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Carnap on concept determination: methodology for philosophy of science

James Justus

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Abstract Recent criticisms of intuition from experimental philosophy and elsewhere have helped undermine the authority of traditional conceptual analysis. As the product of more empirically informed philosophical methodology, this result is compelling and philosophically salutary. But the negative critiques rarely suggest a positive alternative. In particular, a normative account of concept determination—how concepts should be characterized—is strikingly absent from such work. Carnap's underappreciated theory of explication provides such a theory. Analyses of complex concepts in empirical sciences illustrates and supports this claim, and counteracts the charge explication is only suitable for highly mathematical, axiomatic contexts. Explication is also defended against the influential criticism it is "philosophically unilluminating".

Keywords Explication · Carnap · Strawson · Definition · Methodology · Precision · Ecological stability · Meaning · Concepts

1 Introduction

Recent criticisms of intuition from experimental philosophers and others have helped undermine the authority of traditional conceptual analysis (see Cummins 1998; Goldman and Pust 1998; Stich 1998; Hintikka 1999; Weinberg et al. 2001; Margolis

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J. Justus (✉)
Philosophy Department, Florida State University and University of Sydney, Tallahassee, FL 32306, USA
e-mail: jjustus@fsu.edu
URL: http://philosophy.fsu.edu/content/view/full/35712

and Laurence 2003; Nichols 2004; Bishop and Trout 2005; Ladyman and Ross 2007; Knobe and Nichols 2008; Weinberg et al. 2010). These criticisms show intuitions are acutely sensitive to non-cognitive factors and often grossly mislead about empirical phenomena (e.g. Griffiths et al. 2009), among other things. As the product of more empirically informed philosophical methodology, this result is compelling and philosophically salutary. But the negative critiques rarely suggest a positive alternative. In particular, a *normative* account of concept determination—how concepts *should* be characterized—is strikingly absent from such work. Most cognition occurs in the currency of concepts so this deficiency goes to the core of philosophy as well as science.

Carnap's underappreciated theory of explication provides the alternative needed. Section 2 describes explication and the epistemic rationale underlying it. One kind of concept determination exemplifies the rationale: definition in science. Unlike lexical definitions, scientific definitions readily sacrifice intuitive accord with the meaning of the (often problematically vague) concept being defined to enhance formal rigor, experimental testability, measurability in the field, mathematical and theoretical unification, etc. The objective is improving the ability to describe, discover, and understand the world by establishing relationships between nonscientific concepts used to do so (or concepts from earlier stages of scientific development) and concepts that better facilitate this goal. Given the remarkable epistemic success of science, Carnap understandably thought concept determination in philosophy should follow suit.

Rather than become a staple of philosophical methodology, explication has been challenged on several grounds. For one, Carnap only casually indicated how a handful of simple empirical concepts could be explicated. Explicating abstract concepts in highly formal, axiomatic contexts (e.g. analyticity, mathematical truth, probability) was the principal focus throughout his career. This invites the charge that explication is not suitable for the complex empirical concepts and nonaxiomatic contexts actually found in most scientific work and the grist for most philosophy of science. Section 3 deflects this criticism by describing two applications of the method to one such concept in biology, ecological stability. Besides showing how a single concept can be explicated in distinct ways, the different explications also illustrate priority relations between Carnap's adequacy criteria for explication.

Conceptual flexibility is at the heart of explication: the license to deviate from the intuitive meaning of concepts to reap epistemic rewards. Some claim this constitutes a fatal flaw (e.g. Strawson 1963; Sorensen 1991; Eagle 2004; Loomis and Juhl 2006). Without strong similarity or even synonymy between the concept being explicated and those providing the explication, P. F. Strawson (1963) indicted explication as "utterly irrelevant" to the philosophical analysis of concepts. By deviating from a concept's prior meaning—no matter how problematic it may be—one abandons the only legitimate source of philosophical insight.

Section 4 describes this criticism. Section 5 argues it caricatures explication and that the view of conceptual analysis it presupposes involves a fundamental misalignment of method and objective. Traditional conceptual analysis typically targets concepts found in natural language that critical scrutiny reveals are problematic. Intuitions are consulted to help clarify, rehabilitate, and systematize relations between these concepts. Intuitions are therefore assumed to provide the best guidance about concept determination, despite significant divergence among philosophers (Cummins 1998) and across cultures (Weinberg et al. 2001).

If the goal of analysis is merely *descriptive*—to clarify and describe the current meanings of concepts in natural language—this method is certainly relevant. But gauged against this objective, intuitions are not the sole (and unlikely the best) guide to conceptual content. Linguistic analyses of how language speakers actually employ words associated with concepts (Jackson 1998), and studies of the mental representation of concepts in cognitive science (Goldman and Pust 1998) also provide such guidance, against which intuition must compete and be calibrated.

If the goal of analysis is *normative*—to improve our conceptual tools for epistemic inquiry—recent criticisms (and much of the history of philosophy) show that traditional conceptual analysis performs poorly. As an account of concept determination steeped in scientific practice and grounded by its success, explication is designed to achieve precisely this goal. Section 6 concludes that explication offers philosophy, philosophy of science in particular, a cogent alternative methodology, not because the scientific methodology on which it is modeled trumps intuition a priori, but because of its vastly superior record.

2 Explication

Carnap's most detailed exposition of explication (1950, Ch. 1) was intended as a clarificatory preliminary before he explicated 'degree of confirmation' with a logical relation between hypothesis and evidence statements. Explication is the transformation or replacement of an imprecise concept (*explicandum*) with a new concept(s) (*explicatum/explicata*) subject to four adequacy criteria:

(i) Similarity (of meaning) to the explicandum;
(ii) Exactness[1];
(iii) Fruitfulness; and,
(iv) Simplicity.

