Does experimental philosophy have a role to play in Carnapian explication?

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Abstract. Shepherd and Justus argue that experimental philosophy has an important role to play in the method of Carnapian explication, facilitating the preparatory stage during which the concept to be explicated is clarified. I raise concerns about their specific proposal, before sketching an alternative. In particular, I suggest that experimental philosophy can directly aid the construction of fruitful concepts. This provides a clear practical role for experimental philosophy, both within the sciences and theoretical inquiry more generally. In this respect, experimental philosophy may rightly be construed as one aspect of applied philosophy.

1. Introduction

Joshua Shepherd and James Justus (2015) have recently argued that experimental philosophy can be incorporated, perhaps surprisingly, into Carnap's (1950) method of explication:[1] they propose that experimental philosophy play a clarificatory role in the initial preparatory stage of explication. Shepherd and Justus take themselves to have highlighted "a compelling new positive program for [experimental philosophy]" (2015: 391).

I endorse the authors' search for new, important, positive work for experimental philosophy. And I agree that such work might be found in connection with Carnap's method of explication. But I am unconvinced by Shepherd and Justus' specific proposal: as I argue in §3, there are reasons to doubt that the proposal brings any genuine benefits to the method of explication. A more promising proposal affords experimental philosophy a role at a more important stage of explication, aiding the construction

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[1] Schupbach (forthcoming) argues that experimental philosophy can be incorporated into 'Oppenheimian explication', a method for illuminating concepts. Nothing herein bears upon Schupbach's project.

of *fruitful* concepts. I sketch the proposal in §4. If right, experimental philosophy can play an active role in the development of theoretical conceptual frameworks, directly affecting the shape of future scientific inquiry.

The upshot is that experimental philosophy may count, in one good sense, as *applied* philosophy. It is a genuinely practical concern *how* theorists should engineer their concepts for the purposes of theorising, and the method of explication addresses that practical concern directly, by providing an implementable procedure for undertaking one key aspect of theoretical practice. In this sense, it is natural to think of the method of explication as one aspect of applied philosophy. And thus, insofar as experimental philosophy plays an important role within that method, it too can be construed as applied philosophy.

2. Carnapian explication

Explication, as introduced by Carnap, is a method for replacing terms and concepts with more precise counterparts, in order to facilitate theorising.[2] The imprecise term or concept with which we start, called the *explicandum*, may "belong to everyday language or to a previous stage in the development of scientific language" (Carnap 1950: 3). The precise replacement is called the *explicatum*.

The method begins with an informal clarification of the explicandum (Carnap 1950: 4–5; 1963: 933). Anticipating Shepherd and Justus' terminology, I call this stage *explication preparation*. Such clarification is a "means for reaching a relatively good mutual understanding as to [the explicandum's] intended meaning" and "serves only to make clear what is meant as the explicandum" (1950: 4). The clarification is achieved with informal examples that illustrate how the explicandum is, and is not, to be understood. For example, prior to an explication of SALT, Carnap suggests that one might say: "I mean by the explicandum 'salt', not its wide sense which it has in chemistry but its narrow sense in which it is used in the household language" (1950: 4–5). One might go on to provide the explicatum NACL.

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[2] I will sometimes talk of concepts and at other times talk of terms. In each case, I suppose that an explicandum *qua* term *expresses* the explicandum *qua* concept; and that the explicatum *qua* term expresses the explicatum *qua* concept. Throughout, I use small capitals to denote concepts.

After explication preparation, the theorist should provide the explicatum itself. Carnap provides four requirements that the explicatum should satisfy "to a sufficient degree" (1950: 7). First, the explicatum should be *similar* in relevant respects to the explicandum: we should be able to deploy the explicatum in most situations in which we would previously have deployed the explicandum. Second, the explicatum should be *precise* (or *exact*): exact rules for its use should be given. Third, the explicatum should be *fruitful*: it should feature in relevant laws and generalisations. And, fourth, the explicatum should be *simple*.

