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Concepts – Not just yardsticks, but also heuristics: rebutting Hacker and Bennett

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ABSTRACT

In their response to our article (Keestra and Cowley, 2009), Hacker and Bennett charge us with failing to understand the project of their book *Philosophical Foundations of Neuroscience* (PFN; Bennett and Hacker, 2003) and do this by discussing foundationalism, linguistic conservatism and the passivity of perception. In this rebuttal we explore disagreements that explain the alleged errors. First, we reiterate our substantial disagreement with Bennett and Hacker (B&H) regarding their assumption that, even regarding much debated concepts like 'consciousness', we can assume conceptual consensus within a community of competent speakers. Instead, we emphasize variability and divergence between individuals and groups in such contexts. Second, we plead for modesty in conceptual analysis, including the use of conceptual ambiguities as heuristics for the investigation of explanatory mechanisms. Third, we elucidate our proposal by discussing the interdependence of perception and action, which in some cases appear to be problematic for PFN. Fourth, we discuss why our view of conceptual innovation is different from B&H's, as we plead for linking explanatory ingredients with conceptual analysis. We end by repeating our particular agreement with their mereological principle, even though we present different reasons: psychological concepts should not be applied to mere components or operations of explanatory mechanisms, for which another vocabulary should be developed.

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1. Introduction

In linking neuroscience with conceptual analysis, our article 'Foundationalism and neuroscience' (Keestra and Cowley, 2009) examined what empirical findings about the brain mean for psychological concepts and vice versa. Writing for linguists and others interested in cognitive neuroscience, we stressed that the systems which underlie perception, language and action feature complex interdependencies. In examining conceptual implications, we contrasted our view with that of Bennett and Hacker's (2003) *Philosophical Foundations of Neuroscience* (PFN). Specifically, we denied that neuroscience is to be measured by the yardstick originating in analytical philosophy's attempt to deliver the "[c]onceptual truths [that] delineate the logical space within which facts are located" (Bennett and Hacker, 2007, p. 129). Such a view overlooks the point that, since conceptual analysis is often incapable of completely disambiguating or clarifying the relevant concepts, scientists

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and lay-persons alike are bound to draw on explanatory criteria. Accordingly, we find fault with PFN and object that B&H remain largely silent about methods and findings that, in our view, challenge their position.[1] In replying to our arguments, Hacker and Bennett (this volume) suggest that we failed to understand what they had written.

We regard this colourful rhetoric as showing substantial differences. The most important may be that while B&H take for granted that we converge on knowing how to use the words for psychological concepts, we doubt that, to the extent to which this exists, it always depends on rule-governed use. We are sceptical that the meanings of concepts "are given by what are accepted as correct explanations of meaning by the community of speakers" (Bennett and Hacker, 2003, p. 382) that is to say "*competent* speakers, using words correctly" (Bennett and Hacker, 2003, p. 400, italics added). In our view, where psychological concepts follow such usage, they are less likely to follow strict logical limits than to feature the variability and ambiguity of prototypes.[2] In contrast, in their reply to our paper (Hacker and Bennett (H&B), 2011) liken the definition of psychological concepts to using a nominal definition of *vixen* as a female fox. In a psychological example, H&B invoke "the word 'conscious' and its cognates" (Hacker and Bennett, in press). Rejecting our scepticism, they take issue with our claim that they *posit* this view. Rather, they say that "We *took it for granted* that we *all* know how to use the word 'conscious; and its cognates – for that is all that is necessary for the clarification of *the concept of consciousness*". Given their faith that this is known to competent speakers, they fail to understand either our scepticism or our challenge. Since we do not think that conceptual analysis alone can provide foundations for neuroscience, we contrast this with our 'coherentist' view. Accordingly, we stress the value of conceptual analysis in making informative use of empirical work. Defending this in relation to examples, we turned to the cases of 'blind-sight' and 'distraction-from-pain' used in PFN. In developing our argument, we return to these cases below. In our view, the fact that we can discuss such concepts supports neuroscientist Changeux and hermeneutic philosopher Ricoeur's view that *semantic tolerance* can supplement semantic *critique* (Changeux and Ricoeur, 2000). Conversely, we deem B&H's method of limited value for empirical investigation because it is rule-bound. We are unimpressed by a work that, in a colourful expression, amounts to constraining "draughts players by pointing out that there is no checkmate in draughts" (Hacker and Bennett, in press) for, in our view, the analogy does not apply to neuroscience.

There are big contrasts between neuroscience, neuroscientific writings and games like draughts. In investigating how people draw on brains, neuroscience is developing both methods and also new applications of established concepts. In spite of PFN's limited attention to certain findings and methods, our examples of concepts such as *blind-sight* and *perception* show that they are already used flexibly within and between individuals and populations. Conceptual analysis can not only critique explanatory work but also buttress and refine findings. Using mechanistic explanations (as discussed below), we trace conceptual divergences to variability of psychological functions that are constituted as they draw on overlapping and interfering neural networks. In so doing, we reiterate out agreement with B&H's view of mereological reasoning, i.e. with their critique of describing parts of a system (or brain) as carrying out the functions of a whole person. In giving reasons for this, however, we argue that concepts can also serve as heuristics.

