

ANALYTIC, A PRIORI, FALSE-AND MAYBE NON-CONCEPTUAL*

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ABSTRACT

I argue that there are analytic claims that, if true, can be known a priori, but which also can turn out to be false: they are expressive of merely default instructions from the language faculty to the conceptual system, which may be overridden by pragmatic or scientific considerations, in which case, of course, they would not be known at all, a priori or otherwise. More surprisingly, I also argue that they might not be, strictly speaking, conceptual: concepts may be importantly different from the meaning instructions for the words we use to express them. I will press all this in the context of a general Quinean “naturalism,” where the epistemology that interests me is “a chapter of natural science,” but where the science won't be Quine's behaviorism, but a Chomskyan theory of the “I-semantics” of “I-language.” But, relying on a distinction I draw between an explanatory and a working epistemology, I will be pressing it largely as an explanatory claim, not one that will have serious consequences for on-going philosophical practice, neither with regard to the world, nor, more surprisingly, even with regard to armchair “conceptual analysis.” As Putnam (1965/75) observed, there may be analytic truths, but they don't cut much philosophical ice.

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There could be few philosophers more deserving of a *Festschrift* than Nenad. I first met him in 1984 when I visited the University of Split at Zadar on a Fulbright. I had gone there partly out of curiosity, and partly as a result of my predecessor, Michael Detlefsen's, enthusiastic description of this brilliant, energetic, virtually self-taught analytic philosopher who was, almost single-handedly, transforming Croatian philosophy from its largely Heideggerian focus and re-directing it to analytical philosophy of language, mind, logic and mathematics. I wasn't

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disappointed. Within minutes of my arrival Nenad began racing through the books I had brought, quizzing me about what they contained and why it was important – well, we were off and running, and haven't stopped since. The mix of energy, intelligence and abundant friendliness has always been a joy, and has brought me back to Croatia and Slovenia continuously since then. I'm sure his terrific effect on Croatian philosophy will be manifest elsewhere in this volume, but I'd like to mention one anecdote that testifies to it. Some years ago I served as a referee for the European Society for Analytic Philosophy, and, after the quite 'blind' results were in, puzzlement was expressed about how so many Croatians had come to be ranked so highly. I smiled and murmured, "Nenad" (which I hope neither he nor readers here will mind me using to refer to him here). OK, to work:

1. One Shock Deserves Another

In his (2006), "Devitt's Shocking Idea and Analyticity without *A Priority*," what Nenad finds shocking is what Devitt (2001) himself presents as a "shocking idea" that:

the meanings of some words, including names and natural kind words, are causal modes of reference that are partly external to the head. (Devitt 2001, 477)

Among other things, Nenad finds this notion of content "so austere it has little role to play in psychological explanation" (2006, 76), and so he tries to find a more, as he puts it, "Frutnumian" (74) approach that tries to combine some version of a Fregean theory of "senses," or "ways of thinking of something," with the causal insights of Putnam, Kripke and Devitt.

Now, as a student of the "middle" (ca. 1960-75) Putnam, I was initially sympathetic to Devitt's view myself. Causal relations between (kinds of) things in the world and our uses of terms certainly seem to play an important role in determining the reference of our terms, especially when we are concerned with their use in science, where it is a *posteriori* research that likely serves best in telling us about the nature of natural kinds that exist independently of us (a point to which I shall return). We don't want the reference of a term to be at the mercy of, as Devitt (1996) nicely summarizes the issues, our "ignorance and error." But I share some of Nenad's worry about the lack of a serious psychological role for senses construed as merely causal chains.¹

¹ Which is not to say that Devitt and Fodor don't allow for all manner of psychological links in their various causal chains. The problem for Nenad and myself, and, I suspect, many others, is that *specific* psychological/conceptual links aren't *constitutive* of the content of the caused symbol in ways that many of us think would be required in order for that content to enter into psychological explanations.

There needs to be *something* more specifically “cognitive” than either Devitt’s causal histories, or Fodor’s (1991) purely referential “asymmetric dependencies” and (2008) “perceptual triangulations” seem to allow, both for reference to be adequately determined, but, as I would agree with Nenad insisting, in order for “content” to play a serious role in psychological explanation. As I have argued elsewhere (e.g. Rey 2005; 2009), reflection on the ubiquity of “empty concepts” (e.g. *ghost*, *triangle*, *phoneme*) and “response dependent” ones (e.g. *red*, *funny*, *immoral*), as well as simply the peculiar robustness of “analytic intuitions” (not only “Bachelors are unmarried,” but “If John is killed, then John is dead,” “If John brought Bill to the party, Bill went to the party”) seems to me to show that external reference is clearly inadequate as a theory of their content. One needs, I argue, to “go inside” not merely the skulls, but the minds of thinkers.

But in order to do this, one still does need to allow for the ignorance and error that worries Devitt, and this has seemed to many to be incompatible with any internal, mental condition on meaning. What I will do here is provide Nenad with another shock, moreover, one, unlike Devitt’s, from *within* a Frutnamian approach, and one which even allows for the very *a priori* of the analytic that Devitt explicitly rejected. I want to argue that there are analytic claims that, *if true, can* be known *a priori*, but which (here Nenad should perhaps be sitting down) also can turn out to be false—in which case, of course, they would not be known at all, *a priori* or otherwise²—and, lastly, that they might not be, strictly speaking, *conceptual*: concepts may be importantly different from the meanings of the words we use to express them.

Moreover, ironically enough, I want to do all this, in a way that Devitt *should* accept.³ For I want to press it in the context of a general Quinean

² Maybe Nenad won’t be so shocked: in his (2005a, 303) he does raise the possibility of there being “empirically false analytic propositions.” However, so far as I could see, he doesn’t pursue this possibility in that article or elsewhere, but instead seems to focus on ways in which analytic claims are true *a posteriori* (but I have to confess to feeling I haven’t fully understood the whole of this complex article, much less read the whole of his extensive corpus). As Nenad knows, I’m sympathetic to some version of this latter view (see my 1983 and fn 12 below), but it won’t be the focus of my concern here.

Note, by the way, that care has to be taken in expressing both my view and this possible view of Nenad’s: the usual way of understanding “analytic” is “true by virtue of meaning,” a usage that many have come to reject (see, e.g. Devitt 1996). I at least want to recommend using it to mean, rather, something like: “knowable to be true by virtue of being expressive of a semantic rule,” in which case there will be no contradiction in saying that some claim is analytic and false. (Thanks to Steven Gross for pressing me on this point.)