The first criterion was much weaker than what most philosophers at the time required of a concept's analysis and the philosophical definitions such analyses yielded. Russell's (1905) account of descriptions, for instance, required synonymy between a proposition containing a description and the propositions characterizing it. Moore (1942) also required synonymy between analyzed and analyzing concept(s). But Carnap only required the explicatum be usable in *most* of the cases the explicandum was; explication need not preserve meaning exactly. As the examples discussed below show, even extensions were permitted to change. Carnap never specified the precise degree of similarity needed, but the weak requirement of preserving usage in most of the *important* cases was probably acceptable since criterion (i) permitted "considerable differences" (1950, 7) in meaning. Explication therefore threads between lexical definition, which requires identity of meaning in principle, and stipulative definition, at least if stipulation need not preserve any relationship between meanings (Belnap 1993; cf. Loomis and Juhl 2006, 287).

[1] Following Carnap, 'exactness' and 'precision' are used interchangeably.

Explications are examples, Belnap (1993, 116) notes, of "perhaps the most distinctively philosophical acts of definition," in which, "one wants both to rely on an *old*, existing meaning and to attach a *new*, proposed meaning."

Why allow such latitude? Carnap had a compelling rationale: the increase in fruitfulness afforded by sacrificing close similarity. This conceptual flexibility accords, he emphasized, with scientific practice.[2] Carnap illustrated with a simple example. As the science of zoology emerged, the "prescientific" concept 'Fish', which roughly meant 'animal living in water' and subsumed marine mammals, evolved into a narrower concept 'Piscis' that excluded mammals. According to Carnap (1950, 6), the original meaning was abandoned because:

> zoologists found that the animal to which the concept Fish applies, that is, those living in water, have by far not as many other properties in common as the animals which live in water, are cold-blooded vertebrates, and have gills throughout life. Hence the concept of Piscis defined by these latter properties allows more general statements than any concept defined so as to be more similar to Fish; and this is what makes the concept Piscis more fruitful.

From a contemporary perspective, this example is rather dated. Phylogeny, rather than phenotypic similarity, is the primary reason mammals are excluded from Piscis today (Ereshefsky 2001). The example nonetheless indicates that Carnap (1950, 7) construed fruitfulness as facilitation of "general" statements. For nonlogical concepts, this was gauged by how many law-like generalizations the explication makes possible; for logical concepts, logical theorems.[3] Explication therefore violates the "conservativeness" or "noncreativeness" criterion commonly invoked in theories of definition within formal languages: definitions should not extend the set of theorems (other than those involving the defined concept) derivable within the language (see Belnap 1993).

The exactness criterion requires "rules of [the explicatum's] use…be given in an *exact* form, so as to introduce the explicatum into a well-connected system of scientific concepts" (1950, 7). Ideally, Carnap envisaged the system as a formal language in which explicit meaning postulates and transformation rules specify precise relations between terms that represent concepts, and in which sets of axioms represent scientific theories, as described in the *Logical Syntax of Language* for instance (Carnap [1934] (1937)). In such languages an explicatum could be introduced by explicit definitions that specify relations between it and concepts designated in the axioms, as well as theorems of different theories. This kind of explication maximizes exactness, and as examples Carnap (1950, 16–17) cited recursive definition of arithmetical operations with Peano's axioms and Frege's definition of numerals (1963a, 935).

[2] Some philosophically inclined scientists reached similar conclusions. In support of this flexibility, one of the founders of ecology, Alfred Lotka (1925, Ch. 1), argued that concepts that have emerged for everyday communication will likely not correspond to the concepts that best facilitate epistemic inquiry. Due to their ambiguity or vagueness, concepts commonly assumed to describe the world in everyday communication, such as 'life' or 'causality,' may fare poorly in this regard (Lotka 1925, 16).

[3] Frege, Carnap's instructor at Jena in the 1910s (Carnap 1963c), shared the view that definitions of logical and mathematical concepts should be judged according to their fruitfulness. Definitions should "extend our knowledge" by "drawing boundary lines that were not previously given at all" (Frege 1980, 100–101).

An explicit definition is one way to introduce an explicatum but there are others. An explication may simply identify a term represented in the axioms as an explicatum for a given explicandum. Satisfying the similarity criterion would then depend on what the axioms say about the explicatum—as interpreted in a model of the axioms—evaluated against the meaning of the explicandum. Fruitfulness could be assessed in terms of the theorems the axioms entail.[4] As such, the appeal of this form of explication depends considerably upon the "implicit definition" the axioms furnish for the explicandum. The implicit definition of the set concept furnished by axiomatic set theory, for instance, arguably provides a highly fruitful explication (see Muller 2004).

Carnap was well aware that such systematicity is rare among empirical rather than formal sciences. Exactness for empirical concepts correspondingly focuses less on maximizing precision via integration into axiomatized theories, and more on making concepts *more* precise, particularly through partial formalization or quantification. Carnap (1963a, 935–6) emphasized this explicitly when replying to Strawson (1963): exactness only requires the explicatum be more precise than the explicandum. He granted, in fact, that the explicatum may belong to a "more exact" part of natural rather than formal or scientific language. This appropriately weakened his previous claim that explication should integrate an explicatum into a well-connected systems of *scientific* concepts (1950, 2), for two reasons. First, the more exact part of natural language is presumably more exact because it contains more precise nonscientific concepts. Second, Carnap elsewhere stressed that relations between concepts in natural languages are generally opaque and imprecise compared to conceptual relations in scientific languages (Carnap [1932] (1959)). With ample room to enhance precision and to the extent increasing precision enhances fruitfulness (see below), genuine explications can therefore be formulated in natural language.