Carnap gives the following example (1950: 12–15). The explicandum is WARMER, understood to depend solely on our sensations, and the explicatum is TEMPERATURE, understood as a quantitative concept. The four requirements are satisfied as follows. First, similarity: in most cases in which x is (according to our sensations) warmer than y, the temperature of x is greater than the temperature of y. Second, precision: rules for the use of TEMPERATURE can be given with reference to thermometers. Third, fruitfulness: TEMPERATURE features in (for example) the ideal gas laws. And, fourth, simplicity: both the rules for the use of TEMPERATURE, and the laws in which it features, are simple. In light of such considerations, Carnap takes TEMPERATURE to be "the [explicatum of WARMER] important for science" (1950: 14).

A few comments about the four requirements are in order. First, as noted above, they need only be satisfied to a *sufficient* degree. With respect to similarity, Carnap writes that "close similarity is not required, and considerable differences are permitted" (1950: 7). With respect to precision, it is sufficient that the explicatum be *more* precise than the explicandum. Thus, in *Meaning and Necessity*, Carnap describes the method of explication as "[the] task of making *more* exact a vague or not quite exact concept […], or rather of replacing it by a newly constructed, *more* exact concept" (1947: 7–8, my emphasis). Fruitfulness, of which I will say more presently, is likewise a matter of degree. And, with regard to simplicity, Carnap explicitly subordinates the requirement to the others; the explicatum should be "as simple as the more important requirements permit" (1950: 7).

Second, most commentators, including Shepherd and Justus, take fruitfulness to be the most fundamental requirement. Thus, Shepherd and Justus write that "precision for precision's sake is not the agenda", rather "enhancing precision usually enhances fruitfulness, which is the agenda" (2015:

388). Similarly, Schupbach writes that "Carnap plays favorites with regards to his desiderata, prioritizing fruitfulness over similarity" (forthcoming: 7) and Dutilh Novaes and Reck write that "fruitfulness is ultimately the most significant requirement for an explication overall" (forthcoming: §1.3). The spirit of prioritising fruitfulness is captured in Kitcher's discussion of explication, in which he writes that "[t]here's no higher standard to which our concepts are to answer than the efficient satisfaction of the purposes of inquiry" (2008: 119). And, certainly, insofar as an explicator is principally motivated by theoretical inquiry, it is natural to suppose that the fruitfulness of the explicatum will be her principal aim.

Third, I follow commentators in taking fruitfulness to be *broader* than as explicitly characterised by Carnap. The latter takes an explicatum to be fruitful to the extent that it features in relevant laws and generalisations. However, Dutilh Novaes and Reck write that

> there must be more to fruitfulness than the formulation or derivation of universal statements. […] Carnap's view seems to be that an explication is useful or fruitful when it delivers 'results' that could not be delivered otherwise (or with much more difficulty), i.e. with the explicandum alone. […] The goal is to produce *new knowledge* about the phenomena to which the explicandum pertains. (Forthcoming: §1.5).

Both Kitcher and Shepherd and Justus develop more localised accounts of fruitfulness. Kitcher takes Carnap's account to be "deeply problematic for the biological, earth and human sciences" (2008: 115). He suggests instead that

> we conceive of the aims of the sciences in terms of the provision of answers to significant questions, where the sources of significance are various, sometimes practical, sometimes in terms of the satisfaction of disinterested curiosity. (2008: 115)

And Shepherd and Justus claim that Carnap's account of fruitfulness is not appropriate for epistemic concepts. They suggest that one way that an explicatum for an epistemic concept might be fruitful is by improving our ability to reason: "explications of epistemic concepts should consider how they might cohere with and ideally improve the statistical methods that deliver well-supported beliefs in the sciences" (2015: 398).

As these quotations indicate, what constitutes fruitfulness is to a certain extent up for grabs. I will make use of this flexibility in §4. For now, however, I simply note that there may be a variety of general and perhaps subject-specific ways in which a concept may be fruitful. Whatever the details, fruitfulness is likely to be broader than merely featuring in laws and generalisations.

Once explication preparation has been completed and a suitable explicatum highlighted, then the final stage of explication is to *replace* the explicandum with the explicatum. The idea is *not* to replace the explicandum in *every* possible context. For example, we need not replace explicanda with explicata in ordinary conversational contexts: we do not need to start asking for "NaCl" or "sodium chloride" across the dinner table. Rather, the idea is that, *in the relevant theoretical contexts*, the theorists in question are to use the explicatum in place of the explicandum: chemists (*qua* chemist) should use NACL when they might otherwise have used SALT; physicists (*qua* physicist) should use TEMPERATURE when they might otherwise have used WARMER; and so on.

