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Linguistic Luck

A Response to Rey and Collins

Michael Devitt

1. Introduction

Expressions of thought by members of a linguistic community are typically, to some extent, governed by the rules and principles (briefly, rules) of a shared language. Georges Rey and John Collins ("RC"), in "Laws and Luck in Language: Problems with Devitt's Conventional, Common-Sense Linguistics" (Chapter 5, this volume), helpfully describe "a matter of 'luck'" as something "accidental relative to a certain principled system" (p. xx). So, relative to the system of rules that constitute a language, an expression of thought that is not governed by the rules is a matter of linguistic "luck": the linguistic rules are luck-reducing mechanisms. I am mainly concerned with the extent of that luck: To what extent are the semantic and syntactic properties of the linguistic tokens in expressions of thought, in utterances, not governed by the rules of the language and hence "lucky"? I am also concerned with the source of those rules. Are they innate or learned? Insofar as they are learned, they are accidental relative to innate human nature; they are a matter of luck. And insofar as the rules are thus lucky, so too are the expressions governed by them. So the concern is with two distinct sorts of linguistic luck: linguistic tokens that are accidental relative to linguistic rules are "*r*-lucky"; and linguistic rules, hence expressions governed by them that are accidental relative to human nature are "*i*-lucky."

Having addressed these questions in §I, I will consider RC's critique in §II. I will respond first to their discussion of my views of language and linguistics, the views that underlie my theses about linguistic luck. I will then respond to their egregious misrepresentation of my methodology, a misrepresentation that they use to discredit my views and promote their own.[1]

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[1] RC's misrepresentation builds on Rey's earlier one (2020a), to which I have responded (2020: 428–32). I gave RC detailed comments on three prior drafts of their chapter, but the misrepresentation continued with little abatement. So I asked the editors of this volume, Abrol Fairweather and Carlos Montemayor, for the opportunity to reply. I am grateful to them for giving it to me.

C6S2   **I. Linguistic Luck**

C6S3   2. *R*-Luck and the Semantics-Pragmatics Distinction

C6P3   Perhaps the most exciting development in recent philosophy of language has been
the debate surrounding a group of philosophers and linguists who emphasize the
"pragmatic" features of language over those of traditional "truth-conditional
semantics." The group, roughly identified as "linguistic pragmatists" and/or
"linguistic contextualists," emphasizes the extent to which the content (meaning)[2]
conveyed by a sentence varies in the context of an utterance. They argue that this
content, the utterance's "message," is seldom, perhaps never, constituted solely by
a traditional semantic "what is said"; rather, there is "semantic underdetermin-
ation"; many think that we need to move to "truth-conditional pragmatics." The
group's seminal work is Dan Sperber and Deidre Wilson's *Relevance* (1995).
Major contributors to the debate include Kent Bach (1994, 1995, 2001); Robyn
Carston (2002, 2004); François Récanati (2004, 2010); and Stephen Neale (2004,
2007, 2016).

C6P4   Linguistic pragmatism clearly bears on the issue of *r*-luck. To the extent that a
token's message is constituted pragmatically in context it is *r*-lucky. So if the
pragmatists are right there is a lot more *r*-luck around than has customarily been
thought. In a series of works, culminating in the recent book, *Overlooking
Conventions: The Trouble with Linguistic Pragmatism* (2021b), I have argued
that they are not right. So these works can be considered "essays in anti-luck
semantics."

C6P5   It is taken for granted by almost all that "what is said" involves disambiguation
and reference determination in context as well as the conventional meanings of
the language employed, as well as what is strictly encoded. The controversy is over
whether there is anything else that is determined in context and goes into the
truth-conditional message, perhaps into "what is said." And over whether the
constitution of any such context-determined extra is "semantic" or "pragmatic."
Pragmatists think there is a lot extra and that it is "pragmatic." This yields their
theses of semantic underdetermination and truth-conditional pragmatics.

C6P6   Linguistic pragmatists are led to their theses by a range of interesting phenom-
ena. Consider the following utterances:

C6P7   (1)   I've had breakfast.
        (2)   You are not going to die.
        (3)   It's raining.
        (4)   Everybody went to Paris.

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[2]   I use 'content' and 'meaning' fairly interchangeably.

(5)   The table is covered with books.

(6)   John is a lion.

(7)   The party was fun until the suits arrived.

(8)   The road was covered with rabbit.

