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Generative Linguistics Meets Normative Inferentialism: Part 2

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This is the second installment of a two-part essay. Limitations of space prevented the publication of the full essay in a previous issue of the Journal (Pereplyotchik 2020). My overall goal is to outline a strategy for integrating generative linguistics with a broadly pragmatist approach to meaning and communication. Two immensely useful guides in this venture are Robert Brandom and Paul Pietroski. Squarely in the Chomskyan tradition, Pietroski's recent book, Conjoining Meanings*, offers an approach to natural-language semantics that rejects foundational assumptions widely held amongst philosophers and linguists. In particular, he argues against extensionalism—the view that meanings are (or determine) truth and satisfaction conditions. Having arrived at the same conclusion by way of Brandom's deflationist account of truth and reference, I'll argue that both theorists have important contributions to make to a broader anti-extensionalist approach to language. Part 1 of the essay was largely exegetical, laying out what I see as the core aspects of Brandom's normative inferentialism (1) and Pietroski's naturalistic semantics (2). Now, in Part 2, I argue that there are many convergen-*

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ces between these two theoretical frameworks and, contrary to first appearances, very few points of substantive disagreement between them. If the integration strategy that I propose is correct, then what appear to be sharply contrasting commitments are better seen as interrelated verbal differences that come down to different—but complementary—explanatory goals. The residual disputes are, however, stubborn. I end by discussing how to square Pietroski's commitment to predicativism with Brandom's argument that a predicativist language is in principle incapable of expressing ordinary conditionals.

Keywords: Generative linguistics; anti-extensionalism; normativity; inferentialism; predicativism; public language; communication.

3. *Prospects for Ecumenicism*

Introduction

In Part 1 of this essay, we surveyed the core commitments of two large-scale theoretical frameworks in the philosophy of language—those of Robert Brandom and Paul Pietroski—and seen some of the ways in which they play out in the realm of semantics, including in detailed analyses of various linguistic constructions. It may appear that the two views are so different in substance and overall methodology that a conversation between the two is unlikely to bear much fruit. This is a large part of why so few conversations of this kind ever take place. In the present section, I argue for the contrary perspective, outlining an ecumenical approach that seeks to integrate the two in a variety of ways. In surveying what I take to be significant points of convergence—which then serve as background for constraining residual disputes—I rebuff various superficial objections to the possibility of integration. In each case, I show how the theoretical differences that they point to can be reconciled without doing much (if any) violence to either view.

3.1 *Truth, reference, and other non-explanatory notions*

One obvious shared commitment between Brandom and Pietroski—indeed, the one that most clearly motivates the present enterprise—is their common rejection truth-conditional semantics. Let's review the key points and add some new ones.

Pietroski surveys a battery of arguments against Davidson's proposal, including its more recent incarnations in possible-worlds semantics. These include troubles with (i) empty names, (ii) co-extensive but non-synonymous expressions, (iii) polysemy, (iv) compositionality, (v) liar sentences, and (vi) event descriptions (*inter alia*). Brandom's skepticism is more foundational. On his view, an explanatory strategy that takes truth and reference—conceived of as "naturalizable" word-world relations—as fundamental semantic notions will require a metase-

mantics that is, at best, optional, at worst, incoherent, and, at present, non-existent. Although Brandom doesn't pretend to have supplied a knock-down argument against it, the flaws he identifies in the various attempts to work out this strategy strike me as fatal. Coupled with his development of a powerful alternative—a large-scale framework constructed from the top down, with pragmatics taking an unconventional leading role—as well as his well-motivated treatment of the notions of truth and reference, Brandom deals a serious blow to the mainstream approach of subordinating pragmatics to semantics.

Brandom's *main* reason for pursuing a normative metasemantics is the inability of a purely descriptive ("naturalistic") account to capture the normative notion of (in)correct rule-following. But this is not his *only* argument, and it's worth taking a moment to spell out what strikes me as a potentially more powerful consideration—one that, in making fewer assumptions, can appeal to theorists of a broader stripe.

All extant attempts at "naturalizing" meaning, content, representation, and the like, have in common their insistence on employing in only *alethic* modal notions in their analyses. These include dispositions (Quine), causation (Stampe), causal covariance (Locke), natural law (Dretske), nomic possibility (e.g., Fodor's "asymmetric dependence"), and even appeals to non-normative teleology (Cummins and, independently, Millikan). Brandom points out that, even if these *could* account for ordinary empirical concepts, it's not at all clear how they might be extended to the very concepts that appear in the analyses—i.e., the *alethic modal* notions just listed (among others).

While it's possible to envision, if only dimly, how something in a person (or a brain) can causally co-vary with—or bear nomological relations to—water, mountains, and even crumpled shirts, there's simply no naturalistic model for envisioning the relation between, on the one hand, the words 'possible', 'disposition', or 'asymmetric dependence', and, on the other hand, any particular set of things, events, or phenomena out in the natural world. The same is arguably true for logical, mathematical, semantic, and deontic vocabulary. (Recall the fonts, functions, and fears from 2.3.) Indeed, the metasemantics doesn't seem to get us much farther than 'stick' and 'stone'. And the experts seem to have given up, since the late 1990s, on the hard at work of bringing 'fail' and 'decisively' into the fold. One is well-advised not to bank on any striking new developments in this area, unless, of course, something dramatic happens in the surrounding domains of inquiry. (My money, for what it's worth, is on the AI people.)

By contrast, a metasemantics that makes explicit and essential use of *normative* terms—paradigmatically, deontic modals—is ultimately able to "eat its own tail", to use Brandom's imagery, by shoring up a principled account of *those very notions*. As discussed in 1, Brandom treats normative expressions as serving the function, characteristic of logical vocabulary more generally, of expressing (i.e., making explicit) one's commitments to the propriety of an inference or a plan of practi-

cal action. Brandom (2008) offers, in addition, an account of *alethic* modal vocabulary (recall the safety measures for gasoline wicks in 1.2), as well as a detailed formal analysis of its many important relations, both semantic and pragmatic, to the deontic variety. In this way, his account can claim a major advantage over virtually any *conceivable* attempt at a naturalistic alternative. And, again, the force of this argument does *not* depend on a prior assumption about the normativity of meaning. This functions here not as a premise, but as a conclusion.

3.2 *Naturalism*

Residual worries about adopting a normative metasemantics will doubtless trouble self-avowed naturalists (including former versions of the present author), who tend to have a constitutional aversion to trafficking in normative considerations. But this, too, should be tempered—or so I'll now argue. The concern is, to my mind, largely dampened by the fact that Brandom's norms are in no way "spooky" (despite drawing heavily on Kant), but, rather, grounded directly in social practices. Such practices consist of activities that are themselves rooted in each creature's *practical stance* of reciprocal authority and responsibility to all others in its community.[1] Such stances are overtly enacted and then, over time, socially instituted in a *wholly non-miraculous* fashion, by *natural* creatures.

Moreover, the resulting discursive/conceptual activities are open to assessment in respect of truth, accuracy, correctness, and overall fidelity to "the facts on the ground" (as the assessor sees them). Indeed, the project of normative pragmatics is so obviously *not super*natural that it's not clear why the self-avowed "naturalist" should be at all worried. Even less clear is why *anyone* should get to dictate the terms of legitimate inquiry *a priori*. Why, after all, should our metasemantic theorizing not make any use of the perfectly familiar and logically well-behaved deontic modal notions? Indeed, why even a *lesser* use of them than their alethic counterparts? What's so special about *alethic* modality, anyway? Nothing much, so far as I can see.

Let me dwell on this point, for it seems to me that the knee-jerk resistance to normative theorizing is deeply ingrained in the naturalist's mind. (I should know!) Pressing back against what now strikes

[1] There's been confusion on this point, caused largely, I think, by Brandom's uncharacteristically ill-chosen terminology. He gives the label "practical attitudes" to what I've here (re-)described as "embodied practical stances" on the part of a creature toward its community members. The use of the term 'attitude' has predictably conjured in the minds of some critics the notion of a propositional attitude—something that already bears a distinctively conceptual/intentional content. Plainly, this would render Brandom's account viciously circular, as he is aiming to explicate the notion of propositional attitude content in terms of (what he calls) practical attitudes. If the latter already have intentional contents, then there's obviously no difficulty in spelling out other semantic/intentional notions downstream. Equally obviously, there would be no theoretical interest in doing so.

me as an irrational prejudice, I exhort philosophers to actively discourage it, whatever the fate of Brandom's philosophical project—or, for that matter, mine—turns out to be. Given our daily immersion in social norms and institutions, it's frankly puzzling that so many theorists have allergic reactions to a deontic treatment of language. Norms are *not* puzzling. They are all around us, every moment of our lives. They permeate every social interaction we have and they are the subject of most of our thoughts, *all* of our plans, and our very conceptions of our own identities as free, responsible agents.

Moreover, with respect to linguistic norms in particular, there are (so far) no *obvious* examples in the natural world of linguistic abilities arising in creatures outside of a relatively elaborate social context. Indeed, even intelligent artifacts wouldn't count, if we ever made one, for they'd be related to the human community of happy roboticists in an obviously relevant way. So it's not at all clear—not to me, at least—why this aspect of naturalism should constrain our inquiries into language and mentality. Obviously, naturalism has many *other* appealing features, but this doesn't seem to be one of them.

The deafening silence from classical naturalists on this point has led some, e.g., Price (2010, 2013), to endorse Brandom's normative inferentialist project and to embed it into a larger philosophical framework that eschews notions of correspondence, reference, "the representation relation", etc. altogether. (No "mirrors", he enjoins, using Rorty's metaphor.) Price applies Brandom's expressivist account of logical vocabulary to *all* of human language. The resulting "global expressivism" is a key commitment of the novel brand of naturalism that he recommends to our attention—one that I find deeply compelling.

What I've been describing as the "traditional" or "classical" naturalist view—i.e., the received view among *soi-disant* naturalists in the literature from Fodor onwards—maintains that we should draw on the tools, models, and concepts of natural science in characterizing atomistic word-object or sentence-fact relations—paradigmatically, reference and correspondence. On this picture, "the world" is seen through the lens of natural science (a category itself subject to some dispute). This is a metaphysical framework that has plenty of room for protons, genes, and brains, but stubbornly refuses to accommodate responsibilities, entitlements, and the like—including, remarkably, *persons* (at least not in the fullest sense of that word; cf. Sellars 1963). The "objects" to which language relates us are thus limited by naturalist maxims to only the "natural" ones—whatever *those* are. For this reason, Price calls this view "object naturalism".

The alternative that Price puts on offer is *subject* naturalism. This view retains a healthy and well-deserved respect for the deliverances of natural science, but refuses to go along with the *philosophical* fiction of "naturalizable" reference and correspondence relations. Rather, our naturalistic urges should be directed, Price argues, toward concept- and language-using *subjects*—i.e., the creatures who acquire, produce,

and consume languages, as just one tool in a larger biological-*cum*-social enterprise of maintaining homeostasis in the species. Paradigmatically, such creatures are human persons, but any other naturally social creature can in principle be studied in this fashion.

What's striking, to my mind, is how similar all of this sounds to the methodological aims of theorists like Chomsky and Pietroski. Although both call themselves naturalists, each has made determined efforts to debunk the idea that word-world relations are relevant to an empirical study of the human language faculty. Nor does either theorist harbor the ambition—characteristic of the "classical" naturalists mentioned earlier—to *reduce* intentionality, either by analysis or metaphysically, to some alethic modal base. Here's Pietroski on the issue:

> One can raise further questions about the sense(s) in which *Begriffsplans* are intentional, and which philosophical projects would be forfeited by appealing to unreduced capacities to generate and execute the instructions posited here; cp. Dummett. But my task is not to reduce linguistic meaning to some nonintentional basis. It's hard enough to say what types of concepts can be fetched via lexical items, and which modes of composition can be invoked by complex Slang expressions.

This is another point of convergence with Brandom's pragmatism, which likewise renounces the reductive aims of the classical naturalist project.

Of course, the mere fact that Pietroski declines to take up the issue in *CM* doesn't mean he has no dog in the fight elsewhere. (I don't, myself, know.) By the same token, although it's true that Brandom doesn't aim to reduce intentional notions to some construct of natural science, it doesn't follow, and isn't true, that he has *no* reductive ambitions at all. To the contrary, his normative inferentialism is designed precisely to reduce intentionality to *something* nonintentional, which, in his case, happens to be the normative. This is why normative pragmatics serves for him as a *meta*semantics, in the fullest sense of the word. The 'meta' indicates not only that what's on offer is a "theory of meaning"—rather than a first-order "meaning theory", to use Dummett's distinction. More importantly, it connotes that the semantics is herein *subordinated* to (i.e., must "answer to", in an explanatory sense) the social norms that are the centerpiece of the pragmatics.