1.1. Yardsticks and heuristics: an overview

Seen as yardsticks, concepts serve in taking the measure of various claims. On our coherentist view, this is entirely compatible with *also* using them as heuristics that serve to refine thinking. Especially in cognitive neuroscience, both psychological functions and their concepts display historical and cultural variability (Henrich et al., 2010; Lloyd, 2007).[3] Before pursuing this argument, we respond to key points in H&B's critique. First, we return to Aristotle's work on foundational concepts to suggest that neuroscience is the kind of field that can alter conceptual resources. Progress can arise from, not dismissing, but considering the conceptual entanglements and anomalies that are symptomatic of variability and divergences in psychological concepts. Far from being entirely reliant on nominal definitions, these must be seen as having some continuity with their explanatory counterparts.

Having reiterated contrasts between our view of concepts and that proposed in PFN, we turn to the interdependency of action and perception. Beginning with H&B's response to our reflections on their view of *perception*, we stress that interdependency affects the explanatory definitions of both everyday life and science. Applying this approach to PFN's discussion of blind-sight, we compare such phenomena with ordinary cases. This reopens our charge that, given B&H's view of semantic innovation, PFN exemplifies *conceptual conservatism*. In this context, we extend our view of innovation by showing how

[1] We noted that PFN leaves aside discussion of methods such as neuroimaging and lesion studies and remarked that, for B&H, "alongside logical analysis, these are 'minor issues' (Keestra and Cowley, 2009, p. 535). Having objected to what they term our *sneer-quotes*, they pose a rhetorical question (cited in 2.0 below) to imply that such topics have little to do with investigations of psychological concepts used by cognitive neuroscientists. Challenging this view, we argue that neuroscientific results *can* help to disambiguate conceptual unclarities.

[2] Research shows that neither human nor animal category learning can be explained exclusively by rule formation. Humans and animals learn categories by using rules and by forming prototypes (Ashby and Ell, 2001). Obviously, categories or concepts based upon a prototype allow for more flexibility in acquisition and use than do those that are dependent upon a particular rule.

[3] Research challenges the presence of conceptual consensus or consensus about behavioural criteria in the application of psychological concepts. This holds, naturally enough, for transcultural differences in not only psychological and psychiatric concepts but also their behavioural expression (Chaturvedi and Bhugra, 2007). Recent debate emphasizes intercultural variability in describing the processes and functions that link perception, motivation and cognition to behavior (Arnett, 2008; Henrich, et al., 2010). It would be equally mistaken to conclude that, within a group, there is prevailing consensus. We therefore seek coherence between sources of insights in order to limit the impact of its absence.

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conceptual anomalies can be used to overcome worries about what Kuhn called the 'incommensurability' of scientific traditions. Thus, while endorsing the value of conceptual critique, we claim that much can be learned by using conceptual ambiguities as heuristics.

Finally, we pinpoint where our view coincides with PFN's. We appreciate B&H's discussion of the persistent mereological fallacy in neuroscience and concur that it is an error to ascribe psychological functions to, not a person, but a brain or its parts. Rather than dismiss this as nonsense, we also consider why, at least sometimes, the error arises. We use this to build a case for a mechanistic model of explanation that deals with complexity *without* positing that psychological predicates identify the function of individual components or operations within a mechanism that, as a whole, produces a psychological phenomenon. From this perspective too, we stress, just as do B&H, that neuroscience has much to gain from a vocabulary that does not invoke the psychological domain when scientists refer, not to persons, but to brains or neurons.[4]

2. Modesty in clarifying conceptual networks

In spite of H&B's complaints, we fully appreciate that in *Philosophical Foundations of Neuroscience* they "delineated the conceptual network formed by families of psychological concepts" (Bennett and Hacker, 2007, p. 128). The problem, we think, is what they fail to observe. Though we understand their goal, we claim that, in cognitive neuroscience, clear and comprehensive distinctions between sense and nonsense *cannot* rely solely on analysis of the "conceptual foundations of *cognitive* neuroscience – foundations constituted by the structural relationships among the psychological concepts involved in investigations into the neural underpinnings of human cognitive, affective, and volitional capacities" (Bennett and Hacker, 2003, p. 1, italics in original). Whereas the authors compare their project with the role of mathematics for natural science, we contrast analysis of psychological concepts with making use of mathematical logic. Neuroscience cannot be measured by a yardstick based on analytical philosophy's "*description of our conceptual scheme*" (Bennett and Hacker, 2003, p. 439, italics in original). Of course, PFN goes beyond conceptual analysis: B&H acknowledge that part of the meaning of psychological concepts draws on other components. In their terms: "[t]he criterial grounds for ascribing psychological predicates to another person are *conceptually* connected with the psychological attribute in question. They are partly constitutive of the *meaning* of the predicate" (Bennett and Hacker, 2003, p. 83). While this captures behavioural criteria, we are sceptical, first, that psychological predicates (involving, say, *conscious* and cognate forms), as used in any community, feature enough consensus to yield the comprehensive and strict delineations of a "logical space within which facts are located" (Bennett and Hacker, 2007, p. 129).[5] Second, we are equally doubtful that the relevant behavioural criteria can be characterised without variability and divergence. We therefore challenge a method that assumes just such an agreement and, for this reason, overlooks subtleties of usage, how neuroscience can prompt conceptual change, and evidence that this is beginning to occur.