Before proceeding, note that various philosophical objections have been raised against the method of explication; in particular, Strawson (1963) objected that explication involves a problematic 'change of subject'. As much has already been written in defence of the method I will not respond to such objections here.[3] On the assumption that explication is defensible, I will consider whether experimental philosophy has an important role within that methodology.

3. Experimental explication preparation

Shepherd and Justus (2015) claim that experimental philosophy should be used to *clarify* explicanda during explication preparation, in a process they call *experimental explication preparation*. The idea is that experimental philosophy provides objective methods for clarifying concepts, and thus is well-suited to this preliminary stage of explication.

By way of motivation, they highlight a particular challenge facing any Carnapian explicator: "pinpoint[ing] the content that merits attempted preservation [in the explicatum] and the content that

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[3] See e.g.: Brun forthcoming; Carnap 1963; Carus 2007; Dutilh Novaes and Reck forthcoming; Justus 2012; Kitcher 2008; Maher 2007; Schupbach forthcoming.

should be abandoned" (p. 389). The challenge arises as follows. On the one hand, "being tethered to imprecise explicanda appears to hinder, not advance, the development of fruitful explicata." (*ibid*). In part, this is because explicanda are "problematically vague […] amorphous and imprecise" (p. 388), and "many [candidate explicanda] possess content and encourage implications that would mislead rather than guide explication" (p. 389). Yet, on the other hand, "radical revisionism overlooks how folk concepts often describe features of the world and guide in theorizing about them, albeit rudimentarily" (*ibid*). So, for Shepherd and Justus, the challenge for the explicator is to preserve those aspects of the intuitive content of our concepts that will facilitate future theorising, while discarding the problematic, misleading aspects of those concepts.

They introduce experimental explication preparation to help overcome the challenge.

> To pinpoint the content that merits attempted preservation and the content that should be abandoned […], a method for vetting explicanda is needed. […] With its insistence on using scientific methods to analyse empirical sources of information about concepts […], x-phi has an especially important role to play in *explication preparation* […]. Explicandum clarification, for example, is best achieved through empirically rigorous studies of the kind experimental philosophers conduct […]. (2015: 389–390)

Experimental philosophy, then, can play a role in explication preparation. In particular, experimental studies can clarify the explicandum: they can "uncover regions of vagueness in extensions and intensions of concepts", "reveal instances of conceptual pluralism", "discover sources of bias", "discover unpredictable (even if non-biasing) influences on conceptual judgments", and "outline a concept's central features" (p. 390).[4] Having explicitly mapped out such features of the relevant

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[4] It is unclear whether Shepherd and Justus intend survey participants to be folk, theorists or a mixture thereof. Their challenge to the explicator is framed in terms of *folk* concepts (see e.g. their brief comment about 'radical revisionism', quoted above), suggesting that only folk need be participants. But Shepherd and Justus are also explicitly aware that explicanda can be drawn from an earlier stage of theorising (2015: 388), and folk intuitions would presumably be irrelevant in such cases. Regardless, nothing in what follows turns on how Shepherd and Justus spell the details out here.

explicandum, the explicator will be better placed to perform "the explicative evaluation of [its] conceptual content" (p. 382)—and thus better placed to pinpoint the content that deserves preservation.

Shepherd and Justus take themselves to have shown that experimental philosophy "has an important function within explication" (p. 400). However, *pace* Shepherd and Justus, it is far from clear that this is so. There are at least three reasons: first, it is unclear that there is any reason to deviate from Carnap's non-experimental construal of explication preparation; second, Shepherd and Justus have not provided us with a mechanism by which experimentation might have a genuine effect upon explications; and third, there are reasons to think that any such effect would be minimal anyhow.

First, there is no obvious reason why we should want to deviate from Carnap's non-experimental construal of explication preparation in the first place. As noted above, Shepherd and Justus motivate such deviation by suggesting that some explicanda can be vague, amorphous and imprecise, and can contain content that is liable to mislead the explicative process. However, explication preparation, as conceived by Carnap, is already sufficient to overcome such deficiencies in the explicandum. For Carnap, explication preparation serves to make clear the sense in which the explicandum is to be explicated by pointing towards how the explicandum is, and is not, intended to be understood. Recall his preparation of the explicandum SALT, quoted above; he achieves clarification perfectly well with a single sentence, despite the fact that the ordinary term "salt" is ambiguous. It is unclear what an empirical study of the concept SALT would have added to the situation.