3.3 *Referential purport*

In keeping with his commitment to the methodological tenets of individualism and internalism, Pietroski applies many of the points scouted above to *conceptual thought*.

> [S]ome readers may be unfamiliar or uncomfortable with talk of using representations to think about things in certain ways. So some clarification, regarding aboutness and ways, is in order. … The relevant notion of thinking about is *intentional*. We can think about unicorns, even though there are none. One can posit unicorns, wonder what they like to eat, diagnose various observations as symptoms of unicorns, etc. Similarly, one can hypothesize that some planet passes between Mercury and the sun, call this alleged

> planet 'Vulcan', and then think about Vulcan—to estimate its mass, or wonder whether it is habitable—much as one might posit a planet Neptune that lies beyond Uranus. An episode of thinking about something can be such that for each thing, the episode isn't one of thinking about that thing. … [I]n the course of proving that there is no greatest prime, it seems an ideal thinker can at least briefly entertain a thought with a singular component that purports to indicate the greatest prime. … Paradoxes like Russell's remind us that even idealized symbols can fail to make the intended contact with reality, at least if one is not very careful about the stipulations used to specify interpretations for the symbols.

I want to highlight a key point here: Pietroski is presupposing about concepts *not* that they *succeed* in referring—though he allows that some of them do—but that even *empty* concepts have intentional contents. These latter plainly *cannot* be accounted for by positing straightforward metaphysical relations between words, on the one hand, and bits of the world, on the other.

This emphasis on intentionality in the sense of referential purport is crucial to Brandom's project as well. Rather than setting out to explain *successful* reference/denotation, as the paradigms of perceptual and demonstrative reference have led many theorists to do (2.3), Brandom sees it as necessary to first explain how a creature can so much as *purport* to refer to one thing rather than another, and only later to furnish an account of what counts as success in this regard. Brandom is not alone in adopting this strategy. In the Gricean tradition, the homologous project is cast in terms of the *intentional design* of communicative acts—in particular, a speaker's intentions to refer, denote, predicate, or to speak truly of something. But whether one uses the idioms of purport, design, or intention, the key point is that the phenomenon under discussion does not involve a unique, naturalizable, or semantically-relevant mind-world relation.

That leaves wide open issues about the interface between language and reality, let alone larger questions of metaphysics and epistemology. While Pietroski stays largely neutral on such topics in *CM*, Brandom's account makes quite definite commitments in these arenas. Nevertheless, there is one place where the two quite clearly converge, and that is with respect to their treatments of *de dicto* and *de re* attitude ascriptions. Let's take a look at that.

3.4 *De dicto and de re constructions*

In granting reasonable concessions to philosophers who stress the importance of mind-world relations for our theories of intentionality, meaning, concepts, and the like, Pietroski makes the following remarks:

> I grant that 'think about' can also be used to talk about a relation that thinkers can bear to entities/stuff to which concepts can apply; see, e.g., Burge. In this "*de re*" sense, one can think about bosons and dark matter only if the world includes bosons and dark matter, about which one can

> think. Any episode of thinking *de re* about Hesperus is an episode of thinking *de re* about Phosphorus. This extensional/externalistic notion has utility, in ordinary conversations and perhaps in cognitive science, when the task is to describe representations of a shared environment for an audience who may represent that environment differently. But however this notion is related to words like 'think' and 'about', many animals have concepts that let them think about things in ways that are individuated intentionally. We animals may also have representations that are more heavily anchored in reality; see, e.g., Pylyshyn. But a lot of thought is intentional, however we describe it. (80)

It's important to see that there are two distinct strands of thought here. One is about how *some* representations—again, paradigmatically those involved in perceptual or demonstrative reference—are "more heavily anchored in the world" than others. This might *seem* to put Pietroski's view at odds with Brandom's inferentialism, given how small an explanatory role the latter gives to such "anchoring" relations. But this is worry is spurious.

Brandom's account of perceptual commitments and default entitlements (*MIE*, ch. 4), as well as his (largely independent) account of demonstrative reference and "object-involving" thoughts, are fully compatible with—indeed, positively *require*—the existence of reliable nomic or causal relations between perceptible objects in the world and the perceptual mechanisms of a creature. There are, to be sure, heated debates about how exactly all of that works—e.g., whether the percepts should be seen as having the function of bare demonstratives (Fodor and Pylyshyn 2015), noun phrases (Burge 2010), or inferentially integrated singular thoughts (Brandom 1994). But disagreements on these points are far downstream, theoretically, from the broadly methodological commitments that I want to highlight here. It's these that are the subject of the second strand of thought that I think we should distinguish here.

When Pietroski speaks of a "'*de re*' sense" in which one can think about or ascribe thoughts, he is at once talking about a certain kind of *thought*—the "object-involving" kind discussed above—but also, separately, about a certain kind of thought *ascription*. The latter, he says, "has utility, in ordinary conversations and perhaps in cognitive science, when the task is to describe representations of a shared environment for an audience who may represent that environment differently" (*op. cit.*) Note that this circumscribes the function of *de re* ascriptions to what I think of as "the navigation of perspectival divides". More prosaically, *de re* constructions allow language users to describe the environment in which a creature is deploying its concepts—as viewed *by the describer* (and often her audience)—while *de dicto* constructions function to ascribe *the concepts so deployed*. Here, then, we see another major point of convergence between Pietroski and Brandom. The latter provides an inferentialist analysis of *de dicto* and *de re* ascriptions, according which they perform precisely the function that Pietroski's remarks indicate.

According to Brandom, *de dicto* ascriptions make explicit the commitments of the creature being described—not those of the ascriber, who may be either ignorant on the matter or hold commitments directly contrary to those ascribed. A speaker might say, "Dan thinks the greatest philosopher of language was Quine," without having any commitments one way or the other about whether philosophers exist, or about whether Quine was one of the greats. Indeed, the speaker might think that philosophers are bits of tofu and that Quine is a particularly flavorful brand. None of that would matter with regard to the speaker's entitlement to a *de dicto* claim about what *Dan* said.

By contrast, had the speaker employed a construction that functions in the *de re* sense—e.g., the awkward 'of'-constructions that we've inherited from Quine—then their own commitments would have come into play, with questions arising (at least in potentia) about their *entitlements* to those commitments. For instance, had the speaker's ascription been (1), then their own commitments regarding the existence of philosophers, the list of the greats, and the possibility of philosophical tofu would have become immediately relevant.

(1) Regarding the greatest philosopher of language, Dan thinks that *he* was a piece of tofu!

Turning to subsentential cases, Brandom points out that such shifts in perspectival commitments can be indicated by operators such as "classified as", "described as", "conceptualized as", and (importantly) "referred to as". For instance, in saying "Jamal classified some food *as rabbit*," a speaker, Juanita, purports to indicate some food—the *de re* component of the ascription—and then says what concept Jamal applied to it (viz., RABBIT). The word 'as' marks the onset of the *de dicto* component, ensuring that Juanita does not commit *herself* to the correctness of Jamal's classification. (Perhaps she knows that that the stuff on the plate is tofu.)

Some theorists, having noted that RABBIT is the *only* concept that Juanita ascribes to Jamal, go on conjecture that Jamal can deploy this subsentential concept, *all by itself*, in classifying something as rabbit. Indeed, bewitched by surface grammar, some fail to notice the plain distinction between *what Juanita is doing*—i.e., describing (noncommittally) one *aspect* of Jamal's perspectival commitments—and *what Jamal is thinking*. A mistaken conflation of these two phenomena is what gives rise, I suspect, to the widespread illusion that we can take as it *as a datum* that each of us has ability to think of, classify, or refer to something, by deploying *just one subsentential concept* (or linguistic expression). I'll argue later, following Brandom, that this idea is doubtful; *prima facie*, one can neither classify nor think about something without tokening *complete thoughts*. For instance, the case described, Jamal's classificatory act requires tokening the *complete* thought, CCC IS-RABBIT, where CCC is whatever concept he uses in thinking about the food on the plate.

3.5 *Interpersonal similarity of meaning and content*

Many theorists hold, for reasons that are ultimately unpersuasive, that communication is a matter of passing a message, idea, meaning, or thought from speaker to hearer. As with throwing a frisbee, a successful case in one in which the item sent by the speaker is the very same as the one received by the hearer—if only in the ideal. But, after the mid-century work of Quine, Kuhn, Sellars, and many others who developed broadly holist ideas (e.g., Churchland 1979), it's hard to see this picture as anything but optional. Brandom's account of communication is one of the many that rejects it outright, modeling communication instead on activities like tango dancing, where partners has to give and take "in complementary fashion"; book-keeping, where each participant "keeps separate books" regarding her own commitments and those of other participants; and baseball games, where a common "scoreboard" shows what complex normative statuses each participant bears to each of the others.

Metaphors aside, Brandom's inferentialism carries an explicit commitment to *holism* about meaning and content. Many follow Fodor and Lepore (1992) in seeing this as the root of several major problems for his view. But the objections that Fodor and Lepore press are virtually all rooted in implausibly strong assumptions about the necessity of meaning/content *identity*—rather than mere similarity—for various philosophical inquiries. These include the projects of adequately characterizing successful communication, interpersonal disagreement, and rational belief revision. It seems to me that Brandom's accounts of these things are perfectly fine as they stand, but fans of holism seem to have gone scarce in recent years, and Fodorian views about meaning/content identity have arguably become the *received* views in the field.

In yet another clear case of both his iconoclasm and his significant convergence with Brandom, Pietroski likewise rejects the identity-of-meanings picture, though on grounds that are independent of any holist commitment. Targeting first the extensional account of meaning-identity as co-extensiveness, Pietroski points out that

> speakers can connect the pronunciation of 'pen' to the same meaning without determining a set of pens that they are talking about. If each speaker uses a polysemous word that lets her access a concept of writing implements, as opposed to animal enclosures, they resolve the *homophony* the same way and thereby avoid blatant miscommunication. In this sense, they connect a common pronunciation with a common meaning. But it doesn't follow that any speaker used a word that has an extension... (18)

Later, Pietroski counsels us—wisely, in my view—to give up the whole idea that "successful communication requires speakers [to] use the same meanings/concepts" (33), regardless of the theoretical framework in which this idea is couched (extensional or otherwise). He views it as a mere *idealization* that "members of a community have acquired the *same* language—or that they use the same words, or words that have the same meanings." Despite Brandom's focus on social norms, shared

commitments, and the like, what Pietroski says here is entirely in line with his view. This will take some spelling out, which I undertake in 4. For now, let me emphasize that, if correct, then Pietroski's take on this issue would severely undermine the (already fairly flimsy) arguments against meaning holism.

3.6 *The Pragmatics of Assessment*

We've seen that Brandom places great stress on the notion of assertion, and that he sees this as something that we should characterize in normative terms. Given Pietroski's naturalist commitments, one might think that he disagrees. But this overlooks the key point that I will go on to make in the remainder of this discussion—namely that the theoretical aims of Pietroski's semantic theory are so starkly different from those that animate Brandom's inquiry that their common use of folk terms like 'meaning' should not bewitch us into thinking that they're talking about the same phenomenon.

Pietroski's target throughout *CM* is not communication, but the underlying psychological mechanisms that make it possible. When he does provide hints of his broader views about communication, what he says is entirely of a piece with Brandom's normative pragmatism.

> My own view is that truth and falsity are properties of certain evaluative actions—e.g., episodes of assertion or endorsement—and the corresponding propositional contents that are often described with the polysemous word 'thought', as in 'the thought that snow is white' or 'the thought constructed by executing that instruction'; cp. Strawson. (63, fn. 23)

As with all theory-laden terms, including (especially in this context) 'thought', 'meaning', and 'concept', we should always remember that the aims and presuppositions of the inquiry are far more important to keep in view than the pronunciation of the jargon. Pietroski makes this point in the following passage.