We emphasize that there can be no separation of pure conceptual analysis from proposed explanatory accounts. Our alleged failure to understand PFN arises because we view the analytical method as insufficient for removing conceptual confusion. Indeed, given its evidential basis, there is always a risk of lumping separate phenomena together or, indeed, imposing distinctions on phenomena with an underlying unity. Though we endorse the importance of conceptual analysis, we do not see this as establishing foundations that, in themselves, serve to evaluate empirical work. In terms of our previous paper, we regret that "B&H overlook the Aristotelian view that a field may be unready or unsuited to systematization based on talk about human faculties" (Keestra and Cowley, 2009, p. 533). On this view, variability is allowed and no taken for granted consensus grounds conceptual analysis. At least in part, it is an empirical matter when and how we are to use analysis of psychological concepts.[6] This is not to deny that the study of the brain may benefit from aiming at the development of comprehensive, consistent and, above all, coherent conceptual networks. However, the definition and classification of newly discovered and complex phenomena may develop gradually. There are, moreover, reasons for which biological and cognitive domains resist conceptual disentanglement. In contrast with, say, chemistry, biological and behavioural classifications reveal divergence and lack of unity (Dupré, 2001). Their very multi-causality requires systematization based on a variety of criteria or distinctions. Indeed, appeal to natural kind concepts can lead to ill-founded assumptions about a domain's contents, its definability and the uniqueness of its corresponding definitions (Hacking, 1991). By extension, it would be ill-advised to expect consensus on defining features of psychological concepts. Far from invalidating conceptual analysis, this means that the results can be used both critically and with heuristic value. Reliance on conceptual analysis goes astray only if its outcomes are treated as yardsticks that define any deviance as nonsensical. Indeed, where dispute arises, neuroscientists can often gain from empirical work that sets out to scrutinise both anomalous and normal cases.

Recognition of biological complexity underpins our challenge to the view that, in PFN's sense, neuroscience has conceptual foundations. In spite of intimations to the contrary (Hacker and Bennett, in press) we neither think nor say that

[4] A different case of mereological reasoning emerges when psychological predicates are applied, not to parts of a person, but to a group of persons (as in collective action or collective memory). If, as we think, such descriptions are valuable, this confirms both the worth of mereological reasoning and the need for semantic tolerance.

[5] Debates on matters such as euthanasia, abortion and animal rights testify to not just ethical differences, but also disagreements about concepts like *consciousness*. Though some forms of non-sense can be avoided, we deny that definitions of being conscious and its cognates permit mathematical-like precision.

[6] The absence of empirical support for their strict delineations of the meaning of concepts leads Sytsma to call the method of PFN 'anti-empirical conceptual analysis'. He shows that the authors overestimate the representativeness of their intuitions in that, for example, many respondents do apply 'calculate' to computers – though B&H reject this as nonsensical (Sytsma, 2010).

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the writers of PFN aim to deduce hypotheses or theories from conceptual foundations.[7] Rather, we remind the authors that Aristotle neither used foundations deductively nor in performing investigations. His foundationalist method probably served only didactically or for the systematic presentation of already gathered results (Barnes, 1969). Echoing this, we say: "Neuroscience may be too young to draw on 'already existing knowledge'. First, we still lack clear and complete accounts of relevant empirical findings. Second, we do not find it obvious *which* foundational concepts or definitions (if any) would shape such work" (Keestra and Cowley, 2009, p. 534). Our remarks stress that psychological phenomena and concepts show variability which will undermine appeal to conceptual foundations that are said to build on the consensus of an (unspecified) community.