Consider a less mundane example, such as Haslanger's project to explicate GENDER (2000). (Haslanger does not explicitly use the term "explication". But, if we allow fruitfulness to incorporate political and social ends, as do Carus (2007) and Dutilh Novaes and Reck (forthcoming), then Haslanger's project is clearly an example of explication.) While mentioning a variety of different uses of "gender" in the literature, Haslanger prepares the explication by writing the following:

> The guiding idea is sometimes expressed with the slogan: "gender is the social meaning of sex".
> [...] My strategy is to offer a focal analysis that defines gender, in the primary sense, as a social class. A focal analysis undertakes to explain a variety of connected phenomena in terms of their relations to one that is theorized as the central or core phenomenon. As I see it, the core phenomenon to be addressed is the pattern of social relations that constitute the social classes

of men as dominant and women as subordinate; norms, symbols, and identities are gendered in

relation to the social relations that constitute gender. (Haslanger 2000: 37)

Now, this explication preparation is sufficient for the purposes of 'making clear what is meant as the explicandum': Haslanger intends to define GENDER in the sense in which it is taken to be the social meaning of sex, and in which it is tied explicitly to a particular pattern of social relations. No further information is required to get an initial handle on Haslanger's intended explicandum. In particular, for the purposes at hand, we do not appear to need any experimental evidence about folks' (or feminists' or social scientists') conceptions of GENDER.[5] Haslanger's simple statement of what she has in mind is enough. And yet the folk concept GENDER is arguably vague and imprecise (there are borderline cases of MAN and WOMAN), amorphous (it is sometimes used interchangeably with SEX), and liable to mislead (due to the pretheoretic exclusivity and exhaustiveness of the MAN/WOMAN distinction). If this is right, then there is no obvious reason to deviate from Carnap's construal of explication preparation: non-experimental explication preparation serves its purpose.

The second concern about Shepherd and Justus' proposal is this: they have provided no mechanism by which experimentation can affect the explicative process in any meaningful way. Their proposal is that the explicator should experimentally clarify the explicandum before embarking on the explicative process. But there is no obvious reason to think that such clarification will benefit the explicative procedure.

Ultimately, it is up to Shepherd and Justus to provide the relevant details. But here are two possible mechanisms that they might have in mind. The first mechanism is this: by making the explicator aware of any vagueness, pluralism, bias, etc., associated with the explicandum, experimental explication preparation may highlight potential pitfalls facing her attempt to construct a precise explicatum. However, such a mechanism would seemingly render experimental explication preparation of minimal value. For any serious explication, vagueness, pluralism, bias, etc., are not the relevant pitfalls (and, indeed, can be overcome by non-experimental explication preparation). A serious explicator has prior knowledge of the field for which she is constructing the explicatum, and will be

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[5] Of course, if the project were to clarify *their* conceptions of GENDER, then empirical evidence would presumably be required.

able to situate her intended understanding of the explicandum accordingly (as Haslanger does, above). The principal pitfalls facing the explication will typically be theoretical, and are most likely to come to light through a thorough understanding of the theoretical terrain.

For example, following the above quotation, Haslanger goes on to sketch two theoretical problems facing any attempt to explicate the concept GENDER: "the commonality problem questions whether there is anything social that females have in common that could count as their 'gender' […]. The normativity problem raises the concern that any definition of 'what woman is' is value-laden, and will marginalize certain females […]" (2000: 37). The precise nature of these problems is not of concern here. The point is that *those* are the serious pitfalls that Haslanger faced; and it took knowledge of the theoretical terrain, rather than experimental studies, to draw them out. I see little reason to doubt that a parallel point would apply in other cases of explication.

A second mechanism that Shepherd and Justus might have in mind is this: by giving the explicator a clear idea of the intuitive content associated with her explicandum, experimental explication preparation allows her to more readily evaluate that intuitive content with respect to the four requirements (similarity, precision, fruitfulness and simplicity). Such a suggestion, however, appears in tension with the method of explication: explication does not involve the evaluation of the intuitive content of an explicandum to determine which aspects of that content should be kept and which aspects discarded. Rather, explication involves the *construction* of an explicatum designed to play a theoretical role, and an evaluation of the content of the *explicatum*. The explicator only ever considers the extent to which the *explicatum* satisfies the four requirements: at no stage does she evaluate the intuitive content of the *explicandum*.