> Let's not argue about nomenclature. One can use 'concept' more permissively than I do, perhaps to include images or other representations that are not composable constituents of thoughts. One can also use 'concept' less permissively, perhaps to exclude representations that fail to meet certain normative constraints. Or one might reserve the term for certain contents, or ways of thinking about things, as opposed to symbols that have or represent contents. But I want to talk about a kind of physically instantiated composition that is important for cognitive science, along with a correspondingly demanding though non-normative notion of constituent, without denying that contents and nonconceptual representations are also important. (77)

We will go on in 4 to compare Brandom's and Pietroski's notions of "constituent," where it will become important that Pietroski's aims is to provide a *non*-normative account of constituency. For now, consider how this plays out with respect to Pietroski's distinction between Olde and New Mentalese (2). Recall that Pietroski's account has children starting off with Olde Mentalese concepts that fail to meet the conditions for assembly by FL *Begriffsplans*, and later *inventing* new concepts that are

specially tailored for the job. For instance, Pietroski writes, "Ignoring tense for simplicity, we can form concepts equivalent to: GIVE(VENUS, __ , BESSIE); GIVE(VENUS, BESSIE, __); GIVE(__, BESSIE, VENUS); GIVE(__, BESSIE, __); etc. But children may start with composable concepts that are *less* cognitively productive" (101).

In the remainder of the discussion, I'll lean heavily on the following two key points. First, much of what Pietroski says about the phylogenetically older "Fregean" modes of thought that permit internal substitution is of a piece with the lessons that Brandom likewise draws from Frege. The second key point, which alone serves to resolve many of the apparent conflicts between the two views, is that Pietroski's account is best be viewed as a (partial) theory of the *subpersonal* mechanisms that underlie our norm-tracking abilities, which in turn contribute to making possible the social practices of communication. For instance, Pietroski's account of how a child comes to acquire the concepts that FL can assemble should, if correct, be regarded as laying down empirical constrains on the variety of norm-tracking social practices that a human child will master at various ages.

If the foregoing claims are correct, then the two theoretical enterprises not only share several key commitments, but they are actually *complementary*, each providing an important piece of the overall puzzle about how best to view language(s). While Brandom's inquiry is neither descriptive nor psychological, his account clearly presupposes that there must be *some* descriptively correct account of how any creature's sub-creatural cognitive architecture emerges, whether in early development or in its species' socio-evolutionary ("memetic") history. His claim is simply that such an account can't be the *end* of the story. Rather, he argues, it's the necessary groundwork for a much larger picture of the role of language in social practice.

Likewise, although Pietroski's focus is the psychology of the individual speaker-hearer—assumed to be universal within the species—we've seen that there's room for communal norms in his overall picture. Indeed, passages from *CM* contain explicit remarks about the norms that govern inquiry in the mature sciences—e.g., norms that serve to stabilize referential purport among expert chemists. Such norms, however, would reside largely at the level of *pragmatics*, which is decidedly not Pietroski's focus in *CM*, nor a major aspect of Chomsky's own work.

Perhaps, then, we can adapt David Marr's familiar distinction between "levels of analysis"—if only by crude analogy—by viewing Brandom as articulating a "high-level description" of the conditions that any language user must satisfy in order to count as such. If this suggestion is right, then Pietroski is best seen as telling us about the details of how this high-level description happens to be "implemented" or "realized" in the human case. In the remainder of this essay, I will argue that there is no inherent conflict between accounts pitched at distinct levels of inquiry. There could only be a substantive dispute if the they

shared a common domain. But, as Pietroski's remarks in the following passage make clear, that's just not so in the present case.

> Let me digress, briefly, to note a different response to concepts without *Bedeutungen*. One might adopt a hyper-externalistic understanding of 'ideal', so that no ideal thinker has the concept VULCAN, and no ideal analog of this concept lacks a *Bedeutung*; cp. Evans, McDowell. But then an ideal thinker must not only have her mental house in good order, avoid paradox-inducing claims, and be largely *en rapport* with an environs free of Cartesian demons. In this hyper-externalistic sense, ideal thinkers are immune from certain kinds of errors, perhaps to the point of being unable to think about the same thing in two ways without knowing it; cp. Kripke. One can characterize corresponding notions of concept*, language*, and their cognates. These notions, normative in more than one way, may be of interest for certain purposes. Perhaps inquirers aspire to have languages* whose expressions* have meanings* that are instructions to build concepts* that can be constituents of thoughts*. But my aim, more mundane, is to describe the thoughts/concepts/meanings that ordinary humans enjoy. And I see no reason to believe that the best—or even a good—way to study concepts or meanings is by viewing them as imperfect analogs of their hyper-externalistic counterparts, even if we can and should try to acquire concepts*. Notions like truth and denotation may be required to interpret philosophical talk of thoughts*/concepts*/meanings*. One can also hypothesize that ordinary words combine to form sentences that express thoughts composed of concepts that denote or "are true of " things in the environment. But this hypothesis does not become inevitable as soon as we talk about thoughts/concepts/meanings. On the contrary, natural concepts seem to be mental symbols that can be used to think about things, even when the concepts apply to nothing. And if words like 'give' are used to access naturally introduced concepts like GIVE(_), we must be prepared to discover that words are used to access concepts that fall short of certain philosophical ideals.

4. *Challenges for Ecumenicism*

The ground-clearing maneuvers of the previous section put us in a position to explore residual differences between Pietroski and Brandom that threaten to be more substantive. As advertised, I hope to show that the initial appearances are misleading even in some of *these* cases, contrary to received opinion amongst philosophers of language. Still, I'll ultimately admit defeat when we arrive at the very last topic—the banes of predication and singular terms.

4.1 *E-language*

One place to look for sources of substantive disagreement is in the vicinity of Chomsky's infamous distinction between E-language and I-language (Chomsky 1986). Chomsky initially drew this distinction with the explicit intention of formulating a key difference between his approach to language and that of Quine, Lewis, and others—what was then arguably the dominant view. Given that Brandom was a student of Lewis's and frequently pays homage to him in published work, the

question arises whether his view likewise falls prey to the compelling objections that Chomsky articulated decades ago. We can address this question by focusing on Pietroski's more recent formulation of Chomsky's insights.

Let's begin with the distinction itself. What exactly is an E-language supposed to be?

> Chomsky characterizes I-languages as procedures that connect interpretations with signals. Languages in any other sense are said to be E-languages. Chomsky … isolates a procedural notion and uses 'E-language' as a cover term for anything that can be called a language but isn't an I-language. Thus, E-languages may include certain clusters of behavioral dispositions, heuristics for construing body language, etc. But Chomsky's 'I/E' contrast does connote Church's intensional/extensional contrast, and sets of interpretation-signal pairs are paradigmatic E-languages. Though to repeat, such sets are often defined procedurally. (52)

This negative conception of E-languages ("anything that can be called a language but isn't an I-language") casts such a wide net that it *can't but* apply to Brandom. Certainly, the latter is making no substantive psychological hypotheses about a computational system in the human brain. Nevertheless, as we'll see, much of what Pietroski says in rejecting the study of E-language poses no conflict with Brandom's view. While he argues that David Lewis "wanted to describe our distinctively human language(s)," this is *not* the direction that Brandom takes. His project is to describe language *as such*, not "distinctively *human* languages."

Although my broader claims by no means hang on a particular interpretation of Lewis, it does seem to me that Pietroski's reading of Lewis is not maximally charitable. I say this not because I think that his deep disagreements with Lewis somehow insidiously creep into his interpretation. Rather, I'll argue that there's a way of seeing Lewis as engaged in a wholly different project from the one that Pietroski foists on him. Still, whatever the case about Lewis, the project that I have in mind is the one that Brandom *in fact* undertakes—which, in my view, he carries out successfully, even if his teacher did not.

The subsections that follow distinguish three separate points of debate: (i) extensional vs. procedural conceptions of grammar, (ii) a sentence-first vs. word-first approach to semantics, and (iii) individualist vs. social conceptions of language.

4.1.1 *Extensional vs. intensional constraints*

In getting more precise about Lewis's particular brand of E-language, Pietroski highlights two points: (i) the metaphysical claim that language(s) are extensionally-specified abstract objects—specifically, sets of meaning-pronunciation pairs—and (ii) Lewis's conception of the process whereby a population comes to *use* such an object in social exchanges.

> Lewis's proposal was that languages like English are sets [functions-in-extension, not procedures] that are related to certain social phenomena via conventions of "truthfulness and trust." He speaks in terms of populations "selecting" certain languages. The suggestion is that using a particular language is like driving on the right: an arbitrarily chosen way of coordinating certain actions. (55)

One of the main things that Pietroski finds problematic here is that treating natural languages as functions-in-extension requires relinquishing the explanatory ambitions of generative linguistics—specifically, the aim of describing the cognitive structure that underlies, or even *constitutes*, human linguistic competence. Lacking an account of the cognitive architecture of FL, or the representational format in which it conducts its business, we have no empirically credible story about how adults accomplish real-time language processing, not to mention how children acquire the creative capacity to produce and consume an indefinite range of novel expressions.

> Lewis says instead that "a grammar, like a language, is a set-theoretic entity which can be described in complete abstraction from human affairs." Chomsky offers a way of locating languages and grammars in nature. Lewis stipulates that each grammar determines the language (i.e., the set of sentences) that it generates, but that languages do not determine grammars. So even if there is a Lewisian grammar for a certain set of Slang sentences, this does not explain how the relevant sentence meanings are related to lexical meanings, or how speakers of the Slang know that the strings in question fail to have certain meanings. At best, a Lewisian grammar indicates how a certain kind of mind might abstract lexical meanings from sentence meanings, given hypotheses about the relevant constituency structures and composition principles; see chapter three. But this doesn't yet tell us anything about how humans connect lexical meanings with pronunciations, or how we can/cannot combine lexical meanings. (59)

This all strikes me as correct. What Lewis *should* have been aiming his inquiry at language *as such*, regardless of which creature is using, acquiring, or "selecting" it. Setting aside Lewis, Brandom's target explanandum is "what it is to do the trick," not "how the trick is done by *us*"—or, for that matter, by a dolphin, an AI robot, or a Martian. (I'll suggest a friendly amendment to this point in 4.2). Chomsky, Pietroski, and other generativists, on the other hand, want to know about humans *specifically*. These are different projects, to be sure, but they're not thereby rivals. Though they may well constrain one another, the relations between them can better be seen in terms of the "levels of analysis" picture that I proposed earlier.

In addressing Lewis's account of how populations "select" a language, Pietroski assumes that the target phenomena are sufficiently similar between his inquiry and Lewis's that the two are not only commensurable with one another, but are actually in direct conflict on various key points.

> Lewis asserts that £ is the language used in a population P "by virtue of the conventions of language prevailing in P;" where conventions, in his sense,

> are special cases of mutually recognized regularities of action/belief in a population of individuals who can act rationally. He says that a convention of " truth and trustfulness," sustained by " an interest in communication," is what makes a particular set of sentences the language of a given population. However, Lewis offers no evidence that this proposal is correct. And there is an obvious alternative: if a population P is small enough to make it plausible to speak of the language used in P, then the members of P will have acquired generative procedures that are very similar. So why think there are "facts about P which objectively select" a shared E-language, and not that members of P have similar I-languages? (60)

The answer that I suspect Lewis would give to Pietroski's last question in this passage is that there is no obvious way of individuating I-languages independently of the "forms of life"—i.e., communal practices—in which any linguistic creature is caught up. That's because a discursive creature's concepts and/or lexical items are constitutively related to the norms that structure those practices. Thus, if meanings really *are* instructions to fetch and assemble concepts, then we won't be able to individuate *meanings* without appealing to such norms *either*. It's not clear to me how Pietroski would (or could) respond to this point.

Pietroski's critique of Lewis—particularly, his troublesome notion of "selection"—is carried forward in the following passage. What's new here is that we have a direct quotation from Lewis making precisely the claim that I've been urging on his behalf—namely, that the generative grammarian's target *explananda* are simply *not the ones that* he *seeks to address*.

> According to Lewis, populations select languages by virtue of using sentences in rational ways. So on his view, if language use is not a "rational activity" for young children, they are not "party to conventions of language" or "normal members of a language-using population." [Lewis writes:] "Perhaps language is first acquired and afterward becomes conventional. . . . I am not concerned with the way in which language is acquired, only with the condition of a normal member of a language-using population when he is done acquiring language." I don't know what it is to acquire language in Lewis's sense, or how he would describe whatever creolizers acquire. But even if one wants to focus on "normal" adults, ignoring acquisition may not be a viable option for those who want to find out what Slangs and meanings are. Inquirers don't stipulate how phenomena are related; they investigate.