C6P8   Taken literally, (1) seems to say that the speaker has had breakfast sometime in the past and yet, in context, it likely means that she has had breakfast this morning. Similarly, (2) seems to attribute immortality to the addressee but, in context, will mean something like that he will not die from that minor cut. Although (3) does not say so explicitly it surely means that it is raining in a certain location. (4) seems to say that every existing person went to Paris and yet the message it surely conveys is that everyone in a certain group went to Paris. According to the standard Russellian quantificational account, (5) makes the absurd claim that there is one and only one table and it is covered with books. Yet it is surely being used to say that a certain table is so covered. (6) says that John is a charismatic feline but means that he is courageous. What ruined the party according to (7) was not really the suits but the business executives wearing them. And what covered the road according to (8) was the remains of rabbits. Examples like these are taken to show that a deal of "pragmatic" enrichment is needed to get from what is "semantically" determined to the message, perhaps to "what is said."

C6P9   My view of the semantics-pragmatics distinction (2013a) starts with an idea about languages, an idea that is rejected by Chomskians, as we shall discuss in §II. My idea is that languages are representational systems that scientists attribute to species such as bees, prairie dogs, and humans to explain their communicative behaviors. We then have a powerful theoretical interest in distinguishing two sorts of properties of any particular utterance: (a) the representational properties that it has simply in virtue of being a token-expression in a language, that it has simply as a result of the organism's exploitation of that language; (b) any other properties that may constitute the organism's message. I call the (a) properties part of "what is said," and "semantic"; and the (b) ones—for example, certain "modulations" and Gricean "implicatures"—part of "what is meant but not said," and "pragmatic." "Semantics" is the study of semantic properties, "pragmatics," pragmatic ones.[3] This theoretical basis then provides an argument for the view that what is said is constituted by properties arising from three sources: (i) from (largely)[4] conventional linguistic rules in the speaker's language, rules that determine what is *encoded* in the language; (ii) from disambiguations, where more than one rule

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[3] "Pragmatics" is also used for "the theory of interpretation," the study of the *processes* of interpreting utterances. So the term is ambiguous (Devitt, 2013a: 103–5).
[4] I say "largely" because I accept the Chomskian view that some syntax is innate (§3). Chomskians may think the qualification inadequate, as we shall see (§6).

governs an expression in the language; (iii) from "saturations," for example, the reference fixing of indexicals (and tenses), deictic demonstratives, and pronouns.

C6P10   This is a fairly traditional semantics-pragmatics distinction for which I claim to have given a theoretical, not just intuitive, basis.

C6P11   The key thing about semantic properties is that they are (largely) conventional: conventions *create* linguistic meanings. But, what is a linguistic convention? It is a convention of using a certain physical form to express a certain part of a thought/ message, a concept. Putting this in Gricean terms, it is a convention of using that form with a certain "speaker meaning."

C6P12   How do we *tell* when there is such a convention? We should not follow the custom of simply relying on our intuitions. Rather, we should look for evidence mainly from regularities in behavior (2013a). Is this physical form regularly used to express a certain concept, used with a certain speaker meaning? If so, it is likely, though not certain of course, that this regularity is *best explained* by supposing that there is a convention of so using the form. In principle, evidence could be found also in mental processes but in fact, I have argued (2021a: 147–55), we lack any such evidence.

C6P13   What precisely is the pragmatist challenge to the semantic tradition from the perspective I have summarized? It is helpful to answer this in terms of a three-way distinction among the possible "meaning" properties of an utterance: (A) an encoded conventional meaning; (B) a what-is-said, arising from encoding, disambiguation, saturation; (C) a pragmatic modification, perhaps a modulated what-is-said, an implicature, or whatever. (A) and (B) meanings are semantic properties; (C) are at least partly pragmatic. We should all accept—and I assume the tradition did—that there are novel uses of language, "spur of the moment" uses "on the fly," that yield pragmatic meanings. Thus there can be implicatures like Grice's famous reference letter (1989: 33). And spontaneous ellipses are surely common, requiring what-is-said to be pragmatically enriched or impoverished to get the precise message. Such novel uses are, of course, *r*-lucky. Now one might argue that novel uses of these sorts are more widespread than has been traditionally thought. That is an empirical issue that does not seem theoretically interesting because, however widespread these phenomena, they *obviously* must be explained pragmatically. The interesting pragmatist challenge to the tradition is posed by *the expressions— like those in (1) to (8) above—that motivated linguistic pragmatism*, for these expressions have regular uses that are alleged to be pragmatic; for example, regular saturations of quantifiers with domain restriction, of weather reports with reference to a location, of definite descriptions with reference to a particular object in mind; and, turning to polysemy, regular uses of 'foot' to refer to the bottom of a mountain, of 'suit' to refer to business executives, of 'rabbit' to refer not only to rabbits but also to rabbit stuff. This interesting challenge is to the view that the truth-conditional meaning communicated in such regular uses is typically constituted only by my semantic what-is-said, arising from (i) conventional