One way or another, Shepherd and Justus must provide a mechanism by which, on their proposal, experimentation affects explication—whether by defusing the above comments or proposing an alternative mechanism. Without a plausible mechanism, there is little reason to accept that experimental explication preparation can have an important role within explication.

Finally, even if Shepherd and Justus can fill in the details, there is nonetheless reason to expect that, on their proposal, the effect of experimentation would be minimal. The heavy lifting within any explication is done by the construction of a fruitful explicatum. *That* is the step within an explication

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that facilitates future theorising. But, experimental explication preparation does not contribute to it directly. Experimental explication preparation serves to map out any vagueness, pluralism, bias, etc., in the explicandum. Yet such maps do not obviously indicate how to construct fruitful explicata. As noted above, the explicator does not begin with a full description of an explicandum in order to isolate the content that will prove theoretically useful; rather, she begins with a theoretical need and, to satisfy that need, she seeks to construct a theoretical concept that resembles the explicandum in certain respects.[6]

Consider, for example, a recent example: the concept PLANET.[7] Until recently, there was no agreed upon definition—merely nine canonical instances. However, in the late twentieth century, a number of objects orbiting the sun, comparable in size to Pluto, were discovered in the Kuiper belt. Following such discoveries, in 2006, the International Astronomical Union explicated PLANET in order to provide a more principled taxonomy of celestial objects. A planet was henceforth to be an object such that: (a) it orbited a star but did not orbit another planet; (b) it was large enough for gravity to have formed it into a sphere but not large enough for its gravity to trigger fusion; and (c) it had cleared its orbit of debris. Pluto, and the objects discovered in the Kuiper belt, were demoted to the status of *dwarf-planet*.

What is important here is that the explication was driven by the theoretical need for a principled taxonomy of celestial objects. To find such a taxonomy, it was necessary to consider the properties of the celestial objects in question, rather than folk or scientists' intuitions about what falls under their prior concept PLANET. As professor of astronomy Michael A'Hearn puts it:

> Why do we, as scientists, care how Pluto (or anything else) is classified? […] Scientists put
> things into groups, the members of which share common properties, in order to find patterns
> that will enable us to better understand how the bodies work or how they became what they are.
> […] [I]t is clear that Pluto is not a planet like Jupiter but is rather a planet like the numerous

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[6] The situation here is complicated by the positive view I develop in §4. Nonetheless, the point will remain: experimental explication preparation *per se* will not lead to more fruitful explicata than Carnap's non-experimental explication preparation.

[7] See e.g. Tyson 2009, Weintraub 2007.

Plutinos that live in the 3-2 libration with Neptune. Thus Pluto should be classified as the largest

Plutino. (Quoted in Weintraub 2007: 229)

From the explicator's perspective, the important step in the explication of PLANET was to ascertain the common properties that would facilitate future theorising: once these properties are highlighted, the definition (a)–(c) could be constructed.[8] This is the important work in explication and, on Shepherd and Justus's proposal, experimentation appears to be irrelevant to it.

There are concerns, then, with Shepherd and Justus' specific proposal. I do not take these comments to be decisive but, until Shepherd and Justus provide more details, I will remain sceptical about the prospects of experimental explication preparation. In the next section, I sketch an alternative proposal.

4. Fruitfulness, Uptake and Experimentation

Consider the following characterisation, due to Kitcher, of the attempt to explicate the concept FITNESS in philosophy of biology.

Practicing evolutionary biologists know how to measure fitness. They do so by counting offspring. […] [V]irtually all philosophical concern with the notion of fitness starts from the idea that any identification of fitness with actual reproductive success must be resisted. The philosophical problem of fitness that has dominated discussions in recent decades has been to find some useful surrogate for the measure that field biologists seem to be using. One noted proposal has been the so-called *propensity interpretation of fitness*; a rival has been to suggest that 'fitness' ought to be treated as a theoretical term, whose meaning is partially specified by the correspondence rules of Darwinian evolutionary theory. […]. For the most part, biologists have ignored the arcana of philosophical accounts of fitness. (Kitcher 2008: 120–122)

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[8] On the view I develop in §4, the fruitfulness of the explicatum depends in part on uptake by relevant theorists: so, facilitating future astronomical theorising involved, in part, providing an explicatum that astronomers would indeed *use*.