Carnap distinguished three types of non-logical concepts (in order of increasing precision): classificatory, comparative, and quantitative. In normal scientific progress, concepts *typically* evolve towards greater precision.[5] 'Temperature' is an example. Carnap (1950, 12) boldly hypothesized that in early, undocumented stages in the development of natural language only classificatory concepts such as hot, warm, and cold existed. Later, comparative concepts like warmer and colder appeared. But not until science emerged was a quantitative concept of temperature introduced to describe phenomena of heat and cold precisely. The evidentiary basis for Carnap's speculation appears scant, but it overlaps remarkably with what meticulously detailed historical analysis reveals about the origin of the temperature concept (see Chang 2004).

Differences between the quantitative temperature concept and the comparative concepts of warmer and colder also show why an explicatum need not be useable in all the cases the explicandum was. Organisms might experience an object to be

[4] Note that since explication may be nonconservative, it may introduce inconsistencies into formal systems.

[5] Carnap carefully emphasized that the evolution towards greater precision was typical, not inevitable. For example, he noted that adequate quantitative explicata for some psychological concepts may not exist, so the focus should be finding fruitful comparative or classificatory explicata (Carnap 1950, 14). In these cases, increasing precision can *decrease* fruitfulness.

warmer or colder at different times due to physiological changes even though the object's quantitative temperature remains constant. Such disparities do not indicate temperature inadequately explicates 'warmer' or 'colder' because, according to Carnap, "we have become accustomed to let the scientific concept overrule the prescientific one in all cases of disagreement" (1950, 12–13). The scientific concept has certainly supplanted the prescientific one in this case, but Carnap intended the claim in full generality: if an explicandum and its explicatum differ in meaning, gains in precision and fruitfulness afforded by the latter would justify it taking priority over the former.[6] This does not require, Carnap also stressed, that that the explicandum must be eliminated.

But this analysis seems vulnerable to an obvious objection: isn't Carnap merely changing the subject? Rather than clarify the phenomenal concepts 'warmer' and 'colder,' Carnap has simply abandoned this goal for another, one of more scientific interest. This worry underlies Strawson's (1963) criticism that if conceptual clarification is the objective, precision and fruitfulnesss are *irrelevant* if meaning is not closely preserved. In fact, he argued these desiderata usually thwart rather than advance the goal of "philosophically illuminating" the explicandum because they typically encourage departure from the original, often imprecise meaning. Before describing the contrasting philosophical methodology underlying this criticism in Sections 4, Section 3 deflects a different kind of objection: that explication may be well-suited for highly *theoretical* concepts—such as those actually explicated in great detail and technical sophistication by Carnap (e.g. analyticity, degree of confirmation, semantic information [Carnap [1934] (1937), 1947, 1950; Carnap and Bar-Hillel 1953])—but that it is ill-equipped for the complex concepts actually employed in *empirical* science. The seemingly oversimplified explications of rather rudimentary empirical concepts that Carnap presents unfortunately encourages this view. Many scientific examples could be marshaled (e.g. Carnap 1977 on entropy, Chang 2004 on temperature), but two very different explications of the complex concept of ecological stability show the criticism is mistaken particularly clearly. They also illustrate how explications can enhance exactness and fruitfulness in distinct ways.

3 Precision, fruitfulness, and ecological stability

The idea there is a "balance of nature" was a staple of the schools of natural philosophy from which biology emerged, long before the term 'ecology' was even coined (Egerton 1973). Some early ecologists continued this tradition by attempting to derive the existence of a "natural balance" in biological populations from organismic metaphors and anthologies with physical systems (see Kingsland 1995). Not until the second half of the twentieth century was the concept of a balance of nature rigorously characterized, and the predominantly metaphysical speculations

[6] Simplicity (criterion [iv]) was subsidiary to the other adequacy criteria for Carnap. He noted that many concepts of well-confirmed and well-developed scientific theories are quite complicated, and suggested simplicity only matters when deciding between explicata of comparable precision, fruitfulness, and similarity to the explicandum. The value of simplicity in explication is therefore secondary and context-dependent, similar to its value in other aspects of science such as inference and explanation (Sober 1988).

about its cause superseded with scientific hypotheses about its basis. Although significant uncertainty and controversy still remains about what features of an ecosystem's dynamics should be considered its stability (see Justus 2008a), most ecologists adopt one of two kinds of accounts of ecological stability.

The first appeals to dynamic systems theory and is frequently found in mathematical modeling in ecology (e.g. May 1974; Logofet 1993; Kot 2001). Let a vector $\mathbf{x}(t)$ (t designates time) represent an ecological system, such as a biological community, each vector component representing a distinct part of the system, such as a species.[7] Let $\mathbf{x}^*$ represent the system at equilibrium. The system at equilibrium is *Lyapunov* stable if and only if:

$$(\forall\varepsilon > 0)(\exists\delta > 0)\left(\left|\mathbf{x}(t_0) - \mathbf{x}^*\right| < \delta \Rightarrow (\forall t \geq t_0)\left(\left|\mathbf{x}(t) - \mathbf{x}^*\right| < \varepsilon\right)\right), \qquad (1)$$

where ε and δ are real numbers, $\mathbf{x}(t_0)$ represents the system at some initial time t_0, and '|·|' designates a Euclidean distance metric. Informally, (1) says $\mathbf{x}^*$ is Lyapunov stable if systems beginning in a neighborhood of $\mathbf{x}^*$ remain near it after being perturbed to the state $\mathbf{x}(t_0)$ at t_0, i.e. that the effects of perturbations are circumscribed.