encodings, (ii) disambiguations, and (iii) saturations. The challenge is to the view that, absent novel spur-of-the-moment modulations and implicatures, the message of an utterance is typically that what-is-said. In challenging that view, pragmatism claims that, even setting aside the novel, the message, the meaning communicated, is seldom, perhaps never, constituted solely by that what-is-said; pragmatic modifications of some sort always, or almost always, play a role; there is always, or almost always, some *r*-luck.

C6P14   In response to this challenge, I have argued, particularly in *Overlooking Conventions* (2021b), that almost all of the striking phenomena that pragmatists have emphasized exemplify properties of sorts (i) to (iii). There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said—into semantics—than has been customary; conventions have been overlooked. Contrary to what the pragmatists claim, there is no extensive semantic underdetermination. The new theoretical framework of truth-conditional pragmatics is a mistake. The striking phenomena should be accommodated within a traditional framework. Here is a summary of my case for this.

C6P15   The challenge arises from the many examples of context relativity, like (1) to (8), produced by pragmatists. In arguing for my semantic rather than a pragmatic approach, I divide these examples into three groups. One group is of examples like (1) to (4) exemplifying "saturation" in context. Another is of examples like (6) to (8) exemplifying polysemous ambiguity. Finally, there are examples of "referentially" used definite descriptions, like in (5), "The table is covered with books," which exemplify *both*: they need to be saturated in context by a particular object in mind; and they have another meaning, the quantificational meaning described by Russell, that yields "attributive" uses.[5]

C6P16   To meet the pragmatists' challenge I need to show that semantic explanations of these examples are "the best." My strategy is to show, first, that a semantic explanation is good and, second, that it is much better than its pragmatic rival.

C6P17   I start showing that semantic explanations are good by considering referentially used definite descriptions. For, my discussion of them (1981, 1997, 2004, 2007a, 2007b) is a paradigm of the semantic approach I urge to the pragmatists' examples. I argue that these uses exemplify a non-Russellian referential meaning. So definite descriptions are ambiguous.

C6P18   Many treat these referential uses pragmatically in a Gricean way as involving, for example, conversational implicatures (Kripke 1979; Neale 1990; Bach 1994). Others treat them pragmatically in a Relevance-Theoretic way where both referential and attributive uses involve other sorts of pragmatic modifications (Recanati 1989; Bezuidenhout 1997; Powell 2010). A pragmatic response to

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[5] See Donnellan (1966) for the referential/attributive distinction.

referential uses was encouraged by the indubitable fact that we seem to be able to give a pragmatic explanation of the referential use of *any* quantifier. Neale has a nice example with 'everyone': Jones says despondently, "Well, everyone taking my seminar turned up" (to Jones' party), in circumstances in which the hearer can use Grice's Cooperative Principle, and the common knowledge that Smith is the only person in Jones' seminar, to derive the message that only Smith attended. As Neale says: "The possibility of such a scenario, would not lead us to complicate the semantics of 'every' with an ambiguity" (1990: 88). Nor, Neale is suggesting, should the possibility of referential uses of 'the *F*' lead us to complicate the semantics of it with an ambiguity (pp. 87–8).

C6P19   I responded to this nice point with what Neale later called "The Argument from Convention."[6]

C6P20   The basis for [the semantic explanation] is not simply that we can use a definite referentially, it is that we *regularly* do so. When a person has a singular thought, a thought with a particular *F* object in mind, there is a regularity of her using 'the *F*' to express that thought . . . . This regularity is strong evidence that there is a *convention* of using 'the *F*' to express a thought about a particular *F*, that this is a *standard* use. This convention is semantic, as semantic as the one for an attributive use. In each case, there is a convention of using 'the *F*' to express a thought with a certain sort of meaning/content.