³ But of course likely won’t—he himself is regularly not only shocked, but is scandalized by my even raising the possibility of the *a priori*; see his (1999). However, more recent conversations about the work of Frank Jackson suggest a concession along the lines developed here. Stay tuned.

“naturalism,” where at least the epistemology that interests me, is “a chapter of natural science” (although, of course, one without his behaviorism, and replete with his *bêtes noires*: mind, meaning and modality). But, as will emerge shortly, I will press it largely as an *explanatory* claim, not one that will have serious consequences for on-going *working* philosophical *practice*, neither with regard to *appeals* to the analytic, nor, slightly more surprisingly, even with regard to armchair “conceptual analysis.”

2. Explanatory vs. a Working Epistemology

Indeed, implicit in what I've just said is an important distinction, not noted by Quine, but crucial for my (and really, I think, for any epistemic) discussion: between what I will call a “*working*,” largely consciously explicit epistemology, and an “*explanatory*” one, or a general scientific account of how animals and people succeed in having and manifesting their remarkable cognitive abilities: the rationality, intelligence and frequent *success* of many of their efforts. Socrates in his *agora* and Descartes in his “oven,” and most “non-naturalized” epistemologists, seems to me *centrally* concerned a working epistemology, specifically with what people are justified or entitled to believe in consciously settling disputes that explicitly arise in science and ordinary life.

Historically, of course, this sort of working epistemology has often been accompanied by explanatory speculations about the “origins of ideas,” of the sort one finds persistently in the philosophical tradition, from Plato to the empiricists, up to and including Quine and Davidson, most of which, however, are seldom informed by any serious, empirically controlled research. I don't want here to reiterate familiar complaints about this (what seems in retrospect) often irresponsible speculation, but merely want to emphasize how one should be careful about inferring the character of this explanatory epistemology from features of the working one. After all, there is no real reason the two should coincide: maybe what we fairly self-consciously do in reflection and explicit argument is quite different from what, as it were, our minds/brains may do, often unself-consciously and inexplicitly, in reasoning and learning about the world.⁴

⁴ I emphatically do not mean to be drawing a “personal”/“sub-personal” distinction here, only a distinction between the obviously different *purposes* of a working vs. an explanatory epistemology (hence the “as it were”). Nor do I mean to be suggesting for a moment that the explanatory *ascription* of attitudes is in any way “normative,” as has many have insisted (see the exchange between myself and Ralph Wedgewood in McLaughlin and Cohen 2007).

One might think that this distinction between a working and an explanatory epistemology is simply the familiar distinction between a “normative” and “descriptive” one. And perhaps it is; but I think it's crucial to notice how normative considerations may enter into an explanatory psychology in way that might differ from the role they play in a working one, at least until we have a sufficiently rich psychology that is able to unify the two. After all, one task of a “descriptive” explanatory psychology is surely to explain just how we and other animals come to understand things and succeed in so many of our efforts as well as we appear to do. It's not unlikely that at least some of this success is due to our using strategies that, given our innate endowment in our normal environmental niche, are immensely reliable, sometimes “rational” (Why did Fisher win so many chess games? He was no dope!)⁵ However, there's no obvious reason why the strategies we might employ in working, conscious reflection are actually those that explain an animal's success, especially given its specific endowment in its specific niche. Aside from the fact some of those successful strategies may turn out to be highly specialized and specific to particular domains—say, of language, or the folk theory of biology or mind—and there may be no *general* explanatory epistemology to be had, there's the increasingly obvious fact that a genuinely explanatory epistemology turns out to be immensely more difficult than traditional philosophers have supposed. Our working, reflective practice simply cannot wait on its results.

Given our ignorance of what is genuinely responsible for our cognitive abilities, it may well be that the best working epistemology for the foreseeable future is Quine's pragmatic “Neurathianism”: in explicitly justifying one's claims, one starts at different places at different times, depending upon what serious doubts have been raised about some issue, much as, in Quine's familiar figure from Neurath, one repairs a boat while remaining afloat in it, piece by piece, standing on one side to repair the other, only to stand ultimately on the other to repair the first.⁶

⁵ Devitt (pc) has urged me to distinguish here “structural” and “causal” explanatory epistemologies. Although I share his interest in the distinction, I don't see that it's relevant for purposes here, since normative considerations might play a role in both sorts: an animal's structural competencies may as much explain and underwrite its successes—the justification, warrant or reliability of its beliefs—as would appeals to specific causal, historical, perhaps evolutionary processes.

⁶ This points to another possible difference between the working and the explanatory: ruling out alternatives relevant to especially social reflection may not be the same as what is ruled out by explanation of success. The hypotheses that need to be ruled out by a linguist determining a grammar may be more various than those for a toddler acquiring it.

This Neurathian figure can seem to invite Quine's other familiar figure of “confirmation holism” whereby⁷

our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body. (Quine 1953, 38)

a claim that he seems to treat equally as a claim about explanation as well as about working reflection. In his behavioristic framework,⁸ he viewed people's cognitions as essentially bundles of dispositions to assent and dissent that changes under the pressure of stimulation, with no particular disposition sacrosanct. And perhaps something like such a view is convenient for working purposes, where we're largely concerned with just such dispositions and disputes among the target sentences. But it's become pretty clear since the demise of behaviorism that this model won't suffice for explanatory ones, where, increasingly, the interest is in innate, mostly unconscious cognitive capacities regarding objects, minds, mathematics, morals, as well as what seems a relatively autonomous human language faculty (“HLF”) that may or may not be manifested in overt verbal dispositions. The crucial point for present purposes is not whether these latter specific proposals are true, but only to indicate how, while a working epistemology for the foreseeable future might well be Quinean, without sacrosanct “foundations,” an explanatory epistemology could turn out to be otherwise, based on peculiar innate principles, concepts, and the inputs from our perceptual modules; indeed, from an *explanatory* point of view there could turn out to be *a priori* knowledge after all, even if it would be useless for the foreseeable future in any working epistemology (see my 1993, 1998, and §6 below).

⁷ I stress “can seem to invite” since, of course, it doesn't entail it, nor—as Quine (1975/81) himself notes—need the “holism” include quite *all* “our statements about the external world,” but merely “large chunks” of them. But, if it's only large chunks, then there's no longer any challenge here to the *a priori* of logic and math, which might remain quite as insulated as they strikingly appear to be (Duhem didn't include them in *his* holism; one wonders why Quine thinks he really can include them in his. Indeed, one might well wonder about the “only” in the quote, see my 1998, 2013), as in Devitt's (1996) repeated claim that “there is only one way of knowing, the empirical way that is the basis of science (whatever way that may be)” (1996, 2; see also his 2011, 21). Any serious claims here depend upon being *far more precise* about the holistic virtues of hypotheses—simplicity, generality, etc.—than Quine ever was (see his 1986, 493 for his frankness on the issue).