We therefore argue that, at times, explanatory features are to be included in the work of determining concepts. Logically, therefore, conceptual analysis will gradually have to be integrated with how we understand its neural underpinnings. Parallels between analysis of psychological concepts and definitions such as: "an animal can be said to be a vixen if and only if it is a female fox" (Bennett and Hacker, 2007, p. 147; Hacker and Bennett, in press, p. 461) are likely to be limited. Given variability and lack of consensus, nominal definitions throw little light on how human beings refer to psychological phenomena. We are confident that Aristotle would agree with us because his psychological definitions link bodily and behavioural factors. Anger, for example, is defined both as "a craving for retaliation and "a surging of the blood and heat round the heart" (De Anima, 403 a31–b1). To the extent that a definition can disambiguate, the example speaks for careful inclusion of components which draw on functional and causal explanation.[8]

Since we find continuity between nominal and explanatory definitions, we advocate linking conceptual analysis with explanatory models and investigative methods such as neuroimaging and use of animal models. Thus, we regret that PFN overlooks such issues. In response, H&B wonder what "these topics have to do with our investigations into the salient psychological concepts" (Hacker and Bennett, in press, p. 460). In so doing, they manifestly fail to answer to our proposal to use mechanistic explanation (Bechtel, 2008), as is common practice in the life and cognitive sciences. Further, as shown below, many of PFN's challenges to neuroscientific writings are entirely *consistent* with a mechanistic approach. Advantages accrue, we claim, from linking explanatory components to analysis of psychological concepts. First, every community of speakers will feature both dissension and divergences. Second, instead of denying conceptual variability, this can drive investigation of complexity. Third, inclusion of explanatory components in definitions allows conceptual divergence to be used in positing *mechanisms*[9] that shape the variable phenomena identified by a concept. This use of conceptual divergence is thus reason to contrast the semantic conservatism of PFN with the semantic tolerance and criticism defended by Ricoeur and Changeux (Changeux and Ricoeur, 2000). Given that H&B pay no heed to our view, we return to the example of *perception* to take the argument further.

2.1. *Action–perception interaction and conceptual anomaly*

It is central to our project that "perception and action are behaviourally, structurally and neurally interdependent" (Keestra and Cowley, 2009, p. 544). This, we argue, has important consequences for psychological concepts. The empirical work with which we illustrate this finding about neural systems applies, among other things, to language (Willems and Hagoort, 2007), to neural coding of action and perception (Hommel et al., 2001; Prinz, 1997), to representations of action (Jeannerod, 2003), and to enactive views of perception (Noë, 2004). There is more than ample neural evidence that perception is partly determined by the specific action in which we are engaged and, conversely, that the action is guided by ongoing perception. It is this interdependence that grounds the conceptual inconsistency and anomalies about which, where PFN is not silent, B&H invoke what we regard as a quite unsatisfactory view of *perception*.[10]

In their response, H&B offer quotes showing that they do account for "the relation of perception to activity and passivity" (Hacker and Bennett, in press, p. 462). Illustrating this, they refer to subjects who "try to perceive and attempt to discern better" (ibid.), to the fact that "one can be more or less skilled at perceiving" and they contrast this with a situation where "[o]ne cannot choose to hear a loud noise in one's vicinity" which refers to the non-voluntarily and passive character of perception (ibid.). Further, they quote their observation that "[p]ossession of a sense-faculty is manifest in behaviour" and conclude that our criticisms are "egregious" and that "K&C could not understand what they read" (Hacker and Bennett, in press, p. 463). Then, as now, we are dissatisfied because, from our perspective, H&B's observations are largely beside the point. Their unsurprising observations overlook the *interdependency* of action and perception. This, we argue, contributes to

[7] Our use of the adjective 'strict' in connection with foundationalism caused confusion. In line with our arguments for linking conceptual analysis with explanatory models, we advocated, "abandoning strict foundationalism in order to make space for work that depends on semantic tolerance" (Keestra and Cowley, 2009, p. 543). Further, in discussing the concept of foundationalism, we failed to say that the foundations invoked in PFN are not associated with the deductive use of conceptual foundations. Had we done so, there would have been less chance that readers would have applied our didactic exposition to PFN.

[8] Aristotle's *Posterior Analytics* marks a transition from nominal to explanatory or causal definitions even if, in nature, these are inseparable (Charles, 2000; Demoss and Devereux, 1988). For Aristotle, explanatory pluralism renders definition of biological functions and properties quite unlike defining mathematical objects (Gotthelf, 1997). Like Aristotle, we think that this also holds for psychological functions which vary both in different kinds and within an individual. Thus bodily aspects are needed in analysis, description and explanation of psychological functions (van der Eijk, 1997).

[9] We follow Bechtel (2008) in seeking out – not primarily explanatory laws – but biological structures that perform functions in virtue of the operation of component parts, component operations, and their organization. Taken together, these are responsible for one or more phenomena.

[10] We quoted the observation that for such reasons, "the traditional divisions among perception, cognition, and action look increasingly unhelpful" (Clark, 1997, p. 221).