The second characterization of ecological stability is more frequently found in recent empirical studies in ecology (e.g. Tilman 1999; Lehman and Tilman 2000). Let B_i be a random variable designating the biomass of species i in an n-species community C assayed during some time period, and let $\overline{B_i}$ designate the expected value of B_i for the time period it is assayed. Tilman (1999) defined the *temporal stability* of C, $S_t(C)$, as:

$$S_t(C) = \frac{\sum_{i=1}^{n} \overline{B_i}}{\sqrt{\sum_{i=1}^{n} Var(B_i) + \sum_{\substack{i=1;\, j=1 \\ i \neq j}}^{n} Cov(B_i, B_j)}}, \qquad (2)$$

where *Var* designates variance and *Cov* designates covariance. Unlike (1), S_t characterizes ecological stability in terms of the variability of biomasses of species in the community. The idea motivating this account is that if two series of abundances are plotted across time, the more stable one exhibits less fluctuation (Lehman and Tilman 2000).

Both accounts satisfy the exactness criterion by formulating precise explicata, but in utterly different ways. The first increases precision by proposing that the formal statement (1) characterizes ecological stability. The second, however, increases precision by characterizing ecological stability as a quantitative property whose value is determined by the mathematical function (2).

(1) and (2) increase precision in different ways, and they are fruitful in different ways. The first explication integrates the concept of ecological stability into a well-developed mathematical theory that provides analytic methods for evaluating the

[7] Specifically, the components of $\mathbf{x}(t)$ are variables that usually represent densities, abundances, or biomasses of species in the community. Components of the equilibrium state $\mathbf{x}^*$ therefore represent these densities, abundances, or biomasses at equilibrium.

stability of mathematical models of biological communities. This increase in formal rigor and tractability, however, has nontrivial costs (see Justus 2008b). Accurate mathematical models describing the dynamics of real-world ecological systems are usually unavailable. Without such models, evaluation of whether (1) holds must be done with time series data on $\mathbf{x}(t)$, which is extremely difficult. Strictly speaking, for instance, the universal quantification in the consequent of (1) [$\forall t \geq t_0$] requires monitoring system behavior over indefinite time periods. Whether (1) holds also depends on system behavior after one perturbation (represented by the displacement from $\mathbf{x}^*$ to $\mathbf{x}(t_0)$). Empirical evaluation of this behavior in the field is often obscured when environments subsequently (and frequently) perturb systems.

In fact, the infeasibility of empirically evaluating (1) was a disadvantage Lehman and Tilman (2000, 535) cited in favor of (2): temporal stability is “readily observable in nature” because evaluating (2) only requires empirical measurement of biomasses, rather than the difficult empirical assessment of dynamic system behavior (1) requires. Once the concept is made empirically tractable in this way, the role stability may have in system dynamics and its possible correlations and causal relationships with other features of biological communities can be rigorously investigated (see Tilman 1999). Greater measurability with (2), however, also has nontrivial costs. Unlike Lyapunov stability, temporal stability has no theoretical underpinning in a mathematical theory of system dynamics, such as dynamics systems theory (Hinrichsen and Pritchard 2005). (2) also does not characterize ecological stability in terms of how communities respond to perturbation, which seems indispensable to the concept (see Justus 2008a, §5), and therefore threatens to violate the similarity criterion. Ideally, an explication would enhance fruitfulness while retaining sufficient similarity to the explicandum, without tradeoffs. The tradeoffs in fruitfulness between (1) and (2)—in this case between a more empirically and a more theoretically oriented conception of fruitfulness—indicate this may be rare in science.

(1) and (2) explicate ecological stability in different, nonequivalent ways. Lyapunov stability, for instance, is a concept whose application requires that a community initially be at equilibrium, whereas temporal stability places no restrictions on a community’s initial state. The existence of nonequivalent characterizations of the same concept would typically be taken to indicate an underlying problematic ambiguity of that concept. But developing multiple, distinct explicata for even an *unambiguous* explicandum is permissible and sometimes advisable on Carnap’s account of explication. Since preservation of strong similarity of meaning is not required, different explicata can differ from the meaning of the explicandum in distinct ways. Pursuing several distinct explications for a given concept may be useful since different explicata can be fruitful in distinct ways, similar to the merits of different logics (e.g. Carnap, R. [1934] (1937)), and the different merits of (1) and (2). This permissive view of the relationship between the meaning of the explicatum and explicandum, however, prompted the most serious criticism of explication.

Before describing that criticism, it is worth considering more generally why explication is designed to reflect the indispensable role of precision in scientific methodology. Explication requires increasing precision—at one point Carnap even says it is “the only essential requirement” (1963a, 936)—but its value does not derive from precision alone. Rather, precision is paramount because it *usually* enhances fruitfulness. Vague concepts are rarely, if ever, components of the well-confirmed generalizations the fruitfulness criterion targets. The problematically vague concept 'cause', for instance, is not a technical term of contemporary science and currently plays no role in any reputable scientific theory (Dowe 2000).[8] Concepts like 'germ' or 'life' arguably share this status, as 'ecological stability' did before being explicated.

Making concepts more precise, in contrast, often facilitates in developing and discovering well-confirmed generalizations. 'Fish', 'temperature', and the much more complicated and precise characterizations of 'energy' that emerged in physical mechanics and electrodynamics are paradigmatic examples. Without sufficiently precise concepts, it is difficult if not impossible to derive predictions from statements containing them. Without such predictions, in turn, statements cannot be confirmed or disconfirmed. Testing predictions of hypotheses and theories against data is a staple of scientific methodology that has clearly proved to be an epistemically reliable basis for inference. Precision facilitates this methodology and its success therefore grounds the exactness criterion.