C6P21   'Every' and other quantifiers are different. There is no convention of using them to convey a thought about a particular object in mind. With special stage setting they certainly can be used for that purpose, as Neale illustrates. But then Grice shows us that with enough stage setting almost any expression can be used to convey almost any thought.   (2004: 283)

C6P22   The idea is that there is a convention for 'the *F*', but not for 'every', that demands saturation by the particular object in mind; the saturation is semantic. So 'the *F*' is ambiguous, having both a quantificational meaning that yields attributive defin-ites and a referential meaning that yields referential definites. And the referential use is not *r*-lucky.

C6P23   I argue in the same way that other pragmatist examples of expressions being saturated in context should be treated semantically. Each such utterance, like referentially used descriptions, has an implicit slot to be filled; each requires saturation by an implicit reference to something the speaker has in mind:

C6P24   (1)   I've had breakfast. [Implicit reference to a period]
    (2)   You are not going to die. [Implicit reference to a potential cause of death]

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[6] See also Reimer (1998).

(3)   It's raining. [Implicit reference to a location]
(4)   Everybody went to Paris. [Implicit reference to a domain][7]

C6P25   Note that, in each case, the implicit reference, can be made explicit: "I have had breakfast this morning"; "You are not going to die from that minor cut"; "It is raining in New York"; "Everyone at the conference went to Paris." Note also that there are indefinitely many possible saturations for the sentences in utterances (1) to (4), just as there are for referential definites or demonstratives. We know from informal observation that each of (1) to (4) exemplify a *regularity* of saturating expressions of a certain form to convey a message. Indeed, pragmatists don't just note these regularities they emphasize them. These regularities can be plausibly explained by supposing that there are linguistic rules for these expressions, brought about by conventions, that demand saturation in context. So they are not *r*-lucky.

C6P26   The same line of argument works for many other expressions; for example, 'eat', 'dance', 'sing'. And a similar one works for many others including 'ready' and genitives like 'Peter's bat'.

C6P27   Turn now to polysemy and examples like the following:

C6P28   (6) John is a lion.
C6P29   (7) The party was fun until the suits arrived.
C6P30   (8) The road was covered with rabbit.

C6P31   I argue that such utterances should be explained as examples of semantic polysemy that are disambiguated in context. 'Lion' in (6) is an example of metaphor-based polysemy; 'foot' and 'warm' are among countless others. 'Suits' in (7) is an example of metonymy-based polysemy; 'glass', denominal verbs like 'to google', and compound nouns like 'language teacher', are among countless others. 'Rabbit' in (8) is an example of "regular" ("systematic") polysemy: any count noun for an organism yields a mass noun for the stuff of which it is made. There are countless other regular polysemies; for example, a word for the producer of some item yields a word for the item produced (or vice versa), as with 'Honda' and 'newspaper'. All of these polysemous words are regularly used with more than one meaning. The case for treating them semantically is that these regularities are well-explained as conventions. Once again, no *r*-luck.

C6P32   The second step in meeting the pragmatists' challenge is to show that these good semantic explanations are better than pragmatic ones. We can then conclude that the semantic explanations are "the best."

[7] I tell a similar story about many sub-sentential utterances (2018).

C6P33   The general problem for pragmatic explanations stems from "the psychological-reality requirement" (2007a: 12–16). Consider any pragmatic explanation of the speaker meaning (message) conveyed by an utterance's sentence. The explanation involves a derivation of that meaning from the sentence's conventional meaning in the context. A paradigm example is the derivation of a Gricean implicature. Now, it is not enough for a pragmatic explanation to be good that there *is* a pragmatic derivation, discoverable by a linguist, of the speaker meaning from the conventional one. For, there is always such a derivation with a "dead metaphor." The metaphorical meaning of a word is derived from its conventional meaning. Over time, a metaphorical meaning often becomes regularized and conventional: the metaphor "dies." Yet a derivation of what is now a new conventional meaning from the old conventional meaning will still be available.[8] For the pragmatic explanation to be good, its derivation must have an appropriately active place in the cognitive lives of speakers and hearers: in brief, the hearer must go through the derivation and the speaker must make the utterance on the assumption that the hearer will go through the derivation. *That's the psychological-reality requirement*. And it may be met easily with a *novel* use, like Jones', "Well, everyone taking my seminar turned up," because speaker and hearer may be *conscious* of the derivation. But there is unlikely to be any such awareness with the likes of (1) to (8). So, if the derivations are present at all, they must be *subconscious*. A pragmatic explanation then faces two powerful objections (Devitt 2021a: 138–40).