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the observable variability by showing that, at times, explanatory components are needed in defining functions.[11] For example, consider wearing inverting goggles. As is well known, with practice, one can cycle wearing such instruments and, later, re-adapt to goggle-free life. Importantly, re-adaptation is achieved, "only by individuals who *actively* interact with their environments" (Noë, 2004, p. 92, italics in original). Citing Taylor (1962), Noë points out that having regained the ability to cycle normally, for example, subjects may continue to see the writing on shop windows as inverted. Normal vision can be restored by active cycling that fails to bring back normal reading. Given the specifics of the example, with Noë, we regard this action–perception interdependence as crucial to understanding the concepts of both *perception* and *action*. Similar claims can be made in relation to, for example, subjects' cultural differences in perceptual discrimination that is tacitly manifest in behavioural responses (Nisbett and Miyamoto, 2005). Given a wide range of examples, we argue for a conceptual framework that presupposes neither consensus, a hypothetical community, nor uniform behavioural criteria. Such an approach offers much to neuroscience precisely because it tolerates (and highlights) inconsistencies and incoherencies.

We discussed another case of action-perception interdependence which, in our view, has conceptual consequences. In this, a patient with a damaged V1 area is said to show 'blind-sight' when behaviourally demonstrating 'good visual discrimination capacity in the absence of acknowledged experience' (Weiskrantz, 1997, p. 19). We claim that this shows how, at least in part, mechanisms that contribute to the functions overlap or interfere in ways that disturb a common view of perception (by giving surprising results). Thus, judged by the evidential criteria normally used in discussion of *sight,* the patient's behaviour is anomalous. In PFN, it is suggested that this disallows the concept – 'blind-sight' – from capturing what is observed. It is said that, in this patient "the normal convergence of indices of sight – namely, appropriate affective response, behavioural reaction, reoriented movement, verbal description, answers to appropriate questions, etc. – is subtly disrupted." While we agree with this, we pause when the authors continue, "[b]ut such convergences constitute the framework within which verbs of vision are taught and used. (. . .) The consequence of a conflict of criteria is that one can neither say that the patient sees objects within the scotoma nor say that he does not." We cannot agree when they conclude that this "indicates the *inapplicability* of a concept under special circumstance" (Bennett and Hacker, 2003, p. 396, italics not in original). Our view is more nuanced: we endorse the ambiguity because, by maintaining the concept of 'sight' (and its normal indices), we pick out the exceptional nature of blind-sight. On our view, much is gained in applying '*sight*', not as a yardstick, but as a heuristic. Whereas rejection of the term would slow the prospects for a neuroscience of vision, its use raises questions about what can be learned from such an exceptional yet striking form of action–perception interdependence.

Regarding normal and non-pathological cases we agree with Noë, that the "ability to perceive not only depends on, but is constituted by, . . . sensorimotor knowledge" (Noë, 2004, p. 2). As a consequence, competent language users form a concept of *sight* that is not fully bounded. As is well known, in reflecting on 'game', Wittgenstein can be read to propose a related view: "We do not know the boundaries because none have been drawn. To repeat, we can draw a boundary—for a special purpose. Does it take that to make the concept usable? Not at all. (Except for that special purpose.)" (Wittgenstein, 1974, p. 69).[12] Neuroscientists can therefore pursue their purposes by noting loosely bounded concepts and, of course, testing cases with blurred boundaries. Their research can illuminate pathological cases and, at times, challenge established views: used heuristically, variability and dissension can drive investigation.

In our article we note that, when surprising phenomena defy a conceptual framework, B&H often treat these anomalies as singularities or as exemplifying a limiting case. For example, competent language users often discuss finding attention distracted from pain. We used this thought-provoking, yet non-pathological phenomenon (mentioned in a footnote of PFN) to which, regretfully, H&B do not respond. Thus, we reiterate that the case defies their assertion that "there is no difference between *having* a sensation and *feeling* a sensation." We deem it unsatisfactory to dub this a "curious anomaly" which "can be viewed as a singularity (in the mathematical sense) in the grammar of sensation" (Bennett and Hacker, 2003, p. 121, footnote 2). In this context, we contrast mathematics with B&H's mode of conceptual analysis. A singularity will arise, for example, if a mathematical function touches (0, 0) in a Cartesian co-ordinate system. First, this raises the question of whether the authors truly believe that for competent language users 'distraction from pain' resembles such a singularity. Second, we wonder if anything in psychology could be analogous to touching (0, 0) in Cartesian co-ordinates.[13] It is our view that, while mathematical foundations can be used in a consistent and comprehensive way to *reconstruct* or *generate* a strictly defined logical space of mathematical objects, there is no parallel space for psychological phenomena. Analysis of psychological concepts can only facilitate systematic *reconstruction* that limits, but does not eliminate, the incompleteness, inconsistency and vagueness of natural languages. Since we reject the analogy with mathematics, we argue that psychological anomalies can be pursued in ways that lead to innovation in the conceptual network. Indeed, the mismatch between having and feeling a sensation exemplifies the blurred conceptual borders, hiatuses, and overlaps that characterize the incomprehensive classificatory systems of natural languages. As argued below, the phenomenon and its description become heuristics in studying neural mechanisms that are recruited for pain and for attention – which can indeed be shown to interact (Valet et al., 2004). One

[11] In our paper, we show that the neural underpinnings of language overlap with those of action and perception. H&B, by contrast, treat language as mapping onto public verbal patterns and overlook such issues (they do, however, deny that they have a 'theory of language'). Building on the claim that there are no *a priori* linguistic units (e.g. Harris, 1998), a growing community trace language to how we co-ordinate action and perception (see, Cowley, 2007, 2009) and thus regard it as irreducible to its verbal aspect. In this context, we leave such issues aside.