Although increasing precision usually enhances fruitfulness, there are exceptions. Two cases of precision increases not producing greater fruitfulness can be distinguished. First, the *current* state of scientific knowledge may be the problem. At a given time, scientists may simply fail to recognize the law-like statements more precise explicata would make possible. Greater precision may not enhance fruitfulness in a stronger sense if the concept being explicated represents something essentially imprecise. Carnap thought some psychological concepts might be examples. Beliefs states, for example, may be best represented with qualitative rather than fully quantitative probabilities (Walley 1991, Ch. 5). In such cases greater precision may not increase, and even decrease fruitfulness due to facts about the world rather than just limitations of scientists' epistemic acumen (see Justus 2006).

Serious doubts about whether law-like generalizations exist in sciences such as biology and psychology also suggest the notion of fruitfulness should be broadened to recognize the improvement an explicatum can have in discovering well-confirmed generalizations through enhancing experimental testability, measurability in the field, theoretical unification, mathematical rigor, etc. rather than just in the production of law-like generalizations. Explications of ecological stability illustrate this more expansive and defensible notion of fruitfulness.

4 Strawson's criticism

Explication contrasts sharply with the method of concept determination predominantly employed in philosophy. Call it 'conceptual analysis.' As aptly represented by Strawson (1963, 514–518), conceptual analysis assumes there are underlying

[8] The qualifiers 'contemporary' and 'currently' are intended to grant that the concept could have such a role in future science. Among extant analyses of causation, Salman (1997) and Dowe's (2000) account of causal interactions as intersections of world-lines that involve exchange of conserved quantities is one candidate; the causation concept sometimes found in studies of Bayes nets is another (see Spirtes et al. 2001). These accounts offer characterizations of the concept precise enough to make possible, for instance, its empirical detection and even empirical measurement of causal interaction strength.

principles governing concepts and their use in ordinary communication, or that would in highly idealized, maximally rational, and philosophically informed versions of this communication. Elucidating these principles is the goal of conceptual analysis, and philosophical definition as its end product.[9] Details about the way concepts are actually deployed, intuitions about them, and thought experiments that probe their conditions and range of application provide the raw material from which the elucidation is derived. Strawson hoped that judicious scrutiny of these factors would uncover the "logical behavior which ordinary concepts exhibit" (Strawson 1963, 513) which the "elusive, deceptive modes of functioning of unformalized linguistic expressions" (1963, 512) often mask. Almost three decades later, Strawson offered an analogy to help clarify the methodology (1992, 7):

> Just as we may have a working mastery of the grammar of our native language, so we have a working mastery of [our] conceptual equipment. We know how to handle it, how to use it in thought and speech. But just as the practical mastery of the grammar in no way entails the ability to state systematically what the rules are which we effortlessly observe, so the practical mastery of our conceptual equipment in no way entails the possession of a clear, explicit understanding of the principles which govern our handling of it, of the theory of our practice. So—to conclude the analogy—just as the grammarian, and especially the model modern grammarian, labours to produce a systematic account of the structure of rules which we effortlessly observe in speaking grammatically, so the philosopher labours to produce a systematic account of the general conceptual structure of which our daily practice shows us to have a tacit and unconscious mastery.

Finding such principles would clarify the underlying "structure" of our concepts. As such, Strawson claimed the method provides the best basis for defensible definitions of concepts and the best way, perhaps only way, to resolve the conceptual confusions and the philosophical problems they generate.

Contrasted with this methodology, Strawson objected that explication is not similarly "philosophically illuminative":

> however much or little the constructionist technique [i.e. explication] is the right means of getting an idea into shape for use in the formal or empirical sciences, it seems prima facie evident that to offer formal explanations of key terms of scientific theories to one who seeks philosophical illumination of essential concepts of non-scientific discourse, is to do something utterly irrelevant—is a sheer misunderstanding, like offering a text-book on physiology to someone who says (with a sigh) that he wished he understood the workings of the human heart. (Strawson 1963, 504–505)

Part of his basis for this claim was the observation that:

> it seems in general evident that the concepts used in non-scientific kinds of discourse could not literally be *replaced* by scientific concepts serving just the

[9] "Paradigmatic conceptual analyses offer definitions of concepts that are to be tested against potential counterexamples that are identified via thought experiments" (Margolis and Laurence 2008, §2.1).

same purposes; that the language of science could not in this way *supplant* the language of the drawing-room, the kitchen, the law courts and the novel. (Strawson 1963, 505)

Since scientific concepts cannot replace nonscientific ones and retain the same functionality, there is no reason to expect a "well-connected system" of the latter would shed any light on the former. It follows, Strawson asserted, "that typical philosophical problems about the concepts used in non-scientific discourse cannot be solved by laying down the rules of exact and fruitful concepts in science. To do this last is not to solve the typical philosophical problem, but to change the subject" (1963, 506). Instead of yielding insights about concepts that generate philosophical puzzles, explication pursues the project of finding precise substitutes for them. It thereby abandons the only subject matter Strawson believed could resolve the puzzles. Desertion does not constitute illumination.

Strawson located the motivation for explication in an untenable, even dogmatic dismissal of traditional philosophical problems (Strawson 1963, 517). Conceptual analysis, in contrast, involves careful analysis of the actual concepts and usage that generate the problems. Thus, for Strawson explicata will only provide insights about concepts (explicanda) if there is a clear account of the relations between them. But this requires an accurate description of the explicanda, as conceptual analysis at least might provide. One could only think otherwise, Strawson (1963, 513) suggests, "if one is led away from the purpose of achieving philosophical understanding by the fascination of other purposes, such as that of getting on with science."

5 Philosophical methods and objectives

Carnap and Strawson's disagreement primarily concerns the similarity criterion. Carnap was willing to sacrifice close similarity to achieve greater precision and fruitfulness. For Strawson, as one deviates from a concept's content one abandons the goal of philosophically illuminating it in favor of something else.

