)'s (1a).
[18] Nickel (2010)'s (1b).
[19] Nickel (2010)'s (2).
[20] Díez et al. (2013)'s (2^*).

which the negation is metalinguistic, in which case the acceptability of (8) would fail to establish that (5) is false even in that context.)

The second argument Díez et al. offer against the asymmetry of 'because' concerns equations like the ideal gas law $PV = NkT$. They observe that one can say things like (9) and (10), and suggest that this involves a violation of asymmetry.

(9) The pressure is such-and-so because the volume and temperature are so-and-so. (386)

(10) The temperature is such-and-so because the pressure and volume are so-and-so. (386)

Examples like this do threaten to provide materials for counterexamples to asymmetry, though they need to be cleaned up a bit in two ways. First, the mentions of volume in (9) and (10) might seem to undermine the claim that those sentences are counterexamples to asymmetry, but we can imagine a context in which the volume of a container is known, and just explain its pressure and temperature in terms of one another, using sentences like (11) and (12).

(11) The pressure is P because the temperature is T.

(12) The temperature is T because the pressure is P.

Second, the joint assertability of (11) and (12) would look like real counterexamples to the asymmetry of 'because', though its important that the assertability really be joint. It would suffice to find a context in which the conjunction of (11) and (12) is assertable.

(13) The pressure is P, because the temperature is T, and the temperature is T, because the pressure is P.

(13) has a couple more commas than the reader might have expected. But

those commas are important. I submit that the only way to utter (13) acceptably is to use comma intonation before each 'because' clause. That is, I submit that a version of (13) with two fewer commas is never acceptable. This in turn is because (13) isn't an actual counterexample to asymmetry. Rather, what (an acceptable utterance of) (13) means is something like the following: we know that the pressure is P because the temperature is T, and we know that the temperature is T because the pressure is P.[21] That is, we have to read the sentence as making a pair of epistemic 'because' claims, where 'because' is being used as in (14).

(14) He likes her, because he brought her moss.

But that means there is no violation of asymmetry in (13). Our knowing that the pressure is P might well be explained in terms of (our knowing that) the temperature is T, and vice versa, without anything whose logical form is perspicuously captured by something of the form 'A because B and B because A' being true.

If the foregoing analyses of Díez et al. (2013)'s argumentation are correct, then Nickel (2010) is vindicated on the asymmetry of 'because'. There are at least some substantive and context-invariant constraints on the semantics of 'because'. In fact, I think there are rather a lot of such constraints, and I have elaborated on some of them elsewhere. The way to establish Ghentian explanatory pluralism using data about 'because' would be to show that 'because' is actually semantically ambiguous between different domains. But the linguistic data are just not there.[22]

[21] Something like that, but only something like it. My official view is that (13) offers (causal!) explanations of the speaker's having permission to assert that the pressure is P and the speaker's having permission to assert that the temperature is T. See Shaheen (2017a).

[22] For an account of where the joints in the meaning of 'because' actually are, see Shaheen (201X), where I argue for the claim that 'because' is ambiguous between a causal sense and a second sense that gets its content, according to my argument in Shaheen (2017b), via a causal metaphor.

4 Concluding Remark

This paper has been almost entirely negative. It has tried to show that the published arguments for Ghentian explanatory pluralism by Ghentian explanatory pluralists misconstrue the monist project, foisting on explicators like Hempel descriptive and normative claims that they did not hold and need not have held. It has also tried to show that arguments for a certain kind of "anything goes" criticism of monistic theories of explanation pressed by explanatory pluralists rest on illicit context shifts and misconstruals of linguistic facts. There is, much noise to the contrary, nothing in the literature at all that shows that a conceptual analysis of explanation is doomed to either emptiness or the kind of domain pluralism advocated by Weber et al. (2013), Díez et al. (2013), or others. Any impression to the contrary is born, I think, of bad philosophy of language. This is not to say that the positive Ghentian pluralist project itself is without value. But it is to say that its pursuit cannot be justified by the criticisms of the great hits of philosophy of explanation Ghentian pluralists have so far offered.

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