In the end, I agree with Pietroski that Lewis's talk of populations "selecting" languages is hard to take seriously, especially in light of the Chomskyan alternative. Casting things in Lewis's way plainly does leave the crucial process of "selection" unexplained—at least at the level of psychology, which is arguably where all the exciting action is at. Nor can I bring myself to credit the idea that the central aim of formal linguistics should forever be what Lewis said it should be—i.e., the extensional characterization of some abstract set (a moving target, as Pietroski points out, given the constant introduction of novel lexical items). But one can share Pietroski's doubt that there's any meaningful sense in which people "select languages", as well as his broader anti-

extensionalism, but *nevertheless maintain* that there's a level of theoretical abstraction at which language *is* best viewed as a distinctively social phenomenon.

The conventions that govern the phenomenon in question might be "truthfulness and trust", as Lewis thought, or they might be more explicitly normative, as in Brandom's conception of socially-instituted assertional and inferential commitments and entitlements. Likewise, the languages that such conventions institute might be static abstracta, as Lewis seems to have believed, or they might be flexible, dynamic, and highly context-dependent social relations, as on Brandom's account. The latter, moreover, has no theoretical use for the notion of "stable populations" that go around "selecting" various abstract objects—each of which somehow manages to answer to the label 'Spanish' (or whatever), despite their extensional differences.

There is, therefore, no call to reify such strange entities on the normative inferentialist picture. Neither linguistic phyla (e.g., Romance or Germanic) nor local dialects (2020 Boston English) need be real—not in *Lewis's* sense, anyway—in order for Brandom's project to get off the ground. Indeed, as previously mentioned, the "communities" that Brandom appeals to in his account of norm-governed social practices can be as small as *two creatures*. (Actually, a footnote in *MIE* reveals that Brandom might even be willing to countenance something like an "I-thou relation" coming to be instituted by temporally asymmetric recognition of authority and responsibility relations between *distinct time-slices of one and the same creature*.) Thus, in principle, every new dyadic social interaction can serve to institute novel *local* norms, alongside any that were previously shared. Whatever else Brandom might be accused of doing, then, attempting to individuate and reify stable (let alone timeless) public languages is no part of his brief.

4.1.2 *The primacy of sentences*

Having argued against Lewis's *metaphysical* assumptions, Pietroski goes on to take issue with his *methodological* claim that semantic inquiry should begin by assigning meanings to sentences, rather than to subsentential expressions.

> Lewis goes on to say "if σ is in the domain of a language £, let us call σ a sentence of £." But he wasn't using 'sentence' as a technical term for any string to which a language assigns a meaning. Rather, Lewis initially restricted his notion of a meaningful expression to sentences. He later introduces talk of word meanings via talk of grammars. (55)

As we saw in our earlier discussion of Pietroski's views on sentences, he views this sort of approach as being out of step with contemporary theorizing in generative linguistics.

> Linguists have since replaced "S" with many phrase-like projections of functional items that include tense and agreement morphemes, along with various complementizers. This raises questions about what sentences are, and

> whether any grammatical notion corresponds to the notion of a truth-evaluable thought. But theories of grammatical structure—and to that extent, theories of the expressions that Slangs generate—have been improved by *not* positing a special category of sentence. So while such a category often plays a special role in the stipulations regarding invented formal languages, grammatical structure may be independent of any notion of sentence. (61)

Here again, we must distinguish Lewis from Brandom. For the latter, the primacy of sentences for is not a mere stipulation, nor an irrational fetish for a specific syntactic type. Rather, his principal—and I think quite principled—grounds for isolating sentences at the outset of a normative pragmatic inquiry is that this is the only type of expression with which a can make an *assertion*—i.e., an explicit move in an norm-governed *inferential* practice. Brandom's concern with the normative structure of this "game of giving and asking for reasons" is what motivates his focus on the role that assertions play, both in reasoning—as premises and conclusions—and also in communication. In the latter context, they serve the function of allowing speakers to undertake normative statuses—paradigmatically, commitments and entitlements.

Pietroski's methodological counsel is to postpone discussion of communication to a later day. As he remarks, specifying the structure of FL is work enough for one lifetime—surely more. But it doesn't follow from this that a theoretical inquiry with *different* aims must be, in some sense, second-rate, let alone illegitimate. Nor is it clear that the two inquiries are even in competition with one another over how to best to describe a common subject matter. As noted above, their common talk of "meanings", "languages", and the like might tempt one into thinking that the topic under discussion is the same for both theorists. But that would be a mistake. The two theoretical frameworks in which these terms are couched—and relative to which they have their theoretical import—are so dramatically different in respect of their explanatory aims that viewing them as even *purporting* to refer to the same phenomena is a bit of a stretch.

Unless I'm mistaken, Pietroski himself seems to have fallen into this trap, despite his earlier counsel to avoid such temptations. One clear place where he does so is in the following passage, which is notable for including—now for a second time—Lewis's direct protests against being saddled with the views that Pietroski nevertheless goes on to attribute to him.

> By contrast, Lewis held that sentences are prior to grammars. While granting that we should not "discard" notions like phrasal meaning (relative to a population P), or the "fine structure of meaning in P of a sentence," he says that these notions "depend on our methods of evaluating grammars." … For Lewis, a grammar Γ is used by P if and only if Γ is a best grammar for a language £ that is used by P in virtue of a convention in P of "truthfulness and trust" in £. One might have thought that a "best" grammar for the alleged set £ would be one that best depicts the procedures acquired by members of P. But according to Lewis, "it makes sense to say that languages might be used by populations even if there were no internally represented gram-

> mars." He then makes an even more remarkable claim. "I can tentatively agree that £ is used by P if and only if everyone in P possesses an internal representation of a grammar for £, if that is offered as a scientific hypothesis. But I cannot accept it as any sort of analysis of "£ is used by P", since the analysandum clearly could be true although the analysans was false." Note the shift from Lewis's opening question—what is a language?—to a search for an analysis of what it is for a language to be used by a population. … This shift is interwoven with the insistence that languages are sets of sentences, and a willingness to accept the consequences for how grammars are related to non-sentential expressions.

What Pietroski describes here as a "shift" in Lewis's explanatory aims strikes me as a correct description of what Lewis was up to all along; or, at any rate, *should've* been.

Setting aside Lewis exegesis, we can turn again to Brandom, whose methodology is a good deal more perspicuous. As already noted, Brandom rejects the "set of sentences" conception of languages, tying social norms to practices that are fluid, socially distributed, and highly context-dependent. Nevertheless, he can accept Lewis's views (quoted in the passage immediately above) about grammars and about the status of subsentential expressions. Again, that's because he motivates the primacy of sentences (or "full thoughts") by reference to their roles in assertions, and hence in the norms governing the social practices in which assertion is a basic move. Such a practice need not be as articulated as ours, in respect of either content *or* syntax. And while Pietroski is prosecuting an empirical inquiry into *human psychology*, Brandom's is assaying the pragmatics of *assertion*, *inference*, and *assessment*.

Here is one final bit of textual evidence for my suggestion that Chomsky and Pietroski, on the one hand, and Lewis and Brandom, on the other, are simply talking at cross purposes. Pietroski writes:

> I think [Lewis's] ordering of priorities is misguided. Slangs are child-acquirable generative procedures that connect meanings with pronunciations in ways that allow for constrained homophony. So whatever meanings are, there are natural procedures that connect them with pronunciations in specific ways. Instead of adopting this promising starting point for an empirically informed discussion of languages and meanings, Lewis offered a series of stipulations. Many others followed suit. But that doesn't make it plausible that Slangs are sets. And if Slangs are I-languages in Chomsky' s sense, then we shouldn't ignore this fact when asking what meanings are. … [Lewis] adds that "the point is not to refrain from ever saying anything that depends on the evaluation of grammars. The point is to do so only when we must, and that is why I have concentrated on languages rather than grammars". But in reply to a worry that he is needlessly hypostatizing meanings, he says "There is no point in being a part-time nominalist. I am persuaded on independent grounds that I ought to believe in possible worlds and possible beings therein, and that I ought to believe in sets of things I believe in." So why be a part-time grammarist, given Chomsky's reasons for thinking that children acquire generative procedures? Quine's worries about the "indeterminacy" of meaning are not far away. But while Lewis speaks of evaluating grammars, he does not engage with Chomsky's notion

> of an evaluation metric, or the correlative notion of "explanatory adequacy".

I suspect that the reason Lewis would give for being what Pietroski disparagingly calls a "part-time grammarist" is that he is—as he says repeatedly—*not* concerned with specifically *human* languages, but with language *as a general phenomenon*. This, in any event, is the line that Brandom takes. And while Pietroski's arguments in favor of his semantic proposal are compelling in the context of *his* particular brand of inquiry, it's difficult to make sense of his idea that a different "ordering of priorities" can be "misguided". At worst, an ordering of priorities—i.e., the adoption of some concrete set of methodological maxims, descriptive aims, and explanatory ambitions—can fail to be illuminating about the domain that it carves out for itself. But it's hard to see how this charge can be credibly leveled against Brandom's inquiry, the results of which many philosophers—the present author included—find deeply revelatory of communicative linguistic practices.

4.1.3 *Public languages, dialects, and I-languages*

Another context in which the notion of E-language has been invoked is to account for how folk terms like 'Italian' and 'Swahili' manage to pick out something in the world. Pietroski takes a dim view of this theoretical aim.

> we can describe many I-languages as English dialects (or idiolects), without English being any particular language. Prima facie, there are many ways to be a speaker of English: American, British, and Canadian ways; young child ways, adult scientist ways; etc. Being a speaker of English seems to be a *multiply realizable* property whose instances are similar in ways that matter for certain practical purposes. We can use 'English' to group certain I-languages together, perhaps in terms of paradigmatic examples, an intransitive notion of mutual intelligibility—think of Brooklyn and Glasgow—and historically rooted dimensions of similarity. There need not be an English language that each speaker of English has imperfectly acquired; cp. Dummett. We can use 'Norwegian' similarly, and classify both Norwegian and English I-languages as Germanic, without supposing that Germanic is a language shared by speakers of Norwegian and English. Analogies between linguistic and biological taxonomy can be preserved, whatever their worth, by thinking of specific I-languages as the analogs of the individual animals that get taxonomized—with 'Human' as the most inclusive category, and 'Indo-European' indicating something like a phylum.

Turning to Brandom, we note once more that he is not seeking to taxonomize the natural languages of the human species. His theoretical aims do not require reifying or hypostasizing the norms that govern specific discursive interactions, nor individuating the entities that allegedly answer to the names 'English' or 'Norwegian'. There seems to be no principled reason why Brandom couldn't countenance Pietroski's eminently reasonable view on these matters. Thus, when Pietroski describes a hypothetical theorist who "grant[s] that children acquire I-languages, yet maintain[s] that Slangs are E-languages that connect pronunciations

with extensions of idealized concepts," the theorist he is describing is not Brandom. If the latter were in the business of theorizing about Slangs, in particular, then he might indeed take up the hypothetical proposal that "each Slang is a social object that certain speakers acquire by internalizing a generative procedure and meeting some *further* conditions." But no part of his actual view hangs on whether "English", "British English", or "Germanic" name metaphysically real entities, let alone ones that are individuated extensionally. Nor, again, is his theory a descriptive psychological one. And it most certainly has no truck with extensions.

We can illustrate all this more clearly by considering a contrast that Pietroski draws between what he calls E-NGLISH (an E-language) and NGLISH (an I-language).

> One might describe E-NGLISH in terms of the strings that certain people *could* understand as sentences, and the meanings they *could* assign to those strings, given a certain dictionary and a suitably idealized sense of 'could'. But this presupposes that a competent speaker of NGLISH has an expression-generating procedure whose lexicon can be expanded. So we don't need to invoke E-NGLISH to say what NGLISH is.
>
> One can stipulate that if Amy, Brit, and Candice speak English, there is something that Amy, Brit, and Candice speak. But then the "thing spoken" may be a class of I-languages. One can stipulate that people share a language if and only if they can communicate linguistically. But then "shared languages" may not play any role in *explaining* linguistic communication. Sharing a language, in the stipulated sense, may be a matter of using similar I-languages in combination with other human capacities and shared assumptions about potential topics of conversation.

These, I maintain, are all claims that Brandom should be happy to accept. True, he holds that there are social norms governing the communicative exchanges of creatures who can understand and produce indefinitely novel constructions. But he should have no qualms with the claim that humans happen to do this via some subpersonal psychological apparatus. Plainly, his account must presuppose that there is at least one way of "doing the trick"—our own—though it leaves room for others.