5.1 Common ground

Despite their differences, there is a significant point of potential accord between Carnap and Strawson unappreciated by contemporary commentators (e.g. Loomis and Juhl 2006; Maher 2007). Carnap and Strawson could agree about one aspect of the issue: understood as an *empirical* method of inquiry about concepts, conceptual analysis *can* help clarify the current meanings of explicanda. Appropriating Strawson's (1992) analogy, just as linguists analyze linguistic practices of natural language speakers to determine syntax and semantics, how philosophers think about concepts helps identify their meanings and elucidate their interrelationships. How terms that designate concepts are employed in everyday communication also helps pinpoint these meanings and relationships (Jackson 1998),[10] and studies of the

[10] This assumes that usage patterns for terms of natural language provides reliable information about (but does not necessarily determine) meaning.

mental representation of concepts in cognitive science are similarly salient (Goldman and Pust 1998). Cognitive studies and linguistic analyses of term use are therefore relevant to conceptual analysis understood in this empirical sense. In fact, Carnap (1963a, 920) explicitly advocated this kind of approach:

> It seemed rather plausible to me from the beginning that there should be an empirical criterion for the concept of the meaning of a word or a phrase, in view of the fact that linguists traditionally determine empirically the meanings, meaning differences, and shifts of meanings of words, and that with respect to these determinations they reach a measure of agreement among themselves … In [Carnap 1955], I tried to show the possibility of giving operational rules for testing hypotheses concerning the intensions of predicates of a natural language, on the basis of responses by the users of this language.

'Intension' is Carnap's term for a predicate's meaning. Developing this method for determining predicate intensions in natural languages (Carnap 1955) was part of Carnap's response to Quine's criticisms of an analytic-synthetic distinction. If intensions can be identified with this method, the analyticity of statements concerning these intensions can be assessed. The significant congruence in Carnap and Strawson's own response to Quine is striking given their disparate philosophical commitments (see Grice and Strawson 1956).

In fact, Carnap accords an important role to this type of empirical conceptual analysis in explication. Before proposing an explicatum, Carnap stressed that an explicandum's meaning should be clarified as much as possible through informal methods (Carnap 1950, 3–5). This preliminary clarification helps determine whether an explicandum is ambiguous, which suggests multiple explicata are appropriate, and whether an explicandum is too vague to even explicate.[11] If an explicandum is too vague, presumably its similarity to potential explicata cannot be evaluated, and attempts to explicate it would therefore be pointless.[12] Carnap (1963a, 936) suggested, furthermore, that such informal clarification alone may resolve some philosophical confusions, particularly those that emerge when natural language terms are used outside their normal scope in everyday communication. Experimental philosophers' emphasis on empirically uncovering the (sometimes indefensible) factors responsible for intuitions and other conceptual judgments, among the proverbial "folk" as well as the philosophically trained, makes an important contribution to this element of explication.

Carnap could even grant that proverbial "armchair" conceptual analysis—where the predominant focus is what intuitions reveal—can help identify the current meaning of an explicandum. Philosophers are (usually) competent natural language speakers and especially keen to subtle differences in meaning. Their understanding of concepts at least constitutes relevant empirical data about their

[11] "[Philosophers] often immediately start to look for an answer [to questions like 'What is *X*?' where *X* is some concept] without first examining the tacit assumption that the terms of the question are at least practically clear enough to serve as the basis for an investigation, for an analysis or explication" (Carnap 1950, 4).

[12] Carnap (1950) did not make this argument explicitly.

meaning (Goldman and Pust 1998). Thus, far from "dismiss problems about ordinary language" (Maher 2007, 333), Carnap recognized a role for traditional philosophical approaches to such problems in the first step of explication. This fact is too often overlooked by critics (Strawson 1963; Eagle 2004; Loomis and Juhl 2006) and advocates (Maher 2007) of explication alike. Of course, since philosophers' intuitions about concepts likely reflect their theoretical commitments, their intuitions will likely disagree (because their commitments typically diverge), and fail to reflect those of the general populace (because these commitments are not shared) (Dowe 2004).

5.2 Method–objective mismatch

Despite this potential accord, "clarification of the explicandum" was merely preparatory for Carnap. Although attempting to clarify an explicandum's meaning with the methods considered above can be a valuable pre-explication preliminary, explication requires a very different methodology, one that sanctions tradeoffs between preserving meaning and enhancing precision and fruitfulness.

Aligning methodological resources with philosophical objectives is the central issue. As Section 4 indicates, Strawson utilized a more substantive conception of conceptual analysis than the empirical version Carnap could accept. On this view, arguably the dominant view in philosophy, conceptual analysis is not simply empirical inquiry into what philosophically trained individuals intuit about concepts.[13] The results of such inquiries, and definitions based on them, would seem to lack any normativity. They would merely *describe* how a fairly small, generally unrepresentative group of individuals construe certain concepts, not *prescribe* how they should be.

Rather than accept this attenuated view, intuitions are taken to confer special status on such analyses: "[conceptual analysis] attempts to show the natural foundations of our logical, conceptual apparatus, in the way things happen in the world, and in our own natures" (Strawson 1963, 516). Its purpose is therefore to uncover the "foundation of our concepts in natural facts, and to envisage alternative possibilities, it is not enough to have a sharp eye for linguistic actualities" (1963, 517). Intuitions about this "natural foundation" supposedly supply conceptual analyses with prescriptive force. Deviation from intuition to increase precision or fruitfulness, regardless of how epistemically advantageous and inductively warranted, is therefore indefensible.