C6P34   **Occamist Objection:** The pragmatic explanation has heavy psychological commitments that we have no reason to suppose can be met. I have argued (2021a: 147–55) that though psycholinguistics has discovered many interesting facts about language processing, they have not produced any evidence of the subconscious processes that pragmatism requires. One might reasonably respond that, despite years of impressive work, we know quite little about how we process language *in general*. Still we do know that there *must be* the largely subconscious convention-exploiting processes of saturation and disambiguation in speakers and hearers, even if we do not know the details. For, those are standard processes of language use. So we already know that there must be the sort of processes required by the semantic explanation, the rival of the pragmatic one. This is a crucial part of the background knowledge that is so important in assessing which is the better explanation of the phenomena in question. The semantic explanation is committed to mechanisms that we already know exist, even though we are short on the details. The pragmatic explanation is committed to subconscious processes for which we need independent evidence before we should suppose that they exist at all. There is no such evidence and so we should prefer the semantic explanation.

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[8] I have used this consideration against Modified Occam's Razor, as normally construed (2013b: 297–300).

C6P35   **Developmental Objection:** Not only should we prefer the semantic to the pragmatic on Occamist grounds if we lack evidence of the existence of the required new psychological processes, there is a good reason to suppose, *a priori*, that these processes do *not* exist. Consider an expression that does not have a certain meaning conventionally in a language but that comes to be regularly used with that meaning in successful communications by speakers of that language. That success tends to lead to the expression having that meaning by convention, thus building the language. So, why would the regular saturation of the expressions in (1) to (5), and the regular use of the polysemes in (5) to (8), not have had that same happy result? A convention eliminates the need for the demanding mind-reading processes in speakers and hearers that a pragmatic explanation requires. This is how having a language aids communication. Why would we have denied ourselves the benefit of conventionalization with the expressions in (1) to (8)?

C6P36   In sum, the semantic explanations of the likes of (1) to (8) are good. In contrast, the pragmatic explanations posit subconscious processes that we have no independent reason to suppose exist: the Occamist Objection. Worse, our background knowledge makes it unlikely that those processes do exist: the Developmental Objection. The semantic explanation is "the best." There is much less *r*-luck than pragmatists imply. Novel uses of language aside, utterances are typically *r*-unlucky.

C6S4   3. *I*-Luck and Universal Grammar

C6P37   What about *i*-luck? Are linguistic rules innate or learned? Insofar as they are learned they are *i*-lucky; hence the expressions governed by them are *i*-lucky. Chomsky has famously argued that some fundamental rules of syntax, the rules of Universal Grammar (UG), are innately determined. So it is not *i*-luck that our language does not contain expressions that offend these rules. Thus, to take one of RC's examples (p. xx), it is not *i*-luck that "Who will John and meet Mary?" is not in our language although "John and **who** will meet Mary?" is: the absence of the former is not accidental but ruled out by innate constraints. I *emphasize* that I accept this innateness, because one might get the impression from RC's Chapter 5 (this volume), and particularly from Rey's important recent book (2020b), that I do not really.[9] My acceptance is clear in the conclusion of the relevant chapter in *Ignorance of Language*:

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[9] Rey states: "[Devitt] does allow that *some* features may be innate (2006a: 13,103), but this seems mostly lip-service, since, so far as I can find, he never provides any serious examples.... His claim does suggest that he does not take the oddities of UG rules to seriously tell against his conventionalist conception. To the contrary, Devitt sometimes seems actually to think that conventional rules would

C6P38   I think that the [arguments from the universality of UG-rules, from the poverty of stimulus, from language creation, and from the continuity hypothesis], taken jointly, present a persuasive case for [the interesting thesis that] humans are innately predisposed to learn languages that conform to the UG-rules; the initial state respects the UG-rules.[10]   (2006a: 271)

C6P39   This thesis raises the question: *In virtue of what* does the initial state "respect" the UG-rules? I took Chomsky to embrace the "even more interesting" thesis:

C6P40   The initial state respects the UG-rules *because it embodies the UG-rules*. Not merely do we inherit some language-constraining rules that makes us respect the UG-rules, which is all that [the above thesis] requires, we inherit the UG-rules themselves. (2006a: 246)

C6P41   I'm dubious, but still argue:

C6P42   If we assume [that] a person thinks in a Mentalese governed by structure rules that are similar to those of her language [then] we have good reason to suppose that something close to [the even more interesting thesis] will explain that respect and hence be part of a persuasive future explanation of language acqui-sition; we have good reason to suppose that the UG-rules are, for the most part, innate structure rules of thought.   (2006a: 271)

C6P43   I point out that:

C6P44   neither of these theses entail that speakers have innate representations of lin-guistic rules or innate propositional knowledge—knowledge-that—about them: the innate rules might be simply embodied and any innate knowledge might be simply knowledge-how.... As a result, these theses alone do not entail the existence of any innate *concepts* and so do not seem to bear on the traditional debate over innate ideas.   (pp. 246–7)

suffice *instead* of innate ones. As we noted from the start, a major challenge to a conventionalist view is to explain, *inter alia*, the WhyNots" (2020b: 206).

Rey does not mention my argument (2006a: 248–51) that leads to the unequivocal conclusion quoted in the text. This argument is surely not "lip-service." And the fact that its conclusion is *inconsistent with* treating "the oddities of UG rules" as conventional shows that I take those oddities "to seriously tell against" their being conventional! And it is baffling that he finds "baffling" (2020b: 210) my recent footnote about his (neatly named) WhyNots, which reads: 'To avoid misunderstanding, perhaps I should emphasize that I grant the Chomskian view that the language has some of its syntactic properties, including perhaps the WhyNots, as a result not of convention but of innate constraints on the sorts of language that humans can learn "naturally"' (2006a: 244–72, 2020: 379, n. 11)

Rey seems to take this as a change of heart, but it has always been my view, as is obvious from the cited discussion in *Ignorance*.

[10] But see Scholz and Pullum (2006) for some skepticism.

C6P45   In any case, I found no basis for this "very exciting" thesis that linguistic rules are indeed innately represented and propositionally known, no basis for the thesis that UG itself, the *theory* of those rules, is innately known (p. 272).

C6P46   I turn now to the critique of my linguistic views by Rey and Collins (RC).

C6S5   ## II. Response to Rey and Collins

C6S6   ## 4. The Linguistic Conception

C6P47   As can be seen, I agree with RC that language is, to an interesting degree, *i*-unlucky. But I think that the linguistic expressions in utterances are governed by rules that are mostly not innately determined and so those expressions are *r*-unlucky, something that RC do not seem to contemplate. My stance on linguistic *r*-luck reflects a view of language and linguistics that RC emphatically reject. Our most serious disagreement is over *what grammars are about*: I oppose the Chomskian "psychological conception" and urge the "linguistic conception." So, RC rightly introduce the linguistic conception in their §2.3 as "Devitt's Alternative" view of "the explanatory scope of linguistics" (p. xx). But, it needs to be emphasized, my alternative view of this scope is *not* "conventionalism," which is the second part of RC's mantra, "common-sense conventionalism," that begins in their title and dominates their discussion of my views in what follows. We may have a disagreement over the role of conventions in the explanation of language but, we shall see in §5, this is not of much significance generally and is *totally irrelevant* to my alternative view of "the explanatory scope of linguistics." Unfortunately, RC conflate the serious disagreement with the irrelevant one throughout their discussion of my views, thoroughly muddying the waters.

C6P48   I shall discuss the linguistic conception and identify the conflation in this section. I shall discuss my alleged "conventionalism" in the next. In §6, I shall address their baseless charge, exemplified in the first part of the mantra, that my methodology is one of relying on common sense.

C6P49   As RC note, according to the linguistic conception, which they call "(LR)," a grammar is about a non-psychological realm of linguistic expressions, physical entities forming symbolic or representational systems. In contrast, the received Chomskian psychological conception is that a grammar is about a speaker's linguistic competence and hence about mental states. RC dismiss the linguistic conception, but without any attention to the argument for it, a matter to which I will return. Here is a very brief summary of the argument.[11]

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[11] The argument was first presented in Devitt (2003), which formed the basis for Devitt (2006a: ch 2). Later presentations and developments are in Devitt (2006b: 574–87, 2008a,b, 2013c). Devitt and Sterelny (1989) is an earlier version of the argument, but contains many errors.

C6P50   The argument starts with three general distinctions. The distinctions are, first, between the theory of a competence and the theory of its outputs or inputs; second, between the structure rules governing the outputs and the processing rules governing the exercise of the competence; third, between the "respecting" of structure rules by processing rules and the inclusion of structure rules among processing rules. ("Respecting" is a technical term here: processing rules and competence "respect" the structure rules in that they are apt to produce outputs governed by those structure rules; analogously, apt to process inputs.)