Ultimately, as I've emphasized throughout this discussion, the *appearance* of friction is, here as elsewhere, rooted in a cluster of verbal disputes. Underlying these is the fact that Brandom's normative inferentialism, while sharing many homophonous pieces of jargon with Pietroski, is pitched at a *higher level of description*, so to speak, than a cognitive theory of NGLISH. Thus, Brandom should grant that NGLISH—the psychological apparatus that a given speaker possesses—can be specified without any recourse to E-NGLISH (if such a thing can even exists in a sense that he would countenance).

Note that, if Brandom's inferentialism is on the right track, then the "shared assumptions about potential topics of conversation" that Pietroski mentions are *partly constitutive* of the discursive norms that enter into an illuminating pragmatic account of communication. Need-

less to say, these not be the only things that enter into such an account. At the level of phonological and morpho-syntactic processes, as well as the ability to access and assemble concepts, the computational—and, ultimately, neurocognitive—explanation will surely be couched in the terms that generativists recommend. These are arguably subpersonal processes, which, again, is not the theoretical "level" at which Brandom's account is pitched.[2] A substantive disagreement can only be maintained if both inquiries have a shared target; this is, once again, not such a case.

Still, I think it must be admitted that a large part of what makes a cognitive theory of NGLISH theoretically interesting is that it enters into a larger account of how a specific creature—the human animal—is able to track, follow, articulate, challenge, reject, and revise the norms of discourse that have contingently arisen in its social *milieu*. This is not the only source of theoretical interest, of course. The biologically-realized combinatorial principles that constitute NGLISH are a marvel of neurocognitive information-processing, and should be studied by natural science as such, with the fascination that grips empirical linguists and neuroscientists alike. Nevertheless, the internal operations of this apparatus would be of no *adaptive* use to a creature if they made no contribution to the shaping of a broader class of norm-governed social activities. And, biology aside, the beauty of the mechanism is only enriched, not diminished, when we take into account the social interactions that it makes possible.

Famously, Chomsky (2016) rejects the idea that language is an adaptation specifically designed for social/communicative purposes. He entertains an alternative hypothesis to the effect that the *narrow faculty of language*—what Pietroski is here calling 'NGLISH'—was initially an aid to individual thought, making it possible for recursive structures in the mind to be composed, and thus "entertained". This hypothesis strikes many (including myself) as implausible, but even if it turns out to be true, Brandom's claims would still hold. That is, it would remain the case that the creature's newly-structured individual-level thoughts played a role in its profound re-shaping of the prevailing social norms. Had its thoughts or judgments—i.e., its normatively evaluable commitments to things being thus-and-s0—not *somehow* become entwined with broader social practices, they would not thereby have been *commitments*, whatever else they might have been. This is because, lacking a social existence, there is no normative check on what the creature had committed itself *to*. The conceptual contents of the internal states of a solitary creature would thus be underdetermined to such an extent that it may be more accurate, not to mention fruitful, to view them as *subpersonal* states—or, at any rate, as something other than judgments (*strictu dictu*). But this reeks of the kind of terminological legislation that both Pietroski and Brandom repeatedly warn against, so I'll leave the matter there.

[2] I spell out what I mean by "subpersonal" in Pereplyotchik (2017: 7.3)

Putting aside terminology, what Brandom is concerned to articulate is the general structure of pragmatic social statuses—e.g., commitment and entitlements—and how the norms that govern those statuses can be used in theorizing about the semantics of linguistic expressions in any language. The project is "general" in the sense that it designed to be applicable to *any* case of linguistic communication, not just the human case. How the norms of communication that we find in humans today might have evolved in the distant past of our particular species is, once again, no part of Brandom's explanatory target. Clearly, they emerged somehow, and the empirical story is bound to be fascinating. But all Brandom really needs to get *his* project going is the very broad claim—empirical, though perhaps only by a courtesy—that *some* natural psychological mechanisms underlie, and thus explain, in the descriptive naturalistic sense, any given creature's facility with social norms.

One might suspect that there is nevertheless a substantive *metaphysical* dispute in the vicinity. Brandom posits public linguistic norms, whereas Pietroski has no truck with public entities of any kind. But matters aren't so clear. Consider how Pietroski characterizes two hypothetical ontological views about what NGLISH really is: "We can identify NGLISH with an I-language, or perhaps a class of I-languages that differ only in small ways that are irrelevant for the purposes at hand." But what the difference could there possibly be between positing an E-language and positing "a class of I-languages that differ only in small ways that are irrelevant for the purposes at hand"? What ontological payoff, that is, can there be for a theorist who insists on I-languages *and resemblance classes thereof*, as opposed to public languages—or, at any rate, theoretically useful public linguistic entities (TUPLEs)?

Pietroski hints that the issue may have to do with other metaphysical features of E-NGLISH and NGLISH, including their *modal* properties: Whereas "a Slang seems to have its composition principles *essentially*, … E-NGLISH includes no composition principles; the set contains only string-meaning pairs, atomic and complex." The difference is that sets are individuated by their members, but "any initial list of atomic expressions can be updated." Pietroski's point, if I understand it correctly, is that the ongoing process of language change creates a moving target for semanticists like Lewis. As new lexical items emerge (or "go extinct") in the actual world, the set that such a theorist intends to pick out changes. Indeed, Pietroski quips that "[i]dentifying Slangs with sets of expressions is like identifying animals with sets of molecules, and insisting that growth be described as replacing one animal with another. Even if this metaphysics is coherent, it may not cohere with plausible biology and linguistics" (57).

It isn't clear, though, why this sort of consideration couldn't be pressed just as hard with regard to I-languages. Although Pietroski says that "a Slang seems to have its composition principles *essentially*," he plainly acknowledges that I-languages, conceived of as a psychological procedures, can *also* be updated. For instance, "communicative

failures can lead children to modify their (still modifiable) procedures for connecting meanings with pronunciations, subject to constraints" (58). Indeed, it seems to be an empirical hypothesis whether all of the elements of Pietroski's *own* cognitive/semantic proposal in *CM* come online in the child's I-language at the same time. Perhaps I-languages initially allow only instructions for combining monadic concepts, and only add (limited) dyadicity later in the maturational process. If the latter, then we might ask, "Is this a *new* I-language?" More importantly, how could we tell?

On the assumption that I-languages have their lexical entries and composition rules essentially, we can adapt Pietroski's animals/molecules analogy, seeing a child's Slang as a succession of distinct I-languages—one for each day, week, month, or year. This entails that a child's Slang over the course of a year can be a set of significantly different I-languages. Though it is doubtless amenable to empirical inquiry, the question, "How many I-languages per day?" seems at best metaphysically awkward. Similarly, Pietroski holds that "dialectical variation … makes appeal to a single set of English expressions seem silly." Rightly so. But, again, the same considerations apply to the I-languages of speakers who are competent in many "dialects" or "languages" (as the benighted folk call them).

What are the individuation conditions on I-languages, in light of these considerations? In Pereplyotchik (2017: ch. 3), I argued that the answers to such questions are not much clearer in the case of I-languages than for the case of E-languages. I suspect that it will remain so for the duration of sustained inquiry in the decades to come.

4.2 *Philosophy has not failed cognitive science*

In a recent paper, provocatively titled "Why Philosophy Has Failed Cognitive Science," Brandom argues that analytic philosophy, exemplified in the work of Frege, has devoted a great deal of energy to clarifying the nature of logical and semantic notions, but that we've thus far failed to properly hand off the fruits of our heritage to researchers in cognitive science.[3] The present section is devoted to a survey of the claims that Brandom makes about this alleged failure. I'll argue that Pietroski's work provides a direct counterexample to several of these claims, but that Brandom is right to point out that many theorists, in-

[3] Recognizing that cognitive science is comprised of many fields, Brandom aims his criticism more directly at philosophers who work on topics in cognitive psychology, developmental psychology, animal psychology (esp. primatology), and artificial intelligence, *rather than* at those who study topics in neurophysiology, linguistics, perceptual psychology, learning theory, and memory. Admittedly, this is a strange way to cut up the terrain. In particular, for our purposes, it's not at all clear why philosophers of *linguistics* are not on Brandom's list of targets. But let's not dwell on this. If only for the sake of furthering our present inquiry, I'll include philosophers and language and linguistics in the list, making no exception for myself.

cluding Pietroski, hold commitments that Frege's insights should lead us to reject.

4.2.1 *Modes of inquiry, philosophical and scientific*

Brandom recounts the way in which modern approaches to logic and semantics began with Frege's *Begriffsschrift*, which furnished us with a new logic and new ways of thinking about meaning. Russell then showed us how to apply these ideas more generally in philosophy. But, while the ideas that Frege and Russell developed about logic and semantics were quite *general* in their import, later theorists to attempted, with variable success, to apply those general ideas to the specific case of natural language—i.e., the system of representation that normal human children acquire. This, no doubt, has to do our sheer familiarity with the only clear case of language use available—i.e., our own—coupled with the anthropocentrism that motivates any inquiry into language. The resulting confusion had the effect of blurring the lines between a general philosophical theory of language, on the one hand, and an empirical linguistic inquiry into the special case of human linguistic competence. That, Brandom maintains, is a mistake. On his view, the kind of inquiry that Pietroski is engaged in deals with the contingencies and the specifics of how humans acquired conceptual and linguistic abilities. Philosophy, by contrast, deals with the "normative" question of what *counts* as "doing the trick" for *any* creature.

Although I've followed Brandom in putting the point this way through the discussion so far, I must now register that this is not, in my view, the best way of saying what I think Brandom intends to say here. At any rate, it's not, by my lights, the point that he *should* be making at this juncture in the dialectic. For, one might legitimately wonder how "What counts as doing the trick?" gets to be a *normative* question—whether in the case of language or of anything else—rather than a straightforward question of fact. Presumably, "What counts as being a horse?" is not a normative question, for the simple reason that horses are a natural type of object, studied as such by zoologists.[4] Analogously, what counts as mastering a language may well be a matter of having acquired an I-language, in Pietroski's sense. As a friendly amendment to Brandom, then, I will address this worry on his behalf by re-iterating and fleshing out the proposal that I floated earlier in the discussion, regarding "levels of analysis".

As I've noted, what Brandom (and probably Lewis) has on offer seems to be a *high-level description* of a theoretically interesting kind of social practice—specifically, ("what counts as a") *language* game—

[4] A theorist sympathetic to Brandom might reply that the notion "counts as" is normative because it's a matter of what competences and abilities it is *appropriate* to ascribe to a creature. But, here again, a parallel move can be made in the case of horses, vis-à-vis the properties that are *correctly* ascribed to *them* (notably, the property of *being a horse*).

with all of the impressive social, practical, and cognitive benefits that make this kind of practice worthy of careful study. Correspondingly, I suspect Pietroski's proposal has its home in an inquiry pitched at a lower level of analysis. To flesh the picture out further, it will be useful to quickly rehearse a central tenet of the mainstream approach to "levels of analysis" in contemporary philosophy of science.

A view that posits multiple levels of theoretical analysis, whether in biology, computer science, or in the sciences overall, is not thereby committed to any particular story about how theoretical progress at any one level can, should, or must constrain theorizing at any other. True, the early proponents of a "levels of science" picture also attempted—unsuccessfully, as it turns out—to secure a "unity of science" thesis. But later thinkers, notably Fodor (1975), generously disabused us of these lofty goals. What we know now is that theoretical pressure can and often does "go both ways", with higher and lower levels informing one another in equal measure, and with equal authority. Lower levels, as such, are no longer seen as having an inherent epistemic privilege. This is a point that Chomsky, too, makes frequently.

Similarly, we can now appreciate the fact—poorly understood until fairly recently—that theories at different levels of inquiry are often to a large extent *independent variables*. A theorist who formulates a high-level analysis of some phenomenon typically assumes—often with good grounds—that the generalizations they discover at *that* level might be implemented in any number of ways by lower-level mechanisms. That's one half of the independence claim. The other half is best appreciated from the perspective of a theorist working at the (relatively) *lower* level of analysis. From *this* vantage point, the mechanisms, laws, generalizations, and/or principles that are discovered, however "abstract" they might seem, are assumed to be just one instance of an even *more* general phenomenon—a token of a potentially much larger type.