The fundamental question confronting this view of conceptual analysis is what, if anything, do intuitions demonstrate beyond how a set of humans understand and use

[13] One potential exception is Jackson (1998, 44), who is "suspicious" about the view that conceptual analysis provides insights about the world. He calls this "immodest" conceptual analysis, and expresses doubts about his earlier use of it in the knowledge argument (Jackson 1986). Jackson (1998) defends instead a "modest" conceptual analysis that attempts to determine the folk theory he assumes underlies concept use in natural language and on which cognitive and linguistic studies bear. But if this is the goal, it is unclear what legitimate role modest conceptual analysis has in metaphysics, as Jackson believes it has. As traditionally understood, metaphysics is concerned with uncovering the fundamental nature of the world, not anthropological theorizing (*cf.* Jackson 1998, Ch. 3).

particular concepts?[14] Evaluating this question requires distinguishing two objectives. One is whether intuitions *can* help clarify the current content of concepts, which there is little reason to doubt (as discussed above). The stronger, more problematic goal is that intuition provides reliable insights about the "foundation of our concepts in natural facts," insights that potentially compete with scientific claims, and which indicate how concepts *should* be determined. The first goal is descriptive, the second is normative. Given its poor track record on the second (e.g. Harman 1994; Hintikka 1999; Griffiths et al. 2009), the onus is on intuition proponents to defend its epistemic utility and role in concept determination. Different attempts to do so have been made, but several compelling criticisms cast serious doubt about its feasibility.

A review of the literature underlying the growing recognition that intuition is epistemically suspect cannot be undertaken here. It has been thoroughly elsewhere (e.g. Levin 2004; Ladyman and Ross 2007; Knobe and Nichols 2008). But without retracing well-worn steps, it is worth briefly recounting why it is unlikely a convincing defense will be forthcoming. What is required, Goldman and Pust (1998) note, is a convincing explanation of how intuitions, which are mental events of some kind, are a reliable source of information about extra-mental facts. With visual perception, for instance, this is supplied by a complex set of causal relationships that includes the objects being perceived, the light reflected from them, the perceiver's retinas, and the visual cortex. Currently, some details of these relationships and those involved in other types of perception may be unknown, but it is clear what *kind* of connection exists between the extra-mental objects or properties perceived and our perceptions of them. The same cannot be said for intuition. The common contention that concepts are abstracta and truths about them are grasped through a non-empirical faculty of intuition obscures more than it clarifies (see Levin 2004). The apparently significant diversity of intuitions across cultures only exacerbates this and other difficulties (Stich 1998; Weinberg et al. 2001).

6 Conclusion: conceptual methodology for philosophy of science

If recent criticisms of intuition are sound, much of philosophy is in need of a new methodology. This does not mean traditional conceptual analysis has no philosophical future. If *description* is the objective—clarifying the current meanings of terms, elucidating the putative 'folk' theories underlying concepts, etc.—conceptual analysis is one of many possible sources of information (anthropological, cognitive, and linguistic studies are others). And for some philosophical issues, acquiring descriptive

[14] Another difficulty specific to Strawson's grammar analogy will not be pursued here. Strawson assumed an underlying "system" or "structure" exists that governs our concepts and explains how the ordinary language terms referring to them facilitate communication (1963, 513; 1992, Ch. 2). The form of this conceptual system was never made clear. Strawson called it "logical" (1963, 513), however, and suggested conceptual analysis should reveal *logical* relations between concepts, *i.e.* necessary and sufficient conditions. Whether there are conceptual relations of this kind is an open question and psychological experiments seem to disconfirm Strawson's claim (*e.g.* Rosch and Mervis 1975). Based on evidence that context-sensitive prototypes govern concept use, for instance, Ramsey (1998) argues that the traditional goal of conceptual analysis of finding necessary and sufficient conditions for concepts that are resistant to intuitive counterexamples is misguided.

information about concepts may yield sufficient clarity to resolve disagreements. Ethical judgments, for instance, often concern whether an evaluative concept applies to an act or decision. Intuitions about hypothetical scenarios *may* help reveal unrecognized features of evaluative concepts relevant to these judgments. But relative to this descriptive goal, intuition-based conceptual analysis contributes but one voice. It must compete against results from experimental philosophy and non-philosophical disciplines, and its results may be (and likely are) less credible about conceptual content. The sensitivity of intuition to seemingly non-cognitive factors such as race, sex, socioeconomic class, and others uncovered by experimental philosophers further challenges the legitimacy of traditional conceptual analysis, even for descriptive projects of this kind, and encourages their emphasis on developing an empirically informed alternative methodology.

This descriptive characterization of conceptual analysis obviously fails to reflect its normative ambitions in other areas, such as epistemology, metaphysics, and philosophy of science. But if the goal is providing normative guidance about concepts by enhancing their cognitive credentials, the critique of intuitions has sweeping implications. If, specifically, the objective is aligning our concepts with a conceptual system that better describes and better facilitates in discovering the structure of the world, then the absence of a credible epistemic basis for intuition entails it is not normatively controlling. Given this objective, explications need not preserve intuitive meaning. Requiring *similarity* ensures some continuity in the epistemically driven conceptual changes explication counsels. But the same rationale that sanctions sacrificing strong similarity may also advise abandoning rather than attempting to explicate some concepts. As a method of conceptual engineering, explication accords no privilege to the conceptual apparatus we inherit and which provides the fodder for traditional conceptual analysis.

Freed from intuitive constraint, concepts and their analysis become just another set of epistemic tools evaluated according to the function they, and the broader cognitive and linguistic structure in which they participate, are intended to serve. Carnap (1963b, 938–939) championed this pragmatic view:

> In my view, a language, whether natural or artificial, is an instrument that may be replaced or modified according to our needs, like any other instrument … A natural language is like a crude, primitive pocketknife, very useful for a hundred different purposes. But for certain specific purposes, special tools are more efficient, e.g., chisels, cutting-machines, and finally the microtome. If we find that the pocket knife is too crude for a given purpose and creates defective products, we shall try to discover the cause of the failure, and then either use the knife more skillfully, or replace it for this special purpose by a more suitable tool, or even invent a new one. [Strawson's] thesis is like saying that by using a special tool we evade the problem of correctly using the cruder tool. But would anyone criticize the bacteriologist for using a microtome, and assert that he is evading the problem of correctly using a pocketknife?