⁸ Which, astoundingly, persists in the recent second edition of the (1960/2013) book without a trace of acknowledgment by its introducers of the now fifty or so years of difficulties raised for it since the first edition! To be sure, Fodor (2000) raises Quine's holism as a problem for a more computational explanatory psychology, but relies essentially on the same (rash?) generalization from Duhem that Quine invoked (see previous fn). Best, I submit, to regard the jury as still out on the issue.

In any case, without therefore abandoning a Quinean *working* epistemology, I want here to explore one strand of this possibility or, anyway, the possibility that some sort of “analyses of concepts” may be an important explanatory ingredient of our cognition, despite the fact that they may have a negligible role to play in our on-going explicit practices of working justification.⁹

3. Meaning without Truth

As a way of making this point particularly vivid, I want to press what might seem at first blush the incoherent possibility that *some* of these “meaning analyses” could in fact turn out to be correct as proposals about the semantics of our concepts, but they may nevertheless turn out to be false as claims about the world. I will even press further and claim that, if they turned out to be correct semantic proposals, then they could provide us with a *a priori warrant* as claims about the world, even if they turned out to be false; they would amount to a *a priori* knowledge if they turned out to be true and not seriously defeasible.

Specifically, I want to show how this possibility is raised by some recent proposals of Chomsky (1996, 2000) and Paul Pietroski (2005, 2010), according to which the HLF makes available to our cognitive and other “performance” systems various phonological, syntactic and at least rudimentary semantic material, *without that material itself always having or determining fully semantic phenomena of reference and truth*. Thus, Chomsky (1996) writes:

We cannot assume that statements (let alone sentences) have truth conditions. At most they have something more complex: ‘truth indications’ in some sense. There is good evidence that words have intrinsic properties of sound, form, and meaning; but also open texture, which allows their meanings to be extended and sharpened in certain ways. (1996, 52, quoted in Pietroski 2005, 253)

⁹ I like to think that this ought to mollify Devitt, whose opposition to the *a priori* surely has to do largely with a working epistemology, i.e., of appeals to the *a priori* in scientific practice. After all, he isn’t seriously concerned, any more than Quine was, with the actual *explanation* of our knowledge of logic, mathematics, or, for that matter, meaning; or, if he is, then—along with the rest of the philosophical tradition—he should be far more daunted by the task than his (1996, 2) claim about “the only way of knowing” allows.

As Chomsky points out (2000,188), this proposal is a way of fleshing out Peter Strawson's (1950) claim that the things that are true and false are standardly not sentences by themselves, but *statements* made on specific occasions in specific contexts. Distinguishing "I(nternal HLF)-expressions" from the usual "External language"¹⁰ expressions, Pietroski (2010) puts it this way:

I-expressions do not have satisfaction conditions in the same way that concepts do: the semantic instruction issued by "cow" is satisfied by fetching a concept that has a Tarskian satisfaction condition; and a polysemous expression like "book" may not determine a Tarskian satisfaction condition. (Pietroski 2010, 266)

These days Strawson's concerns have, of course, come to occupy center stage quite independently of Chomsky, especially for those participating in the so-called "contextualist wars" between quite number of recent philosophers, occasioned largely by what have come to called "Travis cases," a few of which are worth very briefly noting as providing still more motivation for the above "radical contextualist" view of Chomsky and Pietroski.

3.1. Travis Cases, "Contextualism" and Language Games

Since the late 1970s, the philosopher, Charles Travis, has been arguing for what he claims is a gulf between the linguistic "meaning" of a sentence and its truth-conditions, by imaginatively calling attention to how even very innocuous sentences, such as "The kettle is black," "The ink is black," "The leaves are green," and "John weighs 160 lbs." He provides a breathtaking range of examples—hence the term "Travis cases." Here are a few:¹¹

The Black Kettle: Contrast someone saying "The kettle is black" to their spouse when (a) buying one in a store, comparing it to the red one; (b) having bought the red one, complaining that, having been left on the stove, "It's black."

¹⁰ By "External languages" I will mean the usual "languages" we ordinarily refer to, e.g. Croatian, French, English, which are (I think) ordinarily understood as systems of, in part, conventional rules. Chomskyans do also speak of "E-languages," which are not understood this way, but rather as *sets of expressions* of these ordinary languages, an extensionalist suggestion that flourished in Behaviorist approaches to linguistics, but which I had thought had long died out. I shall follow Chomskyans in using the "I-" prefix for things associated with the HLF: thus, e.g. an I-language, I-semantics (although I haven't actually seen this last in use).

¹¹ See Travis (1985, 1996), Bach (1999), Borg (2004), Recanti (2004), Cappelan and LePore (2005), Carston (2010) and Vincente (2012) for representative discussions of different ways to deal with such cases.

Blue Ink: In one context, the ink can properly be said to be blue if and only if it produces blue writing when used; but in another context, it can only be said that the ink is blue if and only if it looks blue (irrespective of whether it produces blue writing when used).

Red/green leaves: Someone paints some russet maple leaves green, and when asked what color they are now, she says “The leaves are green. But of course this sentence would be false if a Botanist asked her, ‘What color are your maple leaves?’” (Travis 2008, 111)

Refrigerator milk: “There's no more milk in the fridge” would be true in reply to someone asking about what's available for breakfast, but false in reply to someone worried about whether the milk spilled in the fridge has been cleaned up, when it hasn't been.

Weight: “John weighs 150 lbs” might be true after a big meal, or when he's in his normal clothes, but false otherwise.

Cutting Grass: “John cut the grass” would be false if he failed to do the mowing he was hired to do, even if it was true that he had carefully made a small incision on each blade of grass. (Searle, 1983).

As I said, there has been extended controversy regarding how to treat these cases, into all of which there is no need to enter here.¹² People have appealed to various devices e.g. implicature, hidden indices, polysemy, precisifications, open texture, in the hope of preserving some vestige of truth conditions for a raw sentence, but Chomsky and Pietroski are sceptical any such devices will be sufficient for the full range of cases.

It's interesting to align the Chomsky/Pietroski radical contextualism with observations of the later Wittgenstein (1953)¹³: we play various “language games” with the material the HLF makes available, and which we may deploy in a wide diversity of ways, for which a substantial, unified theory of reference or truth may not be needed or available; or, at any rate, is not the topic of I-semantics. A striking example of such a game is the variety of things we perfectly happily say about “the weather” and “the heavens.” We not only talk about “the heavens above,” but say more specifically: “The sun rose today at 6am,” and “The sky is blue, although it was grey earlier,” “There was a double rainbow for an hour,” even though, on modest scientific reflection, we know very well that the sun doesn't actually rise, and that there are really no such “things” as “the (blue/grey) sky,” or “the two rainbows,” or, come to

¹² See Sperber and Wilson, D. (2005) for review of much of the relevant literature.