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According to Kitcher, philosophers have been caught up with the idea that scientific theories and explanations are to be understood in terms of scientific *laws*. From such a perspective, the theory of evolution appears to be governed by a principle "to the effect that traits of relatively greater fitness will become more prevalent in a population" (2008: 120); but any such principle is trivialised if, following evolutionary biologists, fitness is defined in terms of *actual* reproductive success. Kitcher, however, suggests that the underlying view of scientific theories and explanations is inappropriate in this case: "[t]he practice of evolutionary biology […] couples detailed mathematical accounts with empirical data about the causes of some component of fitness […], and there is no need to invoke any grand principle of natural selection" (p. 121). The philosophers' explication, then, is of little value to actual practice in evolutionary biology.

Accepting Kitcher's characterisation of the situation for the sake of argument, it is tempting to conclude that the philosophers' explicata for FITNESS have not been particularly fruitful. Regardless of whether the explicata could be used to formulate a 'grand principle of natural selection', or whether they could facilitate the generation of new knowledge or provide answers to significant questions, the philosophers' explicata have not influenced scientific practice. This is suggestive of the following: *uptake* can be a contributing factor to the overall fruitfulness of an explicatum. That is, one explicatum might be more fruitful than another if, all else being equal, the former but not the latter is adopted by the relevant theoretical community as a replacement for the explicandum in question.

For the remainder of this paper, I seek to pursue this line of thought. I sketch the conception of fruitfulness I have in mind, before explaining how it affords experimental philosophy a genuine role in the method of explication.

First, fruitfulness. We saw in §2 that different theorists understand fruitfulness in different ways. For Carnap, a concept is fruitful insofar as it features in relevant laws and generalisations; for Dutilh Novaes and Reck, insofar as it produces new knowledge; for Kitcher, a concept of biological, earth and human sciences is fruitful insofar as it facilitates the provision of answers to significant questions; and for Shepherd and Justus, an epistemic concept may be fruitful by its improving our reasoning ability. Now, these different ways of understanding fruitfulness are not in conflict, and we

should not try to decide between them. Rather, these theorists have highlighted a cluster of criteria, such that different concepts may be fruitful by satisfying different criteria.

Three points are worth noting. Firstly, it is unlikely that each discipline will have its own, well-defined cluster of criteria. Consider again Kitcher's suggestion that fruitfulness for biological, earth and human scientific concepts be understood in terms of the provision of answers to significant questions. If Kitcher is right about this, then, nonetheless, his criterion might also be appropriate in other disciplines: perhaps, say, the concept of STRING in string theory is fruitful in part in virtue of its providing an answer to the question "What is the world made up of?". And, conversely, other criteria might nonetheless sometimes be appropriate in the biological, earth and human sciences: perhaps, say, the evolutionary biological concept RELATIONSHIP CO-EFFICIENT is fruitful in virtue of its featuring in a law, namely Hamilton's Rule. Of course, it may be true that some criterion is particularly well suited to some specific discipline(s)—indeed, I take that to be what Kitcher has in mind, and I have no objection to him so understood—but, regardless, we should not identify some given criteria of fruitfulness as being definitively *for* a specific discipline.

Secondly, the cluster of criteria might turn out to be open-ended. That is, we may be unable to give a list of criteria such that, for *any* given concept, it can only be fruitful by satisfying some of those criteria. (This is why I call it a 'cluster' rather than a 'set'.) Criteria for fruitfulness are, at least to some extent, dependent on the specific aims that a concept engineer may have, and the specific context in which a concept is explicated may suggest its own criteria for fruitfulness. At the very least, it is a viable enterprise to suggest new criteria that hitherto have not been recognised as belonging to the cluster.