[12] Stokhof diagnoses in recent Wittgenstein interpretations a 'quest for purity', where Wittgensteinian philosophers seek strict autonomy against science and argues that Wittgenstein himself had a more nuanced position (Stokhof, 2010). We thank Martin Stokhof for discussing this issue with one of us.

[13] Of course the authors may have some other mathematical singularity in mind. If this is the case, it would be interesting to know what this was.

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advantage of conceptual analysis is precisely that it can reveal atypical cases which, linked to empirical work, bring important issues to the fore.

For PFN, anomalies reveal nonsense because they violate consensual standards of concept use. Taking the consistent and coherent standards for granted, they suggest that "meanings of words are determined by their rule-governed use, and they are given by what are accepted as correct explanations of meaning by the community of speakers" (Bennett and Hacker, 2003, p. 382). This echoes Kuhn's view of normal science as performed by a community of scientists who share a paradigm concerning the problems, rules, and methods of that science. Its relevance lies in that Kuhn, too, focused on innovation while diagnosing conservatism. He stressed scientists' difficulties with unexpected novelty or anomalies – often of a dual, factual and theoretical nature – that challenged an accepted but constraining paradigm (Kuhn, 1970). Indeed, this parallel raises the troublesome thesis of the *incommensurability* of scientific traditions. This bears on H&B's Section 2, their methodology, and our charge of conceptual conservatism.

2.2. Innovation and pluralism in cognitive neuroscience

Action–perception interdependence brings home differences between nominal definitions of terms like *vixen* with how conceptual ambiguities or unclarities relate to phenomena like blind-sight. Further, given our scepticism about semantic consensus, we stress that conceptual networks vary and diverge both over time, between cultures and both across and within individuals. It is because B&H take for granted a consensus of people deemed 'competent' in recognizing the delineations of logical space that we diagnose the project of PFN as implying a form of *conceptual conservatism*. As H&B recognize, we are not alone in raising this objection and, perhaps for that reason, they offer a more substantial answer.[14] Pursuing our challenge, we note Kuhn's observation that paradigms inevitably "restrict the phenomenological field accessible for scientific investigation at any given time" (Kuhn, 1970, p. 60). When these are superseded, this often depends on unexpected and surprising anomalies that had been seen as external to the field. Famous anomalies led, for example, to rejection of Ptolemeian cosmology by Copernicus. The result of a scientific revolution is, Kuhn argues, not paradigm adjustment but, rather, the rise of a novel scientific tradition. Often, Kuhn affirms, this "is not only incompatible but often actually incommensurable with that which has gone before" (Kuhn, 1970, p. 103). Equipped with a new set of theories, concepts, assumptions and standards, the new framework lacks the ingredients which are needed to translate claims between new and old traditions. With incommensurability, however, comparison between traditions becomes difficult. This is problematic if, like Kuhn, one accepts the view that science brings about a steady growth of knowledge.[15] As a result, even the modest goal of describing and explaining scientific progress depends on limiting room for incommensurability.

Kuhn's problem supports our charge that PFN's view of conceptual foundations sustains an unmerited conservatism. In comparison with astronomy's relative simplicity, the complex and dynamic phenomena studied in the life sciences give rise to a causal and theoretical pluralism that sustains scientific debate (Beatty, 1997). This requires semantic criticism associated with tolerance. Protesting, H&B note that, while we admit that B&H *deny* conceptual conservatism, we ascribe precisely this problem to their work. Overlooking the contrast between their view and our case for conceptual flexibility, they cite our move as an example of "the quality of the reasoning in K&C's discussion of our book" (Hacker and Bennett, in press, p. 461). Once again, using colourful rhetoric, they step over our arguments. To be sure, if one accepts that psychological concepts depend on rules like those involved in nominal definitions of *bachelor* or *vixen*, and if one assumes that a comprehensive and consistent set of such rules represent the consensus of an assumed community of competent speakers on psychological concepts, semantic innovation or adaptation is challenging. In PFN, it is said to set off modifications across the conceptual framework and, therefore, the community. Such conceptual adjustments require modification of "the logical space within which facts are located" (Bennett and Hacker, 2007, p. 129). This is said to lead to dislocation of already gathered facts compared with those found in subsequent research: "Of course, if this were done, the constituent words of these phrases would no longer have the same meaning as they have now. So neuroscientists would *not* be investigating the neural conditions of thinking, believing, perceiving and remembering at all, but *rather those of something else*, which is as yet undefined and undetermined. But this is patently not what neuroscientists wish to do" (Bennett and Hacker, 2003, p. 384 – quoted at Keestra and Cowley, 2009, p. 540, italics added). Just as Ptolemeian and Copernican cosmologists are said to be unable to exchange scientific results and insights, this claim would lead to a situation where neuroscientists who investigate the odd phenomenon of "blind-sight" would focus on a phenomenon that is quite different from that understood by those who contribute to the alleged consensus about *perceiving*. Given our view on concepts, we consider this unwarranted, unnecessary, and undesirably restrictive.