¹³ Which provided some of the inspiration for Travis' original discussions.

think of it, genuine *particulars*, “the wind” or “the rain.”¹⁴ We all play this fairly elaborate language game of reporting on the weather and the heavens without having or needing any clear answers to these questions. It is certainly doubtful that an I-semantics would provide them, although it might well provide some loose instructions about how the expressions can be used.

Philosophers there are, of course, who might take these questions very seriously, and try to construct some “entities” to which (our uses of) the word “sky,” and “rainbow” might “refer.” But surely it's a fool's errand, and a pointless one. It's not as though such metaphysics would deepen our understanding of rainbows or “the sky.” But note that those of us, like me, who on reflection, don't believe there really are such “things” as “the sky,” “rainbows,” nor even any real colors of anything, quite cheerfully play the “heavens” language game, and even think some of our utterances of, e.g. “The sky is grey,” and “The sun rose at 6” as nonetheless, within that game, sometimes *true!*

It bears stressing that this sort of relativization of truth and reference to context doesn't entail any anti-realism in a way that some might suppose, so that one can't ever ask about reality independent of context, or “conversations” (as in Rorty 1978; see also Jackendoff 1983, 2006). There, of course, are many ways to resist such suggestions (see Devitt and Sterelny 1987, for some excellent ones). What I favor is simply not treating all conversations or language games on a par. Indeed, one language game that I and many philosophers and scientists (but probably not many other people) like to play is one that arguably plays a very special, explanatorily authoritative role in our thought: *objective science*, or the effort to try to find a description and explanation of worldly phenomena, so far as possible, *independent of human interests, practices and (other) language games*. Anyway, it seems to me that it's only in this

¹⁴ If you're unconvinced, spend a spare hour or seven seeing if you can figure out exactly what “*thing*” we even intend to refer to with these terms: Just where is the sky, or “the wind”? Is the blue sky here “the same sky” as that a few miles west where it's raining, “the wind” the same one east of here as west? When “the sky is grey,” it's of course the clouds “*in it*” that are grey. And is it “the same sky” in which the stars reside, and into which we rise in an airplane, but which isn't the least bit blue “up close”? And just where are those rainbows? As I speed along the highway, they seem to be “in the same place,” but, of course, as my angle even slightly changes, I'm continually looking at different raindrops in different portions of the atmosphere, as well as different rays of light. Leave aside rivers: it's certainly not clear that one can ever see “the same rainbow” twice!

It is instructive to think of this odd “heavens” talk in thinking about the efforts of philosophers to try to determine the metaphysics of “objects” and “events” in general: the vase vs. its parts, Theseus' ship, hurricanes, symphonies, performances, cities, clubs, projects, ... Aside from cases where there *might* be a serious scientific purpose served by spelling out the metaphysics (the volume of a gas, or the shape of a “wave” in physics; perhaps “money” and “inflation” in economics), it's hard to see how these aren't merely matters of forensics and pragmatics, i.e., to be left to law courts or the practical interests motivating ordinary uses of language.

context that philosophical questions of “reality” and “realism” seriously arise, in which there might be a point to asking whether there really are, independently of our perceptual systems and language games, such “things” as skies and rainbows. Thus, although in the “heavens” language game, it might well sometimes be counted true that “the sky is blue,” in the objective scientific one it never is.¹⁵ Moreover, it is especially in the scientific enterprise that one doesn’t want to be hamstrung by ignorance and error, and in which therefore Kripke, Putnam and Devitt are likely right to insist upon often looking to the natural kind causes of the use of term as a determiner of reference, even though this might be irrelevant in many ordinary exchanges (water may well be H₂O, and necessarily so, even though we are often referring to something much weirder when we ask for “water” from the fridge).¹⁶

4. Analyticity

How does all this relate to analyticity? Recall the dreams of most of 20th C. philosophy, specifically, logical atomism, logical positivism and the “ordinary language philosophy” of (broadly speaking) Wittgenstein, Ryle and many of their followers: certain claims could be known to be true “independently of experience” and couldn’t be refuted by it because of their “meaning,” “logic,” or “grammar,” discovered by some or other process of “analysis.” I don’t want to rehearse here the many difficulties with all these proposals. To the contrary, I actually want to resurrect something like their possibility, albeit in a highly qualified way.

If I-meanings are merely instructions to our conceptual system about material relevant to constructing truth-evaluable statements in various ways in various contexts, then it’s a serious option that a speaker could override the instruction in view of further pragmatic or theoretical reasons to do so. Consequently, one could engage in at least something like the kind of traditional “analyses of the meanings” of ordinary terms without being bound to claiming that these analyses are true in all contexts of the use of the terms, or even in precise scientific contexts, where very special constraints might be in force.

¹⁵ One may be tempted to say here merely, “it isn’t really true.” But notice that “The sky is blue,” could be said to be “really true” on a bright, clear day *within the heavens language game!* It’s in *the scientific language game* that it is really false in the way that (some) philosophers think and find it theoretically interesting that it is.

¹⁶ Thus, I’m inclined to confine both my “Hypothesis of External Definitions” (Rey 1983, 20) and Nenad’s (2005a) similar proposals, to the *scientific* use of language. I suspect, though, that Putnam (1975) is right in thinking that many of the folk may in fact still defer to such usage, even when they know they’re speaking “loosely” with other interests in mind (as when they ask for water from the fridge).