Thirdly, the thought is *not* that there is some algorithm for determining the overall fruitfulness of a concept by looking at the extent to which it satisfies the various criteria. Rather: a given criterion will only be relevant in *some* theoretical contexts (e.g. if Kitcher is right, then featuring-in-laws is largely irrelevant in the context of evolutionary biological explanations involving fitness); it may not be obvious in advance *which* criteria will be relevant (e.g. if Kitcher is right, then philosophers mistakenly thought that featuring-in-laws *was* typically relevant in the context of evolutionary biological explanations involving fitness); and there may not be any *rationally preferred* way to weight their relative importance (e.g. different theorists may come to different judgements about which of two

concepts is the more fruitful, without either being in error). This is not to say that 'anything goes'. There may simply be context-sensitive and subjective elements to determining fruitfulness.

I suggest we think of *uptake* as a criterion for fruitfulness: being adopted by the relevant theoretical community in place of the relevant explicandum may sometimes contribute to the overall fruitfulness of the explicatum. There are two points to make about this.

Firstly, there are at least two possible understandings of the criterion. On one understanding, an explicatum might satisfy the criterion by its *in fact* being adopted by the relevant theoretical community; then, for example, the explicatum might become more fruitful over time by its being deployed more widely. On the other understanding, an explicatum might satisfy the criterion by its being *likely to be adopted* by the relevant theoretical community; then, for example, an explicatum that is highly fruitful in part by satisfying this criterion might by historical accident happen *not* to be adopted by the relevant theoretical community.[9] It is unclear to me which option (if either) is to be preferred. The former is far the simpler; but, if we want the actual fruitfulness of candidate explicata to be a factor during the explicative process, then the latter is perhaps preferable. Regardless, nothing essential herein turns on the choice, and so I remain neutral in what follows.

Secondly, who the relevant theoretical community is will depend on the intended purpose of a given explication. Recall the philosophers' explications of FITNESS. I suggested above that, accepting that evolutionary biologists have ignored those explications, it is natural to say that the explicata are not fruitful. However, this is fair only insofar as the philosophers in question *intended* their explications to be relevant to the biologists; if the intention had been, say, merely to provide a rational reconstruction of evolutionary biology, then the evolutionary biologists' ambivalence towards the explicata would plausibly have been irrelevant to the philosophers' aims. In the former case, then, the relevant theoretical community is the community of evolutionary biologists; but in the latter case, it would rather be the community of philosophers of evolutionary biology. One must look to the intentions of the explicator to determine who the relevant theoretical community is.

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[9] Incidentally, the distinction here parallels that between understanding fitness in terms of *actual* number of offspring, and understanding it in terms of *propensities*.

Let me summarise. I have sketched an account of fruitfulness on which various different criteria can contribute to the overall fruitfulness of the explicatum, where the relevant criteria cannot be determined without reference to the specific theoretical context in which the explication is performed. I have suggested that we consider *uptake* to be one such criterion: some explicata can be fruitful in part by being adopted by the relevant theoretical community.

It should be immediately clear that, construing fruitfulness in this way, experimentation could play an important role in the construction of fruitful explicata. The reason is that determining the conditions under which various communities adopt a given explicatum is an empirical matter. To understand the social, political, psychological, theoretical and other factors that contribute to whether an explicatum is adopted, we will likely require a significant amount of data. In particular, in order to distinguish the factors in play, given the complexity of the case, we will likely require the kind of data that can only be obtained by manipulating just one factor at a time—which is just to say that we would need experimental data. With such data to hand, we could begin to understand how to construct explicata that are more likely to be adopted by the relevant theoretical communities; and this understanding could then be applied in practice in the construction of fruitful explicata. In cases where the uptake criterion applied, experimentation would then genuinely aid the explicative procedure.

Such experimental data, however, are not of the sort typically generated by contemporary experimental philosophy. Rather, they would presumably be generated (if at all) by social and political science and experimental psychology. Nonetheless, it is likely that experimental philosophy would have an important role in the present picture. The reason is this: one factor that is likely to be relevant to whether an explicatum is adopted by a community is how well the individuals in that community take the explicatum to capture the central features of the explicandum, and how well they take it to capture the explicandum's key connections to other concepts. If most theorists within a given community think that the explicatum *fails* to capture the central features of the explicandum, and *fails* to preserve its key connections to other concepts, then the community will likely reject the explication—that is, the explicatum will likely *not* be adopted in place of the explicandum.