What I want to recommend is that we apply these general considerations from the philosophy of science to the concrete case of generative linguistics and normative inferentialism. Although it would be misleading to say that the subject matters that these two research programs seek to address are literally *orthogonal* to one another, the grain of truth in that bit of imagery is this: Brandom high-level account is, as such, indifferent to how lower-level mechanisms might operate in various token instances. Pietroski, on the other hand, is assaying the fine-structure of the *lower*-level mechanisms, but only in the special case of human languages. As such, while the results of his inquiry are *relevant* to Brandom's overall picture—indeed, they might pose devastating problems for Brandom (see below)—they function in practice *not* as substantive theoretical constraints, but as an account of a very special case (particularly to *us*!) of the kind of story that Brandom presupposes can be told for *any* creature to which his normative pragmatic account is applicable.

If this is so, then how do we make sense of the fact that Brandom, like Lewis, explicitly appeals to data from natural language in motivating his analyses of phenomena that are, at least in principle, specific to *our* way of doing things? If the theory is not intended to be a contribution to a "merely parochial" inquiry about *us*, then why use examples from *our* language—indeed, almost exclusively from English, in particular—in constructing and developing it? There are two complementary ways of answering this question. The first, already mentioned, is to point out that Brandom draws our attention to features of human languages (in practice, just English) *not* for the purpose of displaying empirical data that his account can explain, but, rather, to illustrate aspects of language that he believes have pragmatic or semantic analogues in languages beyond the human case. (By analogy, think of the Chomsky hierarchy.)

The second prong of the reply consists of highlighting the fact that Brandom has devoted much time and effort to arguing—ultimately persuasively, in my view—that many of the linguistic devices he treats in his work are actually *universal* features of language *as such*. Moreover, as many generative linguists have pointed out in discussions of Universal Grammar, language universals need not be categorical; they can, instead, take a conditional form, e.g., "any language that has feature F will also have property P," or "if a language can express content C, then it can also express content C*." Brandom (2008) works out a detailed typology of such relationships between logically possible languages, including those that differ either in respect of their general expressive power, or in respect of more specific semantic devices (e.g., deixis). He takes this to be a pragmatic-*cum*-semantic version of Chomsky's famous analysis of the syntactic hierarchies of expressive power.

The view that Brandom promotes throughout his discussions of this topic is that traditional philosophers of language, starting with Quine, directed their efforts at analyzing linguistic constructions that, by and large, shed light on quite general semantic phenomena—i.e., ones that we can hope to one day discover in other species (terrestrial or otherwise), or to build into our intelligent robots. Although such linguistic devices might seem, from the perspective of a modern-day linguist, to comprise a rather motley collection—why propositional attitude reports but not, say, ergative verbs?—the tie that binds them, according to Brandom, is one that we can best appreciate from the vantage point of a (high-level) normative inquiry into general pragmatics. The linguistic phenomena that Quine and others identified early on as being particularly germane to philosophy all have this in common: for each of them, there are good reasons to think that it's *not* just something we *happen* to find in distinctively human languages, but something that tells us about what a language *is*, irrespective of which creatures happen to use it or what subpersonal mechanisms they deploy in doing so.

4.2.2 *Frege's insights*

The lessons that Brandom believes philosophers have failed to pass on to their colleagues in the sciences pertain to four key distinctions, all due to Frege, between (i) labeling and describing, (ii) freestanding and embedded content, (iii) content and force, and finally (iv) simple vs. complex predicates.

The last of these, Brandom argues, opens up a semantic hierarchy that is no less important for cognitive scientists to be familiar with than the syntactic hierarchy that bears Chomsky's name. Taking this hierarchy into account in the context of empirical theorizing would help, he claims, to characterize the phylogenetic and ontogenetic development of linguistic and conceptual capacities. Such a characterization would move upward through what Brandom thinks of as "grades of conceptual content", including the propositional variety, the quantificational refinement, and ultimately the *relational* contents that Frege taught us to recognize.

We saw in 2 that Pietroski has a great deal to say about this. Indeed, the Fregean considerations that he surveys in the service of an avowedly naturalistic theory in cognitive science are precisely those that Brandom recommends to our attention (and *then* some). For Brandom, Frege's insight is that there are patterns in sentences that *cannot* be modeled as mere part-whole relations. For instance, although there is no expression that appears in "Herbie admires Jessica" and "Jessica admires Herbie" that doesn't *also* appear in "Herbie admires Herbie", the latter sentence exhibits an inferential pattern different from the other two—the pattern that we gesture at by employing notational distinctions between, e.g., admire(*x,y*) and admire(*x,x*), or by making explicit their inferential proprieties by embedding them inside of conditionals, as in (2) and (3).

(1) If someone admires anyone, then someone admires someone. (true)

(2) If someone admires anyone, then someone admires *themselves*. (false)

Thus, admire(*x,x*) expresses a kind of predicate that is not a *part* of a sentence, but an *aspect* of it, which we can recognize as an "inferential pattern" and *model* as an equivalence class of sentences. Frege's device of function-application is a way of capturing this idea. Functions are not, in general *parts* of their outputs. (The function *capitol-of(x)* yields Kiev when applied to Ukraine, but neither *capitol-of(x)* nor Ukraine are parts of Kiev.) This is why sentential connectives can be modeled with Venn diagrams, but complex predicates cannot. Even the simplest mathematics uses complex predicates—e.g., *natural number* or successor(*x, y*)—and Frege showed that, once you can build complex predicates, you can keep building endlessly more, in the manner we ran across in our discussion of Lewis's type-theoretic semantics (2).

As we've seen, Pietroski warns against taking for granted a creature's ability to construct concepts of unbounded adicities. But the warning is intended to apply *only* when doing natural-language semantics. For other purposes, Pietroski agrees that Frege's insights are of foundational and lasting importance. Moreover, the hypothesis that he develops posits thoughts that admit of a Fregean semantic treatment (perhaps even a truth-conditional one), but it requires these to first be converted, via Frege's process of concept invention, into the kinds of thoughts that are "legible", so to speak, to the human language faculty. While it's not clear what *independent* empirical evidence Pietroski might offer for positing psychological mechanisms that facilitate such a translation—I am aware of no obvious analogue in the case of other perceptual modules—what *is* clear is this: Brandom's contentions regarding Frege's distinction (iv), between simple and complex predicates, are rendered moot by the very existence of Pietroski's work, which presents an up-and-running empirical inquiry that is deeply informed by Frege's core contributions.

Matters are much less clear with regard to the other three distinctions that Frege was at pains to draw. Let's turn now to his distinction between labeling and describing.

4.2.3 *Sentences, predicates, and classification*

Brandom points out that old-school scholastic accounts of thought were rooted in a classificatory account of concepts—a relic of Aristotelian "forms". The medievals noticed that, once you have singular terms and classifications, you can build *up* to an account of truth, and then analyze good inference in terms of truth-preservation. Pietroski unabashedly endorses this strategy—in particular, the Aristotelian focus on classificatory concepts, which are central to his predicativism about New Mentalese (2).

This raises the question: What exactly *is* classification? How does a predicate get to perform its semantic function? Here is Pietroski's answer:

> …intuitively, a predicate classifies things, into those that meet a certain condition (e.g., being a rabbit) and those that do not. Anything that meets the condition satisfies the predicate, which applies to anything that meets the condition. We can invent perceptible predicates. Though for now, let's focus on predicative concepts, like instances of RABBIT. I assume that many animals have such mental predicates. … [A] predicate may apply to each of [several] things, or to nothing. But these are just special cases of classifying. …even if logically ideal predication is relational as opposed to classificatory, there seems to be a psychological distinction between relational and classificatory concepts, even if we speak of monadic/dyadic/*n*-adic predicates.

What I see, both here and throughout *CM*, are inter-definitions of semantic notions like "applies to," "classifies," "satisfies," and "meets conditions." Although Pietroski has made it clear that he is not trying to

"break out of the intentional circle," to use Quine's memorable phrase, the account he provides does not, to my mind, do much to illuminate the phenomenon in question. A diversity of labels allows us to conjure different clusters of theoretical intuitions. But none of these seems definite enough to make progress with.

Turn, then, to Brandom's answer, which has the virtue of laying out substantive proposals and refining them, arriving ultimately at one that meets various important desiderata. Like Pietroski, Brandom maintains that "classifying" is not the obtaining of some (super)natural relation between a concept and (a portion of) the actual world—let alone non-actual possible worlds. On his view, there are, instead, *acts* of classification—e.g., asserting "That's a rabbit," or tokening the corresponding perceptual thought ("Lo, it rabbiteth!"). We've already seen the details of Brandom's account of assertion, as well as his (subordinate) account of classification. Let's now approach the latter from a different direction, this time contrasting Brandom's view with extant rivals.

If asked, straight-out, "What is classification?," the knee-jerk response that most philosophers would offer is that classification is a matter of *differential responsiveness*. This is a start, but it leaves wide open the question of what vocabulary we're permitted to use in describing the objects, properties, and events to which a physical system might be differentially responsive. If we give ourselves free reign, then the notion becomes too cheap to do serious work; differentially responding to Italian and French operas would count as classifying them, regardless of how the trick was done. But, of course, one wants to know *how* that sort of thing happens, not just *that* it does. Unfortunately, pursuing the answer to this explanatory question by *restricting* our vocabulary to only naturalistically respectable terms quickly lands us with panpsychism—a bridge *much* too far. For, as Brandom points out, even a chunk or iron differentially responds to varying amounts of oxygen in its surroundings, e.g., by rusting.

Equally vacuous is the (unqualified) suggestion that we acquire predicative concepts, and hence classificatory powers, by performing a process of "abstraction" from either the intrinsic qualities of states of sentient awareness—as Hume, Russell, and Carnap all held at various points in their otherwise distinguished careers—or from the raw information supplied by sensory mechanisms, as naturalist like Neurath might have it. Without a detailed and well-motivated account of the operation of "abstraction", the acquisition of classificatory concepts has been labeled once more, but remains stubbornly unexplained.

To their credit, naturalists like Fodor and Dretske attempted to meet the problem head-on. Information-carrying states count as classificatory concepts, they argued, when they're embedded in suitably complex systems—ones that reliably keep track of their environment, learn, and behave flexibly, perhaps on account of their history of natu-

ral selection and innate resources. Burge (2010) adds to this list the requirement that the reliable tracking abilities must have the shape of perceptual *constancies*, not mere sensory registrations.

Brandom maintains that no such project can work, even in principle, precisely because it ignores Frege's conceptual distinction between mere *labeling* and full-blown *describing*. A case of labeling is one in which items are differentially sorted, but only extensionally, such that no specific inferential consequences can be drawn from the presence of the label. A magic wand might tell us that doorknobs, pet fish, and crumpled shirts are all and only the items that share the magic feature, F. But without knowing what F is, in intensional terms, we have no idea what, if anything, *follows* from the application of the label 'F'—i.e., what, in the fictional scenario, is semantically achieved by the activation of the wand. In order for this (or any other) physical signal to become more than a mere label, it must be inferentially articulated, in the sense that there have to be things that *follow from* something's being F, as well as things that can have an instance of 'F' in *their* inferential consequences.[5]

One of Frege's key lessons, then, is that inferential significance is central to conceptual content. Some concepts have only inferential conditions of application, not perceptual ones—either contingently, as with GENE, or necessarily, as with POLYNOMIAL or FRICTIONLESS PLANE. One can, of course, call things "concepts" even when they meet less stringent conditions. But, in that case, one should be sure to note the difference between differential responsiveness and inferential articulation. Moreover, these points hold irrespective of whether a differential-response capacity is innate or learned, and they apply just as much Boolean compounds of more basic units of differential responsiveness—i.e., *compound* labels.

While it's impossible to credit Brandom's claim that philosophers like Pietroski have taken insufficient notice of Frege's foundational insights, there is something to be said, I think, for his criticism on this

[5] Following Dummett's counsel, Brandom urges that we take into account *both* the circumstances *and* the consequences of applying a concept. For some nonsynonymous propositions, the antecedent circumstances coincide, but the inferential *consequences* serve to distinguish their contents. For instance, consider the contrast between "I will one day write a book about anarchism" and "I *foresee* that I will one day write a book about anarchism." The inferential antecedents ("circumstances") of these two claims might be the same, but the inferential consequences are different. This point applies even to observational concepts—e.g., MOVING or MOTION. A motion detector or a well-trained parrot that reliably emits the sound /*Moving*/ when there is, in fact, movement afoot (and not otherwise) does not thereby have the concepts in question. For although the circumstances of application are right, there are no inferential consequences to speak of in these cases. Brandom also makes the helpful observation that operators can serve to distinguish concepts that share both circumstances *and* consequences of application. For instance, the concepts HERE and WHERE-I-AM are shown to be distinct when interacting with the temporal operator 'always': "It's nice here/where I am" vs. "It is *always* nice here/where I am."

particular point. As we'll see below, Pietroski's views on classification don't seem to respect the distinction (whether it be Frege's or Brandom's) between labeling and describing. This has downstream consequences for Pietroski's view that really do seem to be out of step with Brandom's theoretical commitments.