We link their view of conceptual foundations with conservatism: "There are dangers in clinging to logico-grammatical restrictions" because "at times, empirical findings can be used to make conceptual adjustments to a phenomenon under

[14] They stress that they "lay down no restriction on linguistic innovation whatsoever" (Hacker and Bennett, 2011, p. 461). However, this does not address our scepticism. By taking a community consensus for granted, they grant limited space for divergence and variability of opinions. Instead of promoting semantic tolerance, they assume a strict separation of sense and non-sense.

[15] In his *Postscript*, Kuhn urges that incommensurability be seen as a matter of "different language communities and that their communication problems be analyzed as problems of translation" (Kuhn, 1970, p. 175). This view of the problem of translation stems from Quine; according to PFN (cf. Bennett and Hacker, 2003, p. 130) this had a strong yet misleading influence on cognitive scientists. In our view, this has no bearing on our discussion of the problem of incommensurability.

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investigation, without implying that a totally different phenomenon is at stake – as B&H proclaim (p. 384)" (Keestra and Cowley, 2009, p. 543).[16] Rather than take for granted that there is an extant consensus that offers conceptual foundations to neuroscience, we stand by our view that dynamic mechanisms like human brains give rise to phenomena whose description brings forth conceptual ambiguities. We deem the PFN position *conservative* in comparison to one that values semantic tolerance. While not a radical approach, ours posits no strict division between conceptual analysis and empirical work.[17] In line with a hermeneutic approach, it favours innovation by ascribing a more modest role to conceptual analysis (Fleming, 1990).[18] Invoking coherentism, we argue that psychological research be continuously and reciprocally guided by both conceptual work and empirical analysis. On such a view, scientists, philosophers and other scholars can make use of the ambiguity and indeterminacy of psychological concepts in seeking to develop new ways of explaining psychological functions.

Though pleading for semantic tolerance we also argue that much of the criticism in PFN is merited. If, having read only this part of our dialogue, the reader thinks that our positions are totally opposed, this would be mistaken. In closing our paper, therefore, we reiterate what we particularly applaud in PFN. This too may have eluded H&B or, perhaps, confused them. For, while endorsing their view of the mereological fallacy, our reasons are not the same as theirs.

3. Correcting mereological fallacies and explaining them

Since the 1960s many linguistic theories have proposed models of how brains process, generate and produce utterances, sentences and discourse. Taken literally, they often fall foul of mereological fallacies by suggesting that how we speak, write or understand depends on, not persons in environments, but neurons or brains. In addressing linguists and others wishing to learn from cognitive neuroscience, this was one reason for giving attention to PFN. Accordingly, we applauded B&H's view of mereological reasoning: "[a]lthough they ignore the diversity of mereology, the critique is valuable. Indeed, they show that – at times – neuroscientists treat groups of cells as carrying out the activities of a whole person (or organism)" (Keestra and Cowley, 2009, pp. 536–537). PFN offers a 'mereological principle' which states that: "psychological predicates which apply only to human beings (or other animals) as wholes cannot intelligibly be applied to their parts, such as the brain" (Bennett and Hacker, 2003, p. 73). In arguing against such a fallacy, we appeal, not to conceptual foundations, but to explanatory endeavours that are based in semantic tolerance. Not being troubled by conceptual ambiguities, singularities or anomalies *per se*, we advocate using the phenomena that such concepts identify in seeking out explanatory mechanisms. On this view, the kind of conceptual analysis offered in PFN is important to the *initial* identification of relevant phenomena.[19]