In effect, what I want to suggest is that the traditional proposals should be regarded not, as they usually were, as proposals in a working epistemology, but rather as proposals in an explanatory one, specifically as proposals about the I-semantic material issued by the HLF. As such, they are to be ascertained not by traditional armchair reflection (although that may provide important data), but by empirical theoretical investigation, along precisely the lines pursued by Chomskyans such as Pietroski. For just what the HLF presents to the conceptual system is not something we can ascertain by reflection alone. As we have seen for the last sixty or so years, the character and output of the HLF, e.g. merely the syntactic structures it produces, may be surprisingly complex and outré, (dis)confirmable, like all empirical linguistic claims, not only by evidence provided by their own acceptability intuitions about their language, but also by those of others about theirs, as well as evidence of processes of language acquisition, neural processing—indeed, as in the case of evidence generally, anything that might be an *effect* of the HLF.¹⁷ Thus, joining others working in linguistic semantics, Pietroski posits fairly elaborate instructions to construct event-like structures in 2nd-order logic, replete with agents, patients, themes—and, for Pietroski, solely out of monadic predicates!—as the I-referents of verbs. He then argues, to my mind quite persuasively, that these “events” shouldn’t necessarily be identified with *real* events in the external world: the instructions specify “event-like” structures that users of the language may deploy in thought to describe the world as they see fit.¹⁸

5. How Could Something Analytic Be False?

“But how could this be?” someone might ask, “How could, say, an ‘instruction’ to the CI system fail to be obeyed by that system without equivocation, i.e., ‘a change of meaning’?” Well, on the face of it, instructions can still play an important causal, explanatory role in a person’s psychology, without them always being obeyed. Someone can

¹⁷ I discuss this status of “intuitions” in my (2013) reply to Devitt (1999), who presses a different view, there and in his (2013). On this issue I believe Nenad and I converge (see his 2006).

¹⁸ I like to think of the “event-like structures” that serve as the “I-referents” of verbs (indeed of many of “things” discussed in linguistics, such as phonemes, words and sentences) of I-language at least often as “intentional inexistents,” à la Brentano—i.e., things that we “talk” about even when we know full well that they don’t exist (*at all*, nowhere, nohow). Theorists *pretend* they exist merely in order to express the content of representations that imply they do. Apparent reference to them is part of elaborate systems of classification not of worldly items themselves, but of mental states that have those (maybe non-existent) “items” as their contents. But there’s no need to pursue this point here; and I’m happy to remain in the end agnostic about the reality of any specific cases: what matters is the mere, serious epistemic possibility of unreality. See my (2006, 2012), but also, for welcome independent suggestions, Crane (2013).

take fully seriously (and even arguably know *a priori*!) “prohibitions” against killing while still thinking it’s OK to kill in self-defense. I-language instructions, like moral “laws,” may be kinds of defaults for the construction of assertions, which can and maybe ought sometimes to be overridden in the light of further considerations.

Consider, for simple example, what might seem like the most difficult kind of example, a stipulative definition, what Quine (1956) calls “legislative postulation,” e.g. I just stipulate that by “schmuncle” I mean “unmarried uncle.” Wouldn’t this make it analytic that schmuncles are uncles, which I could know thereby *a priori* in way that could never be refuted by experience?

In an important passage, Quine (1956) considers whether such stipulations could create a “truth by convention,” and so an analyticity:

Might we not still speak of a sentence as forever true by convention if its first adoption as true was convention? No; this, if done seriously, involves us in the most unrewarding historical conjecture. Legislative postulation contributes truths to the corpus of truths; the artificiality of their origin does not linger as a localized quality, but suffuses the corpus. (Quine 1956, 119-120)

And here he appeals to his famous justificatory holism:

[S]urely the justification of any theoretical hypothesis can, at the time of hypothesis, consist in no more than the elegance and convenience which the hypothesis brings to the containing bodies of laws and data. (Quine 1956, 121)

Suppose, for example, that Newton had himself explicitly set out “ $F=ma$ ” as a stipulated definition of “ F ” (maybe inscribing it therefore in gold!). This wouldn’t really settle the philosophical question of whether “ $F=ma$ ” is immune to revision, since the stipulation could be overturned if it turned out that it and the rest of his theory didn’t pan out. That is, whether the stipulation is to be taken seriously depends upon “the elegance and convenience” it brings to the rest of our theories of the world. If it doesn’t do so, that might be a serious reason to revise it.¹⁹

But, “Ah,” the Quinean might well reply, “it’s all very well to retreat to default rules rather than strict ones, but what distinguishes a rule, say, ‘Treat all Fs as G’ that’s an I-semantic instruction to the CI system from

¹⁹ As, indeed, I gather Special Relativity gives as reason to do in the case of “ $F=ma$ ”. Pietroski has suggested to me another nice example: it could turn out that water doesn’t boil at quite 100°C despite “100°C” being originally stipulated to be the temperature at which water boils (cp. Kripke, 1980, on the “contingent *a priori*”). Speaking generally (unfortunately without examples), Harman (1996, 399) points out “just as something that is assumed to be true can turn out not to be true, something that is true by stipulative definition can turn out not to be true either.”

simply an entrenched belief that all Fs are Gs? Here, I suggest, we ought avail ourselves of suggestions of a (in this regard) quite recent Quinean, Fodor (1987; 1991; 2008), as well as of Devitt (1996) and Horwich (1998). I have argued elsewhere (Rey 2009) that there's a way of distilling their insights from other features of their views to create the following proposal: the content of an internal symbol in an agent's mind is determined by the property of a meaningful tokening of a term that is *explanatorily basic*, the one on which all other tokens with that meaning synchronically, asymmetrically and explanatorily depend by virtue of that property.²⁰ To a first approximation, it is such properties that, I propose, are being isolated by an I-semantics. They are, as it were, "policies" of the HLF guiding the other uses (cf. Fodor 1987). For familiar example, that "bachelors" is connected to "eligible male" partly guides and explains my calling John "a bachelor" (*ceteris paribus*, if I were to give up the former, I'd be at a loss about the latter), but not *vice versa*: calling John a "bachelor" doesn't guide and explain my thinking bachelors are unmarried (I could give up calling him one, without giving up the latter).

Note that, as *explanatory* conditions, the I-semantic conditions, may be as "hidden" and non-obvious to us as the rules of its grammar (they're certainly not determined merely by armchair reflection in the style of traditional philosophy). Moreover, as only one determinant of actual linguistic usage, there is no problem of our violating those rules in our linguistic practice. Most of us have come, for example, to violate the rule of using the nominative case on both sides of the "is" of identity (only pedants and snoots say "It is I" and not "It's me"), even they still remain rules of our grammar (I nevertheless say "It's she who will visit him" not *"It's her that will visit him").

So this is how I propose we regard uses of "analytic" sentences in science, and other contexts where other considerations may lead us to deny them: Sure, there are the rules, just as there are, indeed, moral precepts against killing people, and it may be explanatorily important that there are, say, in theories of the language and the moral faculties; but, in both cases, there can be considerations that override the strict adherence to the rules, which, I suggest, are really only something like default rules.²¹

²⁰ Thus, I abstract from the (to my mind) excessive referentialism of Fodor, and the excessive "reductionist," "deflationist" and what I call "superficialist" (behavioral, ordinary introspective) suggestions with which Horwich burdens his own account.