The disagreement about classification is joined when Brandom asserts that *thinking about something*, as in "We're still thinking about his tax returns," is a matter of tokening *complete thoughts*—i.e., intentional states that can be expressed by linguistically competent creatures only in complete speech acts, which requires producing *complete sentences* (if only in the paradigmatic case). Pietroski, by contrast, follows the peculiar philosophical convention of using the phrase "thinking about" to denote a punctate event of conceptual classification. While he agrees with Brandom that having a thought requires tokening a "sentential concept", he also maintains that *all* concepts are "ways of thinking about things."

This is where Brandom would disagree, on account of his commitment to the effect that subsentential concepts are *not* complete thoughts. According to him, tokening such a concept cannot *by itself* constitute "thinking about something". To do that, subsentential concepts must (in some way) participate in a sentential one. So while sentential concepts are correctly described as "ways of thinking about things," Brandom follows Frege in viewing subsentential concepts as *aspects of* such ways. Thus, whereas Pietroski claims that "hearing 'Bessie' can… activate the denoter BESSIE, thereby leading [one] to think about Bessie in a singular way" (108), Brandom would deny that activating the denoting concept BESSIE can alone constitute thinking about Bessie—in *any* way—even *once*. This point about "denoters" applies also, *mutatis mutandis*, to predicative concepts.

Pietroski can, of course, *stipulate* that thinking about things doesn't require tokening complete thoughts. But it's difficult to see what could motivate such a move. Relying on brute introspection, one might fancy that singular reference has taken place with only one subsentential concept in play—e.g., "I'm quite certain that I was just thinking of tofu; not anything about it, specifically; just… *tofu*." However, such introspective judgments are known to be an extremely unreliable source of data, whether performed by naïve speakers or by theoreticians.[6] One might, more plausibly, appeal to the theory of *perception* developed by Burge (2010), according to which perceptual awareness involves the application of only *sub*sentential concepts, modeled on noun phrases. But this won't do, either. For, if judgments and classifications are all "sentence-sized", as Brandom argues, then even the *perceptual* mental attitude of *noticing* can't properly be treated as a case of applying just

[6] Distinct methodological troubles plague both of these two options, but they all strike me as insuperable and not worth discussing here. See Dennett (1991) for a primer.

one classificatory concept. Noticing rabbits involves *judging that* there are rabbits in the relevant spatiotemporal vicinity, and making such judgments requires deploying concepts other than the classificatory predicate, RABBIT(_)—e.g., the concept HERE(_).

With all this in mind, I think we should side with Brandom in saying that subsentential concepts *play a role* in acts of classification, where the latter are construed as either as public assertions or as inner endorsements of judgable contents. I see no reason to assume that tokening a subsentential concept is sufficient to carry off an act of classification. Nor is it obvious that classifying is a function of *all* concept application, as Pietroski believes. Does wondering whether Bessie exists really require *classifying* her? The latter question brings us face-to-face with the Fregean distinction between force and content, to which we now turn.

4.2.4 *Force and content*

Brandom draws our attention to an ambiguity that was long ago pointed out by Wilfrid Sellars—the so-called "-ing/-ed" ambiguity—which allows us to use words like "claim" and "thought" polysemously to describe speech acts and propositional attitudes in respect of their intentional contents, on the one hand, and in respect of their illocutionary force or "mental attitude type", on the other. With regard to the latter, Stephen Schiffer has popularized the imagery of different "boxes" in the mind—one that corresponds to the functional role of beliefs, another to that of desires, a third one for intentions, and so on. Pietroski likewise notes the distinction in the following passage from *CM*.

> One needs to be careful with the terminology, since words like 'thought' and 'concept' are polysemous with regard to symbols and contents; 'thought' and 'judgment' are also like 'assertion', which can be used to describe certain events that can be characterized in terms of contents. In speaking of a thought that Sadie is a horse, one might be talking about a mental episode, a mental sentence, or a content shared by various sentences.

Brandom goes on to argue that this distinction is not only useful for theorists, but that it also marks *a distinct level of conceptual sophistication*. Creatures who can tell the difference between the act of *asserting* and the content of what's *asserted* can be said to be aware, at least *implicitly*, of the force/content distinction. To make this awareness *explicit*, a creature can embed a sentence inside of a conditional, thereby stripping it of its force.[7]

Now, on the assumption that classification is, in fact, a kind of il-

[7] Brandom illustrates how conditionals can be used to distinguish those inferential consequences that derive from the *content* of what's said from those that derive from its *force*. Witness, for instance, the strikingly different inferential consequences of the sentences "p" and "I believe that p" when embedded as antecedents in conditionals: "If p then p" is obviously true for all values of 'p', but "If I believe that p, then p" is not foolishly arrogant for a mere mortal to assert, but also disastrously false in all known cases.

locutionary force, Brandom concludes that the assertion 'If F*a* then G*a*' cannot, in point of fact, be used to classify *a* as F, despite invoking both '*a*' and '*F*'.[8] This is another place where his views on the nature of classification come into conflict with Pietroski's. And, here again, I can think of no plausible way around it.

One might suggest, on Pietroski's behalf, that we seem to be able to simply *entertain a notion*—e.g., to contemplate "justice" or "the possibility of pigs flying"—without thereby committing ourselves to anything at all. This, Brandom points out, goes back to Descartes's view that one can first "entertain" an idea/proposition and then, *by an act of mental will*, either endorse or deny it, yielding either a committal judgment or a positive doubt. Pietroski's picture of concept-assembly likewise points in this direction. On that model, the process of assembly eventuates in the construction of a "polarized sentential concept", which is then shipped off to central cognition for endorsement, rejection, or further contemplation.

But this idea is at odds with Kant's equally compelling observation that concepts have contents only in virtue of their role in judgment. Pushing still further, Frege argued that entertaining propositions is a late-coming ability that involves a thinker embedding a proposition into the antecedent slot of a conditional—as in the following soliloquy: "What if *p*? Well, if *p* were the case, then *q* would also; but that would mean that neither *r* nor *s*…". If Frege's proposal is correct, then the ability to "entertain an idea" piggy-backs on two *prior* abilities—viz., to assert conditionals, and then to perform inferences that take *them* as premises or conclusions (e.g., hypothetical syllogisms).

Now, Pietroski agrees that the mental act of endorsement results in a committal judgment, which both he and Brandom take to be subject to normative evaluation—i.e., assessments of correctness, warrant, rational propriety and the like. But it's not clear how Pietroski's ⇑/⇓ operators for assembling polarized sentential concepts facilitate this act of endorsement. More generally, Pietroski's proposal seems to have little to offer in the way of a subpersonal about how *any* kind of force/attitude is superadded, so to speak, to polarized concepts, after the *Begriffsplans* get done assembling them.

4.3. *Predicates and singular terms*

We turn now to our very final topic, which concerns a foundational disagreement between Brandom and Pietroski on the nature of singular and predicative concepts. Recall that Pietroski's semantics for natural language is resolutely *predicativist*, in the sense that it recognizes no analogue of type-<e> expressions—intuitively, singular terms—i.e., no instruction for fetching singular concepts. Recall as well that he *does*

[8] Likewise, he warns against conflating denial and supposition—two kinds of force—with negation and conditionalization, which are semantic functions that directly participate in the content.

countenance the presence of such concepts in the human mind and that he recognizes the useful cognitive roles that such concepts play in thinking/reasoning. But this kind of cognition is couched in Olde Mentalese—the phylogenetically ancient representational format in which pre-linguistic thought was conducted, and which Pietroski thinks we still employ today, outside of language use.

Brandom develops a powerful argument to the effect that *any* language that fails to draw a distinction between predicates and singular terms is in principle barred from introducing basic logical operators—including both negation and the conditional. If this argument is successful, it would have no effect at all on Pietroski's claims about Olde Mentalese, which happily draws that distinction. But it would seem to present a rather major problem for Pietroski's main proposal about natural-language semantics, which has predicativism as one of its core commitments. So it behooves us, in surveying the points of discord between them, to focus on this foundational case, using it to draw out related points of contention about syntax.

4.3. *Brandom's argument*

"What are subsentential expressions?" and "Why are there any?" These are the two questions that Brandom raises in an essay of the same title (2001: ch. 4). In 1, we glimpsed the overall shape of his answer. Here, we'll reiterate the main points and look at some of the details. The reason for doing so is that this is the last—and arguably most challenging—of the issues that divide Brandom's normative inferentialism from the overall generative enterprise.

In the reconciliatory spirit of my overall project, I'll propose a possible strategy for ameliorating the dispute. But I should concede from the outset that this appears to be a particularly stubborn issue. This is frustrating, as the issue obviously cuts pretty deep. Having laid out the details of Brandom's difficult argument, I'll settle, in the end, for having *raised* the question—one that hasn't been discussed, to my knowledge, anywhere else in the literature—of how generative grammar might be (in)compatible with Brandom's substitutional approach to syntax.

4.3.1 *Details and a proof*

As noted earlier, Brandom agrees with Pietroski that discerning subsentential expressions is what makes it possible for us, both as theorists and as language users, to "project" proprieties governing the use of novel sentences. Once we've done this, we can then recombine subsentential items into new expressions, with meanings/contents that were previously inexpressible. Brandom recommends using the notion of substitution for this purpose, adapting Frege's insight that discerning meaningful subsentential expressions is a matter of treating sentences as *substitutional variants* of one another.

In spelling out the syntactic side of this technical notion, Brandom begins by identifying three "substitution-structural roles". These include the role of being an expression that is (i) substituted *in*, (ii) substituted *for*, and (iii) a substitutional *frame*. For instance, "David admires Herbie," is substituted *in* to yield a *substitutional variant*, such as "Herbie admires Herbie," where the expression 'David' has been substituted *for*. The residual substitutional *frame* is what's is common to the two substitutional variants—schematically, "*x* admired Herbie."

On the semantic side, a substitutional variant of a sentence will be defined in terms of the inferences that it enters into, as a premise or a conclusion. In keeping with his inferentialist project, Brandom develops the idea that the meaning of a subsentential expression consists in the materially correct *substitution inferences* involving that expression—i.e., inferences in which the conclusion is a substitutional variant of one of the premises. Thus, 'Herbie' has the meaning that it does partly in virtue of its role in a vast range of materially good inferences, including the single-premise inference from "Herbie barked" to "My dog barked" (as said by me).

With this in mind, Brandom notes that substitution inferences come in two flavors: symmetric and asymmetric. The above inference, from "Herbie barked" to "My dog barked", is symmetric, in the sense that it's materially good in *either* direction. Plainly, this trades on the identity between Herbie and my dog. This is more grist for Brandom's logical expressivist mill. He captures this observation by pointing out that identity is the logical notion that we use to *express*—i.e., make explicit—the *substitutional commitments* that are central to our notion of singular terms (and, relatedly, of the items they purport to denote). Contrast this with the inference from "Herbie runs" to "Herbie moves", which is materially good in only one direction, not in the other. That's because 'runs' is materially stronger, in respect of inferential consequences, than 'move'; the former licenses all of the inferences that the latter does, and then some. The distinction between symmetric and asymmetric inferential proprieties governing substitution inferences is, as we'll now see, the central aspect of Brandom's distinction between predicates and singular terms. Let's turn finally to his definitions of these two notions.

Each of the two definitions has a syntactic component and a semantic component. On the syntactic side, Brandom says that singular terms invariably play the substitution-structural roles of being substituted *for* (as well as *in*), whereas predicates invariably play the role of substitutional *frames*. On the semantic side, he points out that the substitution of singular terms is *always* governed by symmetric inferential proprieties, whereas predicates are *necessarily* governed by *at least some a*symmetric ones. For instance, 'Herbie' is a singular term partly in virtue of the fact that, if the substitution inference from "Herbie barked" to "My dog barked" is materially good, then so is its converse.

Crucially, the same does *not* hold for the substitution inference from "Herbie runs" to "Herbie moves", where the substitution of *predicates* is in play. That's, again, because 'runs' is inferentially stronger than 'moves'. This is an instance of something that Brandom goes on to argue is *constitutive* of predicates as a class—viz., that they *necessarily* enter into at least *some* asymmetric substitution-inferential relations with other predicates in the language.