Mechanistic explanation of a psychological phenomenon depends, in the first place, on decomposing the phenomenon by analysing the concepts that describe it. Such a decomposition is a preliminary guide – a heuristic – to empirical investigations of what can be observed. The subsequent stage is to localize components that constitute the mechanisms responsible for the phenomenon somewhere in the organism (Bechtel and Richardson, 1993). For instance, memory can be decomposed into components such as semantic, procedural, episodic and short term memory. After decomposition, various investigative techniques are needed to identify and localize components and operations that constitute the neural mechanisms that subserve memory (Craver, 2007). Thus, it is important to include conceptual anomalies and singularities precisely because they can show how one mechanism interferes with another or, perhaps, responds in specific conditions – testifying to causal pluralism.[20] For example, given interdependent neural systems, pain may be affected by attention. Thus, just as conceptual analysis acts as a heuristic to develop explanations of phenomena, an explanation can clarify or supersede conceptual anomalies. The argument applies in many domains. For example, in the next edition of the DSM, no clear line will be drawn between normal and pathological conditions. Given the complexity of etiology and underlying biology, it will be recognized that quantitative or dimensional traits characterize both kinds of condition which are, therefore, continuous (Hyman, 2007). This again attests how explanatory insights promote cautious use of classificatory innovation.

The mereological principle remains. No psychological predicate should be applied to the function of a particular component or operation within an explanatory mechanism that as a whole is responsible for the psychological phenomenon. To be sure some may speak otherwise. For example, there are extraordinary cases where Deep Brain Stimulation of locations in the basal ganglia circuitry can alleviate symptoms of pathologies such as obsessive–compulsive disorder, Tourette's syndrome,

[16] Subsequently we discussed synaesthesia. While accepting that, on the accepted definition of number, it "needs no science to tell us that it is senseless to ascribe colours to numbers" (Bennett and Hacker, 2003, p. 133), we referred to research confirming that synaesthesia points to uncommon cross-wiring in neural mechanisms that are usually devoted to different processing domains. Another case that shows the difficulty of strictly defining psychological concepts is that of having an emotion without knowing what its object is. B&H call this a 'limiting case' in that the rule is said to be that emotions have a definite object (ibid. 219), whereas existential philosophers assess a limiting case like "Angst" as central to human existence. Once again, this raises doubts about the dimensions of any community of competent speakers.

[17] Semantic tolerance regarding concepts like 'gene' has not impeded scientific progress. Rather, it has lead to a situation where it is prudent to ask if the concept conflates two distinct phenomena, as is evident from their causal structure (Stotz and Griffiths, 2004). This is no purely conceptual matter in that it involves the evaluation and estimation of causal processes.

[18] An integration of insights from cognitive science and from hermeneutics with respect to the phenomenon of imitation is presented in (Keestra, 2008).

[19] It is far from the case that we do not appreciate B&H's efforts to introduce "new classificatory terminology in order to render a conceptual domain more surveyable" (Hacker and Bennett, 2011, p. 461) or that we view them as crypto doctrinaires. Rather, we regret their failure to address what alternative, elaborate classifications of, say, *consciousness* imply for their views.

[20] This pluralism is also responsible for the fact that most psychological phenomena do not conform to universal laws like those of physics but, rather, feature exceptions. Obviously, this implies neither that psychology knows no – statistical – laws at all, nor that neuroscience cannot explain both lawlike phenomena and their exceptions, as B&H appear to suggest at (Bennett and Hacker, 2003, pp. 362–364),

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or depression (Ward et al., 2010). We agree that the targeted neural areas contribute to a person's behaviour. On the mechanistic view, they perform intricate neurophysiological operations as components of larger neural mechanisms – to which our psychological vocabulary does not apply directly. Indeed, as there is new consensus that neural areas are often 'recycled' (Dehaene, 2005), 'exploited' (Gallese, 2009) or 'redeployed' (Anderson, 2007) by different psychological functions, it is misleading to refer to any such area by using any established psychological predicate. Together with connective analysis, this prompts (Anderson, 2007) to argue that we need to develop a 'domain-neutral' vocabulary. In other words, our psychological concepts are ill-suited to describing neural components and operations.

Conversely, the mechanisms that produce psychological phenomena are so enmeshed with each other that the meaningful use of psychological predicates can only be applied to the person – just as argued in PFN. Our views diverge because we claim that observing and communicating with persons at different times and places allows us to embrace the variability and divergence of both psychological phenomena and the concepts that influence interactions. Indeed, concepts themselves can contribute to human self-understanding and behaviour – in Hacking's (1999) terms, they serve as 'interactive kinds'. A similar influence will emanate from neuroscientific explanations. This again contributes to the variability of phenomena and the concepts used to refer to them. No wonder, for example, that what it is to be *conscious* seems to defy description in terms of a well defined logical space associated with the consensus of competent speakers. While we doubt that, outside mathematics, any such conceptual space could arise, we recognize the vital role of conceptual analysis in bootstrapping explanatory endeavours. Though we recognize fundamental disagreements – and some agreements – we are grateful to B&H's challenge for allowing us to develop our argument more fully. We end, therefore, by echoing Aristotle's observation that not all forms of knowledge permit equal exactitude, mathematics being, not representative, but exceptional (cf. Metaph. 982 a 25 ff.). We endorse this prudence – even if, it seems, some would still like things to be otherwise.

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