²¹ This seems a good place to mention that, in the case of "empty" terms and concepts that refer to nothing, these default rules (or corresponding rules for concepts) may be the best we can do by way of giving an optimal account of the nature of "the phenomenon" the term or concept seems erroneously to be positing; which is why, *pace* Nenad's (2005b) objections to Rey (2005), I think there is room for substantive "analysis" particularly in the case of empty terms, where we can't defer to the external world for help. Harman (1998) also proposes analyses as default rules.

6. And *A Priori* as well!?

OK, so here is where I perhaps will perhaps most shock Nenad, as I've shocked and scandalized Devitt (1996; 1999) before him. I've elsewhere (Rey 1998; 2013) argued that someone might be said to have *a priori* knowledge for a claim if it was produced by an intuitively clear, absolutely reliable causal procedure in one's brain, as might occur, for example, if a Gentzen-style system of natural deduction were realized in one's brain and was the cause of a logically valid belief.

I want to propose something similar for someone's claims that were caused by the output of her HLF, but to make this clearer, I want to want first to retreat (as I think I should have in my earlier discussions) to a notion of mere *a priori* "warrant" or "entitlement," in Burge's (1993, 458-9) sense in which someone may be epistemically warranted or entitled to a belief even it is not consciously accessible, and even it could be defeated by further empirical considerations. To a first approximation:

A statement of "p" is "**analytic a priori**" iff:

if the statement of "p" were true it could be known a priori to be so by virtue of a belief in it being caused and warranted by I-semantic instructions from one's HLF, independently of (non-HLF) beliefs or perceptions.

This requires a slight emendation to Quine's (1956) nice insight about legislative postulation:

[I-semantic instructions] contribute truths to the corpus of truths; the [HLF psychology] of their origin does not linger as a localized quality, but suffuses the corpus. (121)

In view of my distinction between a working and an explanatory epistemology, this actually overstates the case: Quine is right that, from the point of view of the *ultimate working justification* of a claim, neither stipulations nor HLF instructions remain as interesting localized qualities; but from the point of view of an explanatory epistemology, the HLF connection may retain its significance, explaining, *inter alia*, why people are warranted²² in believing it without experience, and why, if it is given up in a specific context, people still register some puzzlement and discomfort that it is. There is, after all, *something* to be said for acquiescing to the meaning constitutive I-semantic instructions, if only to stabilize thought and talk; and sometimes the (as it were) stipulations of the HLF may be just the ones to be preserved in the best science: our

²² One might wonder what place a normative term like "warrant" has in a purely explanatory epistemology. But if, per §2 above, one aim of an explanatory epistemology is to explain how animals come to have fairly reliable beliefs about the world, then "warrant" in general might be understood in terms of those states and processes that contribute to that reliability.

HLF might sometimes provide an apt framework for thinking about the world. If something for which one has *a priori* warrant turns out to be true, and the best science provides no serious “defeaters,”²³ then one can have analytic *a priori* knowledge of the claim. Thus, if it's part of I-semantics that zero is a number, then, given the utter lack of any serious defeaters, one may well know that *a priori*.

Of course, someone might complain that this is a pretty anaemic sort of “*a priori*.” Anna-Sara Malmgren (2006), for recent example, writes:

Any interesting notion of the *a priori* entails empirical indefeasibility: *who cares* if [a piece of] knowledge can be *a priori* in a weaker sense? (201)

And perhaps, unable to trump empirical findings, a defeasible *a priori* would have little significance in a *working* epistemology. But it might still be significant for an *explanatory* one. After all, whatever the ultimate, defeasible working justification of, e.g. mathematics, there can be little doubt that mathematicians seem to arrive at their conclusions “by reason alone,” and that this requires a more serious explanation than merely a vague Quinean appeal to the relative centrality of mathematics in a pervasively defeasible web of beliefs.²⁴

7. But Is It “Conceptual Analysis”? Three Examples

I want briefly to discuss three examples that I think illustrate the attractions of my proposal, and also its perhaps slightly surprising limitation: (i) Cats are animals; (ii) gay marriage, and (ii) an aspect of the mind-body problem.²⁵

²³ I say “serious” defeaters to rule out possibilities, such as “evil demon” or “brain in a vat” hypotheses that we have no reason to take the least seriously, as I think no sufficiently “naturalized” epistemologist should.

²⁴ Note that Kant, after all, defined the “*a priori*” as “justifiable independently of experience”, and, so far as I have read, said nothing about “immunity to revision.” Why couldn't someone be perfectly rationally misled by experience to think, e.g. that quantum mechanics provides a reason to abandon Classical Logic? In my (1993; 1998) I argue that the “unrevisability” conception of the *a priori* was in part something foisted on us by behavioristically inclined Positivists, especially Quine.

²⁵ I'm taking up some standard philosophical examples, but by no means want to suggest that analyticities are limited to them. As linguists have repeatedly pointed out, nouns like “cat,” are not the best candidates for at least I-semantic analyses; for starters, verbs are much better. Thus, consider the interesting properties of some, but not all English verbs, that allow ergativity, or use as a transitive, or as a intransitive whose subject is the object of the transitive, as in *x cooked/boiled/baked y* (which entail that *y* was cooked/boiled/baked; vs. *John kissed Betty* but not **Betty kissed*); or Chomsky's (1988, 33-4) standard example of *If you persuaded John to go to College, then John decided to go to college*, or what certainly *seems* to be the analyticity of *if Alf walks/runs/trots/saunters then Alf moves*. Much subtler discussion of the explanation of these striking phenomena are needed than has been supplied by Quine or his followers (e.g. Fodor 1983; 1998).

(i) Cats: This case is, of course, interestingly trivial. Putnam (1962) and Jerry Katz (1990) famously disagreed about whether “Cats are animals” is analytic. Putnam claimed that it could turn out that the things we called “cats” were actually cleverly designed (very odd) robots from Mars; Katz that, were this to turn out to be true, it would simply show there were no such things as cats, since cats “by definition” are animals. On the view I’m recommending, they could both be right: the HLF might well link “cats” to animals, and yet, for all sorts of uses of the word, we might override that link if our interactions with the robot/cats remained on the whole the same.²⁶

(ii) Gay Marriage: This case is obviously more socially significant. Some years ago, two philosophers, Adele Mercier and Rob Stainton, were asked by opposing sides in a case before the Canadian Supreme Court to provide affidavits as to whether “gay marriage” was a contradiction in terms. I have to confess that my initial reaction (to the understandable annoyance of both Adele and Rob) was to burst into gales of laughter, exclaiming that one could hardly encounter a better example to motivate Quinean scepticism about the determinacy of such questions. However, reflecting on it later, I realized there could be a (mildly) interesting issue here. I have never read the lengthy briefs submitted to the court, but I imagined that, on the one hand, one might argue that the word “marriage” in the I-languages of most “English” speakers—and perhaps the framers of the Canadian marriage laws—was likely linked analytically to “husband” and “wife,” and that “husband” was marked for male and “wife” for female, just in the way “he” and “she” are, so that consequently “gay marriage” was contradictory. Of course, these rules could change: perhaps in time people will no longer mark “husband” as male; in which case, indeed, there’s been an I-meaning change.