(This explains, for example, why it is typically so difficult to explicate a concept uniformly across a variety of disciplines. Consider the concept SPECIES: evolutionary biologists may seek a

taxonomy aligned with evolutionary history; veterinary scientists may seek a taxonomy aligned with physiology; bacteriologists may seek a taxonomy aligned with the interests of (human) medicine; and so on. In each case, at least one community of theorists is likely to reject any given explication of SPECIES because the explicatum fails to capture some feature of the explicandum that theorists in that community take to be central.)

If this is right, then experimental philosophy can have a role to play in the construction of fruitful explicata: in the relevant cases, an individual construction of an explicatum will be more fruitful if it is (more likely to be) adopted by the relevant theoretical community; and the explicatum will be more likely to be adopted if the explicator pays close attention to what the theorists in that community take the central features and key conceptual connections of the explicandum to be; and one task to which experimental philosophy is suited is to uncovering what various groups of people take the central features and key conceptual connections of a concept to be. Indeed, on the latter point, Shepherd and Justus are in agreement, writing that the

> empirically rigorous studies of the kind experimental philosophers conduct [can] outline a concept's features and its dependence relations with other concepts. Work on 'innateness' reveals its central features and indicates the problematic relationships between them (Griffiths et al. 2009). And work on 'free will' has uncovered connections between 'consciousness' and capacities for agential behaviour (Shepherd 2012). (Shepherd and Justus 2015: 390–391).

To expand briefly on one example, Griffiths *et al*. provide evidence that there are three central features that are particularly associated with folk judgements of whether or not a particular trait is innate in a certain kind of organism: Fixity, the trait being generally hard to change once acquired by an organism of that kind; Typicality, the trait being common to organisms of that kind; and Teleology, the trait being something that organisms of that kind are *supposed* to develop or possess (2009: 609). The evidence was obtained by asking participants the strength of the agreement with statements such as "trait *x* is innate", for the eight possible sets of features possessed by the trait (i.e. either Fixed or not, and either

Typical or not, and either Teleological or not). The results suggested that Fixity and Typicality are closely associated with folk judgements of innateness, and Teleology less so.[10]

Although such studies focus on folk, the general point carries across to theorists: experimental philosophy can play a role in determining the central features and key conceptual connections of concepts *as understood by relevant theoretical communities*. The upshot is that experimental philosophy has a potentially important role to play in Carnapian explication.

The proposal, then, is this. Suppose that one seeks to explicate concept C and that one intends the explication to be adopted by theoretical community *T*. Then, to satisfy the fruitfulness requirement, one should seek to satisfy the uptake criterion (perhaps amongst other criteria). To satisfy the uptake criterion, one should seek to maximise the likelihood that *T* will adopt the explicatum in place of C. One partial strategy for achieving this likely involves ensuring that the explicatum captures what the members of *T* take the central features and key conceptual connections of C to be. But, to follow this strategy, one must know what the members of *T* take the central features and key conceptual connections of C to be. Such knowledge can be obtained via the kinds of experiments performed by experimental philosophers. Thus, one is best placed to construct a highly fruitful explicatum for C if one takes into account experimentally obtained data about what the members of *T* take the central features and key conceptual connections of C to be.

It is worth noting briefly that this proposal avoids concerns parallel to those I raised in §3. First, there is reason to deviate from Carnap's conception of fruitfulness: as noted by commentators, his conception is too narrow given the variation between different theoretical disciplines. Second, I have provided a mechanism by which experimentation can have a genuine effect upon explications: explicators are to use experimental data to help guide the construction of explicata. In particular, the explicata are to capture what members of the theoretical community take the central features and conceptual connections of the explicandum to be. And, third, on this proposal, experimental philosophy contributes to the heavy-lifting within an explication: as a result of experimentation along the lines I have suggested, we would expect explicators to construct explicata that are *more fruitful* than would

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[10] See Griffiths *et al*. 2009 for details.

otherwise have been constructed. Insofar as fruitfulness is the principal requirement upon explicata, this is an important result.

If this is on the right lines, then there is clear positive work for experimental philosophy to undertake in connection with Carnap's method of explication. I have cast doubt on the specifics of Shepherd and Justus' proposal to introduce an experimental element to explication preparation, suggesting instead that experimental philosophy can play a role in the construction of fruitful concepts. This provides a clear *practical* role for experimental philosophy, both within the sciences and theoretical inquiry more generally. In this respect, experimental philosophy may rightly be construed as applied philosophy.

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