In reply, Strawson (1963, 505) correctly emphasized that precise concepts such as those found in science will likely never replace most of the imprecise concepts used in everyday communication (see §4). Loomis and Juhl's (2006, 291) response to

Carnap's analogy is similarly accurate: "pocketknives are not replaceable by microtomes for most *ordinary* uses" (emphasis added). But concepts being sufficient for *ordinary* uses and providing effective means for *everyday* communication does not entail they are the best or even passable conceptual tools for epistemic inquiry. If this is the 'specific purpose' to which Carnap alludes, transforming concepts into more precise and fruitful ones is warranted, even if departing from their current content is unavoidable. With respect to this normative goal, it is thoroughly opaque what conceptual analysis contributes.

Loomis and Juhl (2006, 291) raise another, related criticism: "someone who was having trouble using a pocket-knife in an *ordinary* circumstance would not be helped in the least by being shown the workings of a microtome" (emphasis added). Although this may be true, its critical force against explication and Carnap's instrumental view of language is unclear. Continuing the analogy, if the pocketknife corresponds to a term of natural language denoting a concept and difficulty arises in an *ordinary* circumstance—i.e. one in which the term is normally used—then it seems the troubled concept user does not understand the term's actual meaning and should consult a lexicon. If the problem persists, it is likely the individual is expecting more of the concept than it delivers in ordinary circumstances where it functions unproblematically. The microtome corresponding to the more precise explicatum was, after all, only recommended as a "special tool" invoked for "certain specific purposes."

A putative sharp distinction between philosophical and scientific concepts, and a belief their analysis requires disparate evaluative standards underlies these criticisms. On this basis, Strawson could agree with Carnap about the advantages of precision for properly scientific concepts such as 'ecological stability,' but insist that exclusively philosophical concepts such as 'knowledge' or 'personal identity' require a different approach. Explication's role in enhancing the relation between the former and well-confirmed generalizations is clear, the objection might continue, but it is not for the latter.

There are several responses to this objection:

(1) Many (perhaps most) concepts that philosophers of science study seem to fall on the "properly scientific" side of the ledger, including 'entropy,' 'field,' 'fitness,' 'force,' 'information,' 'natural selection,' 'signal,' and 'species' among others. If explication is apt for clearly scientific concepts, it is apt for much of the work in philosophy of science. Intuitions can play a role in helping clarify these explicanda, just as intuitions about thought experiments had a crucial role in the theorizing that led Einstein to relativity theory (Norton 1991). But accord with intuition does not *itself* confer epistemic merit on explicata.

(2) Besides Carnap's influential explicatory analyses, much highly regarded work in the philosophy of science mirrors his approach. The deft interplay of empirical and theoretical work displayed in Skryms' (2010) cogent study of meaning with sender-receiver signaling models is a particularly clear recent example, but numerous others share this approach (e.g. Crupi et al. 2007).[15]

[15] Recall that the degree of precision appropriate for an explication depends on what a concept represents (see §3). Some concepts studied by philosophers of science, like some psychological concepts, may concern essentially imprecise phenomena. If scientific explanation is indelibly intertwined with an amorphous sense of subjective understanding, highly precise explicata for it may be less fruitful than less precise ones (see Trout 2002).

(3) Many concepts central to philosophy of science seem to defy label as strictly philosophical or scientific and it is generally unclear on what basis, besides mere disciplinary division, such a distinction between putative types of concepts could be drawn. There are numerous examples: ‘confirmation,’ ‘disposition,’ ‘inference,’ ‘law,’ ‘model,’ ‘natural kind,’ ‘observation,’ ‘probability,’ ‘representation,’ ‘space,’ and ‘time’ among others. Since nothing about these concepts suggests intuition is any more reliable regarding their analysis, explication remains the only viable method of concept determination.

Admittedly, the ultimate goal of explication—finding well-confirmed generalizations—may seem orthogonal to the purpose of analysis in philosophy of science. But this conflates distance with irrelevance. Analyses by philosophers of science and scientists often differ in degree of abstraction and generality, but they do not differ in kind or in their ultimate goal. The *immediate* aim of analyses of concepts in philosophy of science is improving the understanding of their role (or potential role) in scientific practice, as well as the bearing empirical and theoretical results have on characterizations of these concepts and statements containing them. The epistemic success of science (and failure of intuition) merits this focus and analyses of concepts often found in the ‘Theory’ or ‘Concept’ sections of scientific publications share precisely this objective. Although sometimes quite removed from conceptual work, the ultimate payoff in understanding philosophical work affords is measured in the same enduring currency scientists use to measure success: well-confirmed generalizations. Philosophers of science and scientists’ interests and tools typically differ in level of generality and degree of abstraction, but they toil for the same fruit:

> He who wishes to investigate the questions of the logic of science must, therefore, renounce the proud claims of a philosophy that sits enthroned above the special sciences and must realize that he is working in exactly the same field as the scientific specialist, only with a somewhat different emphasis. (Carnap [1934] (1937), 332)

Rather than “offer nothing” to answer Strawson’s objection (Maher 2007, 333), Carnap sought to undermine the standard philosophical methodology in favor of a more scientifically cognizant, inductively warranted alternative. The compelling criticisms of intuition more than a half century later reveal the prescience of Carnap’s efforts.

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