If all this were true, perhaps it would have legal significance. After all, it’s reasonable—some might say, required—for the Supreme Court to stand by the meanings of the words the framers of the laws had in mind. But are the relevant “meanings” the framer’s *I-meanings*? Well, that is a forensic issue in itself (about which I doubt there is much precedent), but if Chomsky and Pietroski are right, the I-meanings provide not truth-conditions, but only default instructions to the conceptual system, which then goes on to use them to make statements in particular contexts. It’s presumably only these latter that are the appropriate vehicles of truth-values to which the court is obliged to attend. And, as we’ve noted, what may inform those uses can be many more considerations than the I-semantic instructions alone, e.g. the open texture of the concepts the words express, the social significance of the institution, issues of

²⁶ A lot would depend upon further details: do they still bleed, need to be fed, etc? Depending upon how pervasive such robots are (how about dogs and cows?), biology might also have to be weirdly revised. Again, my point is merely that the issue is largely pragmatic/scientific, not settled merely by the (purported) I-semantics of “cats” alone.

compatibility with other clauses of a constitution.

This last consideration points to a further issue of some philosophical interest. Particularly if Pietroski's specific proposals were correct, and the logical form of I-sentences may be different from the form of the representations used by the *conceptual* system,²⁷ then it could turn out that the constraints on concepts aren't the same as the constraints on the words we use to express them. And so the fact (if it is fact) that the word "marriage" might be marked for genders in the above way might not issue in genuine contradictions—which, indeed, it certainly appears not to do: after all, virtually no one who hears the expression (aside *perhaps* from the not disinterested plaintiffs in the case?) regards it as remotely self-contradictory, in the way that they do "round square" or "number that's even and odd." The *concept* MARRIAGE might not actually be bound by the I-semantics of the word "marriage."

Of course, it's not at all easy to get a handle on the constraints on concepts independently of the constraints on the words we use to express them, and aside from drawing attention to this potentially important distinction I'm afraid I have nothing particularly useful to say. Analogously to the project of a Chomskyan linguistics, what seems to me to be wanted is a distinctive explanation of what people genuinely find conceptually possible, such as gay marriage as opposed to a round square, as a consequence of the identity conditions on the concept, and not merely because of failures of intelligence or imagination, or constraints from the HLF. As in the case of I-semantics, it wouldn't be at all surprising that such an explanation may need to appeal to structural facts about the conceptual system that may not be readily available in thinker's surface verbal behavior.²⁸ Of course, maybe at the end of the day there is no real difference between concepts and I-meanings—or, on the other hand, between concepts and specific intransigencies in our thought. But, in view of the marriage case—as well as perhaps the cases of "round square" and "even and odd," which are likely not contradictory

²⁷ For example, as I noted above in passing, Pietroski defends the interesting claim that the logical form of I-sentences involves only one-place predicates, for which the conceptual system may well supply predicates of greater n-arity. There's no reason to insist on this specific theory here; I mention it only for illustrating the possibility of divergencies between concepts and the I-semantics of the words we might use to express them.

²⁸ A possibility that recent discussions of intuition seem to me to need to take more seriously. Williamson (2008), for example, dismisses appeals to "sub-personal" competencies as falling under the rubric of Stanovich and West's (2000, 659) "system 1" phenomena, which involve "quick and dirty" heuristics on which people seem standardly to rely for rapid judgments, as opposed to more reflective "system 2" processes (see also Kahneman 2011). A Chomskyan *grammar* would seem to be as "sub-personal" as a psychological phenomenon might be, but is obviously not "quick and dirty" in the way that perhaps a *parser* is. *Mutatis mutandis* for conceptual structure.

due *merely* to I-meaning—it seems to me a distinction well worth considering.²⁹ Perhaps the following case will make it still more vivid:

(iii) Mind and Body: Consider the common puzzle about the possibility that computers might actually think and enjoy a mental life. In response to this puzzle, some philosophers seem to suggest that it may be analytic that a thinking thing must be alive. Thus, Wittgenstein famously writes:

Only of a human being and what resembles (behaves like) a living being can one say: it has sensations; it sees; is blind; hears; is deaf; is conscious on unconscious. (1953, §281)

a passage Chomsky (2000, 44) cites with approval (see also Ziff 1959, Hacker 1990, 156,168). And the suggestion certainly seems to accord with many folk intuitions: as I tried to make vivid in my (1997, ch 11), many people who cheerfully entertain computational explanations of mental process often balk at the suggestion that an inanimate machine engaged in those very computations would actually be undergoing those processes—e.g. consciously sensing, feeling, genuinely thinking.

Let's suppose that these folk intuitions are in part explained by the I-semantics of mental talk: it's all linked to folk biological talk. In accordance with the view about language sketched here, uses of it could nonetheless be extended to artifacts, depending on the pragmatics of the specific context of use, which might, of course, involve scientific, explanatory considerations.

Not surprisingly, this is precisely how Chomsky (2000) thinks about the issue. Explicitly addressing the question of whether machines can “think,” he writes:

It is not a question of fact, but a matter of decision as to whether to adopt a certain metaphorical usage, as when we say (in English) that airplanes fly but comets do not—and as for space shuttles, choices differ. Similarly, submarines set sail but do not swim. There can be no sensible debate about such topics; or about machine intelligence, with the many familiar variants. (2000, 114; see also 28, 147-8)

But, now, it's not hard to feel that this dismissal of the issue misses something important. Why *don't* people (not only some philosophers) find the questions about whether machines can think (experience red, be in pain, be conscious) as trivial as the question of whether submarines swim?³⁰ Why do pretty intelligent people get confused and troubled about

²⁹ For those who think “conceptual analysis” is really just theorizing about the referents of concepts, I remind them of the problem posed by empty and response-dependent concepts, many of which (e.g. god, the good, the ghostly) also seem often to arise independently of the I-semantics of any specific I-language.