Thus far, Brandom has supplied an answer only to his first question: what are singular terms (and, by extension, predicates)? To summarize, the answer is that singular terms play the *syntactic* roles of substituted *for*s and substituted *in*s, and the *semantic* role of entering *solely* into *symmetric* substitutional inferences. Predicates, by contrast, play the syntactic role of substitutional *frames* that *necessarily* enter into at least *some asymmetric* substitutional relations.

To ask why there are singular terms, then, is to ask the following question: Why do the syntactic and semantic substitutional roles line up as they do? This way of setting up the question allows us to generate a taxonomy of the logical possibilities, in terms of two binary parameters—syntax and semantics. We can thus imagine languages that instantiate the following four permutations.

i) Substituted *for* is *symmetric* and substitutional *frame* is *symmetric*.
ii) Substituted *for* is *asymmetric* and substitutional *frame* is *symmetric*.
iii) Substituted *for* is *asymmetric* and substitutional *frame* is *asymmetric*.
iv) Substituted *for* is *symmetric* and substitutional *frame* is *asymmetric*.

The option that's *actually* instantiated by singular terms and predicates is (iv). The question then becomes: What's "wrong" with the other options?

What rules out option (i), according to Brandom, is that, many of the substitution inferences that are to be codified and projected at the level of sentences by discerning subsentential expressions are *asymmetric*. No weakening inferences could be generated if *all* subsentential components were restricted *solely* to symmetric inferences. What the remaining options have in common is that they assign *asymmetric* inferential proprieties to expression-kinds that play the syntactic role of being substituted *for*. We can thus ask: what's wrong with *that* combination? The answer to this question is where things become technically challenging. Readers who feel like skipping ahead to the next section can take with them only the upshot of the proof: If a language fits the model of options (ii) or (iii), then it does not permit the introduction of conditional contents (contrary to fact, in our own case).

Brandom invites us to consider the generalizations that permit expressions with subsentential contents to determine the proprieties of a

productive and indefinitely flexible class of novel combinations. Asserting that Herbie is (identical with) my dog commits me to the propriety of *all* inferences of the form P(Herbie) → P(my dog). Similarly, for predicates; asserting that anything that runs thereby moves commits one to the propriety of all inferences of the form Runs(x) → Moves(x). This is why, when such content-constitutive and potentially asymmetric substitutional commitments made explicit, they take the form of *quantified conditionals*—another feather in the logical expressivist's cap.

Here, then, is Brandom's proof that options (ii) and (iii) in effect rob a language of its most basic logical notions—even ones as simple as negation and the conditional.

The pattern corresponding to the hypothetical *asymmetric* significance of "substituted *for*s" would replace identity claims with *inequalities*. Let "$t > t^*$" mean that P(t) → P(t^*) is in general a good inference, but *not* every frame, P, will make the converse inference, P(t^*) → P(t), materially good. Now, call a predicate Q an *inferential inverse* of a predicate P if, for all t and t^*, the following condition is satisfied.

Inferential Inverse $=_{df}$ if P(t) → P(t^*) holds, but P(t') → P(t) doesn't, then Q(t^*) → Q(t) holds and Q(t) → Q(t^*) doesn't

Thus, to answer the question of what's "wrong" with options (ii) and (iii), it suffices to show that if *every* sentential substitutional frame has an inverse, then there can be no *asymmetrically* significant substituted *for*s. The demonstration now proceeds by way of the following key lemma.

Lemma: In any language containing the expressive resources of elementary sentential logic, every predicate has an inferential inverse. Conditional and negating locutions are inferentially inverting; e.g., inferentially weakening the antecedent of a conditional inferentially strengthens the conditional. Thus, if condition the antecedent of *Inferential Inverse* holds, the then the consequent can be shown to hold as well.

Proof: Let Qα be defined as Pα r. It follows immediately that P(t*) →S(t*) entails P(t)→S(t), but P(t) → S(t) does not entail P(t')→S(t').

What this argument shows, if it shows anything at all (see below), is that conditional locutions are inferentially inverting precisely *because* they play the indispensable expressive role of making inferential relations explicit. (*Mutatis mutandis* for negation and other logical operators.) If this is right, then we can conclude, as Brandom does, that *any* language able to muster the expressive resources required for introducing basic sentential connectives will also draw a distinction between singular terms and predicates (as defined), assuming it has any subsentential structure at all. Conversely, any language that *forgoes* the term/predicate distinction is thereby severely castrated in its expres-

sive power—incapable in principle of introducing so much as a material conditional.

4.3.2 *Potential replies*

The foregoing argument was developed in Brandom's was presented in its canonical form in *MIE*. (See also the later and more condensed treatment in Brandom, 2001). In the decades since then, many theorists have marshalled a variety of technical objections against his line of reasoning. Some of these are based on straightforward confusions and can thus be defused without much concern (see *MIE*, ch. 6). Others might be more troublesome. Whatever the case about that, I want to ask what bearing this argument *would* have on Pietroski's position if it *were* successful.

As noted above, the argument appears to present a serious problem for Pietroski's commitment to predicativism, at least in the case of natural-language expressions and New Mentalese. (We can breathe easy about expressions of Olde Mentalese, which are in the clear.) How might Pietroski reply to this challenge? Closer to home, if this dispute can't be resolved, does that spell doom for my larger reconciliation project in the present essay?

One possibility is cut things off at the root by rejecting Brandom's substitutional approach to both syntax *and* semantics. Indeed, this is most obvious route for Pietroski to pursue, given his claim that we can't simply take for granted a creature's ability to "slice out" terms from sentences, so as to use them in combinatorically constructing an infinite hierarchy of semantic types, *a la* Frege or Lewis. Such a project, Pietroski argues, stipulates from the outset far more than it explains in the end. Suppose that he's right about this. Does that mean that his empirical results—assuming for present purposes that that's what they are—have literally *contravened* Brandom's strategy? Put another way, if generative linguistics is the correct approach to natural language, then are we barred from using Brandom's "substitutional scalpel" to identify subsentential structure, distinguish between singular terms and predicates, and carry off the inferentialist project at the subsentential level? I do not think so. Or, at any rate, I'm not convinced.

One excuse for my wavering on this point is that the considerations Brandom uses are so general—i.e., so totally independent of other details of the languages to which it applies—that it's hard to see which of them Pietroski is really in a position to deny. True, substitutional syntax smells a little too much like the old-school "discovery procedures" and "immediate constituent grammars" of benighted pre-Chomskyans (see Fodor, Bever, and Garrett, 1974 for a blistering refutation). But the methodological *similarities*, in my view, cut no ice. Nominalist discovery procedures, were, for all their shortcomings, *empirical hypotheses* about human languages. Otherwise, they wouldn't even *get* to be rendered false by straightforwardly empirical arguments. By contrast,

we've seen that Brandom's project, despite drawing on examples from English—again, *for illustrative purposes only*—is explicitly not pitched as an empirical inquiry into human language. So he can't be accused of attempting to resurrect that old idea.

Nor is it clear that Brandom's approach to the general project of delineating syntactic categories is incompatible with further elaborations by the kind of syntax that Chomsky supplies. In the only passage I've found where he mentions generative grammar and its transformation rules (Brandom, 1987), he makes precisely that suggestion:

> Recall … that Chomsky showed that one should not expect to generate the well-formed sentences of natural languages by concatenation, combination, or tree-structuring of any set of categories of this sort. To any such "phrase-structure grammar" will have to be added transformations of such combinatory structures. Categorial classifications are just the raw materials for grammar in this sense, and don't have anything to say about how one might proceed to the rest of the task of syntax once one has the categories. (165: fn. 2)

Lastly, we must consider how working syntacticians in the Chomskyan tradition go about identifying lexical items, morpho-syntactic categories, and other subsentential expressions. Needless to say, as a naturalistic, descriptive enterprise, the practice is subject to change under empirical pressures. Still, for the present day, the most common procedure is to employ what syntacticians call "constituency tests". For instance, the so-called 'do-so' test allows us to carve out phrases like the VP in (4) by reference to its behavior vis-à-vis (5), and then discern the more fine-grained syntactic units within that VP, by reference to (6) and (7).

(4) Jessica [$_{VP}$ swims quickly at the pool].

(5) Michael [does so], too.	'Does so' replaces 'swims quickly at the pool'.
(6) Aron often [does so] at the beach.	'Does so' replaces 'swims quickly'
(7) Hayes [does so] expertly in the tub.	'Does so' replaces 'swims'

Identifying such subsentential (and subphrasal) structure was crucial to the development of X-bar theory in the GB framework, and continues to guide syntactic theorizing in the Minimalist tradition. Of course, the empirical details are not so simple. Syntacticians employ a large battery of tests, not all of which agree with one another in every case. Moreover, any of the tests (including the do-so test) can be challenged on empirical grounds and, in many cases, rendered otiose by discoveries about the inner workings of *other* natural languages, only superficially unrelated. Mercifully, the gory details are not our topic here. For present purposes, the key point is this: When a syntactician employs constituency tests, she is appealing to precisely the kinds of substitutional relationships that Brandom's syntax rests on. To be sure, more complex considerations enter into the picture. For instance, how a syn-

tactic constituent can "move" (in grammars that allow movement, unlike say, HPSG) is a kind of data widely relied upon to determine syntactic category and constituency relations. But I see no reason why this too could not be cashed out in the language of substitutional syntax.

With this in mind, I'll end with the following speculation. If, in a remarkably distant possible world, Brandom *were* to go in for some empirical theorizing about natural languages, it's not clear to me that he would (or *should*) adopt anything other than generative grammar as the optimal theoretical framework within which to prosecute his inquiry. Certainly, he is well aware—how could he *not* be?!—of the theoretical need for Chomskyan grammars. On the rare occasion that he does mention these, he doesn't say anything that even hints at a disagreement. (Recall, "Chomsky *showed*…", my emphasis.) Moreover, his frequent invocations of the Chomsky hierarchy in discussions of computational procedures and the expressive power of various languages (e.g., Brandom, 2008: ch. 1) suggests no particular aversion to core generativist principles. To be sure, this isn't very much to go on. It by no means *shows* that Brandom's "substitutional syntax" is compatible with (any particular) generative grammar. But the consideration to the contrary seem likewise thin.

Conclusion

Attempting to integrate the theories developed by Brandom and Pietroski may strike some as an futile project, analogous to grafting, say, a squirrel onto a cow. One thinks to oneself, "Perhaps it *can* be done, but… why?!" In the foregoing pages, I've argued that this view of the matter constitutes a failure to appreciate the live opportunities for a fruitful merger. Such a merger is, like any large one, a daunting gamble. But, it seems undeniable, from where I sit, that both Brandom and Pietroski have furnished significant insights into the nature of something called "language"—a phenomenon that we should firmly resist regarding as unitary.

That having been said, it seems only natural to suppose that *combining* the two theories will yield a richer *overall* picture than either theory can provide on its own. This sort of thing doesn't always work out; not all teams of All-Stars are All-Star teams, after all. But even if the resulting view is not to one's liking, I find it frankly inconceivable that some such reconciliation project won't have to be effected *eventually*. Perhaps we aren't there yet; perhaps both generative linguistics and normative inferentialism must await more penetrating developments before their future descendants can be merged. (Or, again, maybe the AI people blow our minds with some new-fangled contraption next Tuesday. Who knows?) Whatever the case about that, I hope to have convinced the reader that there are, in fact, very few substantive *dis*agreements between the two approaches. What initially appear to be sharp contrasts turn out, on inspection, to be mostly benign differ-

ences of theoretical focus and explanatory ambition.

I'll close on a broadly sociological note. A mistaken commitment to the incompatibility of generative linguistics and normative inferentialism has had, I believe, negative consequences for both philosophy and linguistics. Specifically, there is, at present, little or no cross-talk between researchers working in these two traditions. Indeed, they seem to be as siloed off from one another as any two major research programs in "analytic" philosophy of language can be. If nothing else, by partially undermining the mistaken assumption of incompatibility, I hope to have gone at least some way toward rectifying the situation. My hope is that others will follow suit, attempting to forge still further connections between the two enterprises. Even if Pietroski and Brandom make for strange bedfellows, there is no question that they make for excellent guides. And, for better or worse, the terrain is largely uncharted. Let us press forward, then—as always, with optimism.

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