³⁰ The comparison was first made by Edsger Dijkstra (1984).

the issue, and debate it at such length? Whatever the “mind/body” problem is, it's surely not an *idiotic* confusion! Moreover, *pace* Wittgenstein, it doesn't seem to me merely an issue about the *words* we use to describe the mind, as seems to be the case with the submarines. There seem to be some deeper conceptual issues. For starters, here are two:

Firstly, surely one serious question that concerns people worried about whether artifacts could think concerns whether those artifacts could in fact share the real, theoretically interesting, explanatory properties of being a thinking thing; they are asking it presumably from the perspective of science. And, here, as in the case of “water” and other explanatorily interesting natural kind terms, we often defer to the objective structure of the world, as Nenad and I and Devitt agree (see §1 above). Now, of course, in the case of submarines, all the relevant facts are in, and so we're left with merely a verbal issue. However, *spectacularly unlike the case of submarines, we are nowhere near having a satisfactory theory of thought or most other mental processes.*³¹ And so it's a perfectly good question to wonder whether whatever goes on paradigmatically in people when they think also could go on in a mere machine.

Of course, present research seems very strongly to suggest that mental processes are some sort of causal, often computational processes that we have every reason to suppose could be realized in some non-biological structure, e.g. a robot. At any rate, no one has provided any good reason to suppose that being alive is explanatorily crucial for thought.³² And so we might conclude that, despite these constraints of natural language, inanimate computers could come to “think” and have mental states after all. In light of this, one could, like Katz on cats, insist on adhering to whatever meaning constraints turn out to be imposed by natural language and so, perhaps, deny that inanimate computers could ever “think,” just as we could deny that gay people could really ever “marry.” But, if the explanatory and legal points were correct, it would be hard to see how this would amount to anything more than a verbal quibble: So, gay people “marry*” and artifacts “think*” instead. (It's a peculiar feature of the whole philosophically charged discussion of the analytic that it may in the end turn on mere verbal quibbles!) Or, perhaps in time, this default link of the I-words for the mind might change, (or we stop using the I-

³¹ As Chomsky (2000, 83) himself should be first to agree, given what he regards as the likely deep “mysteries” surrounding the mind.

³² Searle (1992) often points out, correctly enough, that mental processes *in us* are biological, but, so far as I have read, he provides no arguments that they *essentially* are. One might think that “teleosemantic” considerations, as in, e.g. Neander (2012), would provide an argument, but that would only be so if stories of selection and “correct functioning” were confined to biology, which there is no reason to think they are (robots could be selected and functionally described).

words regarding “mind?”), and the default link to only living things, might disappear. And people might come to wonder what the problem was.

Or would they? I suspect not. For, secondly, as I argue in my (1996; 1997), even after all the facts are in about what robots can do, there would still seem to be a serious resistance to the idea that they really are conscious, genuinely thinking, having sensations, falling in love, etc., and I suspect this is due to the way we not only *talk*, but *think* about and react to human beings as opposed to robots.³³ In the above quote (as in so many of his later views), Wittgenstein seems to me to go wrong to call attention merely to what we “say”: *that* makes the problem he is addressing too ripe for Chomsky's riposte about submarines. Rather, as I think we saw in the case of gay marriage, the issue doesn't seem to be settled by issues of language alone. At any rate, there seems to me more to be said about our *concepts* of mind, sensations, consciousness (as well as perhaps marriage) than merely I-semantics and pragmatics alone.

8. Conclusion

The conclusion I want to draw is essentially a slightly more elaborate version of the conclusion drawn by Putnam (1965/75): although the analytic may reveal something of interest in an explanatory epistemology, it would be of limited utility in a working one. In Putnam's immortal words, it would “cut no philosophical ice, bake no philosophical bread and wash no philosophical windows” (1965/75, 36), or, anyway, not much: *a priori* warrant based on the analytic might play a role at the start of a discussion, setting certain default assumptions and stabilizing the discussion, just as stipulative “legislative postulations” do; but, as Quine pointed out, not really in the long run. If those stabilizing assumptions prove true and reasonably indefeasible, then one might end up with *a priori* knowledge. If not, then, of course, one doesn't.

Moreover, and perhaps surprisingly, “analysis” of the meanings of our words may diverge from analyses of the concepts those words are used to express, and so, apart from linguistics, analyses of word I-meanings may not tell us as much as we'd like to know about the underlying concepts. The kind of “conceptual analysis” needed for many philosophically interesting issues—for example, why the question of whether machines think is more interesting than whether submarines swim—may require

³³ Which may have to do, as many have emphasized, with how our mental concepts are elaborately entwined with “reactive” attitudes, as in Strawson (1974), some of which are surely innate along lines suggested by the lovely infant experiments of Hamlin *et al.* I argue (1996) that such attitudes lead us to project special properties into humans and many animals, precisely along the lines Wittgenstein mentions, that give rise to “explanatory gaps.”

disentangling that project from linguistic semantics.

Returning to Nenad's concerns: the proposals I've laid out may also be a way to realize the "Frutnamian" project he wishes to pursue. The "sense" might remain the I-semantic material the HLF delivers to the cognitive system, or perhaps the concepts elicited by it; while the "reference" is determined by those concepts and the language game in which they are deployed. Specifically, in the case of the science language game that interests Nenad, (middle) Putnam, Devitt and myself, the I-semantics of natural kind terms may consist simply in instructions to the conceptual system to find the nearest, real, contrasting natural kind of a term, if there is one, to which one is causally connected. And, in the case of empty and response-dependent concepts, there may just be the study of the I-meanings and concepts alone. So, while Nenad may find my view a bit of a second shock, he may also regard it as a gift (which, of course, is really how I intend it). Again, happy birthday, Nenad, and many happy Dubrovnik returns!

Acknowledgments

As should be evident, many of the ideas of this paper emerged from discussions over the years with my colleague, Paul Pietroski, whose work in pressing Chomsky's (1996, 2000) arguments for the separation of linguistic semantics from truth and reference has come as a real philosophical liberation for me (I no longer feel quite so guilty for not having a metaphysics of ordinary things), although the suggestion of "analytic and false" can, I think, be traced back to the ground-breaking middle work (ca. 1960-1975) of Hilary Putnam. This paper itself originated in the last part of my updated (2012) entry on "the Analytic-Synthetic Distinction" for the *Stanford OnLine Encyclopedia of Philosophy*, but then more substantially as a talk at the annual philosophy of science conference in Dubrovnik in April 2014, the Arctic University of Tromsø, Norway, and the Center for the Study of Mind in Nature, at the University of Oslo. I am indebted particularly to Nenad, for helpful comments, as well as to Nicholas Allott, John Collins, Carsten Hansen and Steven Gross, and, per usual, to Michael Devitt, whose reactions this time round suggest that just maybe our views don't diverge as much as he has feared in the past.

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