C6P51   The argument continues: these general distinctions apply to humans and their languages. To take my favorite example, just as the theory of the representational system that is the bee's "waggle dance" is one thing, the theory of the bee's competence to produce the dance, another, so also is the theory of the representational system that is a human language one thing, the theory of the speaker's competence to produce it another. Karl von Frisch proposed a theory of the structure rules of the representational system that is the bee's dance. We need an analogous theory that explains the nature of the representational system that is a human language. That theory will surely be even more interesting that the one that got von Frisch a Nobel Prize.

C6P52   *Why* is a theory of the nature of that representational system so interesting? We posit the system to explain behavior, particularly communicative behavior: a noise (or inscription) is produced *because* it has certain linguistic properties, including syntactic ones, and it is *because* it has these properties, and hence is a linguistic expression (symbol), that an audience responds to that behavior as it does. The linguistic properties of symbols explain their striking causal roles in our lives, roles that we may hope to capture in laws. I shall return to the causal role of symbols in §5.

C6P53   What do such theories tell us about competence? Not much. Simply on the strength of von Frisch's theory we know this minimal proposition about any competent bee: that there is something-we-know-not-what within the bee that respects the structure rules that von Frisch discovered. But the theory does not tell us *what* there is in the bee that does this job. Indeed, last I heard, we know very little about that. Similarly, a theory of the structure rules of our language tells us that there is something-we-know-not-what within any competent speaker that respects the structure rules it describes. This is the minimal position on psychological reality that I call "(M)" (2006a: 57). But the theory of the language provides nothing more about the mind than (M): it does not tell us *what* there is in the speaker that does the respecting. In particular, we don't know whether any of the theory's rules are embodied some way or other in the mind and so also part of the psychological reality that produces language. To move beyond the minimal claim and discover *the way in which* a speaker respects the grammar's rules, we need further psychological evidence of actual processing.

C6P54   Finally, I argue that a grammar, produced by linguists, is the syntactic part of the theory we need of the representational system that is a human language:

C6P55   A grammar is a theory of the nature of the system that is a language, not of the psychological reality of that language in its competent speakers (beyond the minimal (M)). (2008a: 206)

C6P56   We have arrived at the linguistic conception (LR).

C6P57   In support of the final move in the argument for (LR), consider some typical grammatical rules (principles):

C6P58   An anaphor must be bound by another expression in its governing category.
A pronoun must not be bound by another expression in its governing category.
Accusative case is assigned by a governing verb or preposition.
A verb which fails to assign accusative case fails to theta-mark an external argument.

C6P59   Such claims about anaphors, pronouns, verbs, prepositions, and the like are about *expressions*, symbols in a human language. These claims are not about mental states: they *do not mention* understanding or mental capacities. Building on this, with reference to Quine (1961), I offered the follow deductive argument for the above view of grammars:

C6P60   (a) Any theory is a theory of x's iff it quantifies over x's and if the singular terms in applications of the theory refer to x's.

C6P61   (b) A grammar quantifies over nouns, verbs, pronouns, prepositions, anaphors, and the like, and the singular terms in applications of a grammar refer to such items.[12]

C6P62   (c) Nouns, verbs, pronouns, prepositions, anaphors, and the like are linguistic expressions/symbols (which are entities produced by minds but external to minds).

C6P63   (d) So, a grammar is a theory of linguistic expressions/symbols. (Devitt 2020: 375–6)

C6P64   The truth of the grammar of a language entails that its rules govern linguistic reality, giving a rich picture of this reality. In contrast, the truth of the grammar does not entail that its rules govern the psychological reality of speakers competent in the language and it alone gives a relatively impoverished picture of that reality (just (M)).

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[12] For a discussion of some examples in Liliane Haegeman's textbook (1994), see my (2008b: 250–1).

C6P65   Grammatical rules *directly* explain the natures of expressions but this is not to say, of course, that they play no role in explaining cognitive phenomena:

C6P66   It is *because* the grammar gives a good explanation of the symbols that speakers produce that it can contribute to the explanation of the cognitive phenomena . . . . *English speakers construe English expressions as if they had certain properties because, as the grammar explains, the expressions really have those properties.*

(2008a: 215)

C6P67   The grammar would not be a complete theory of the language (even if finished): a grammar is a theory of the syntax of a language (broadly construed) and, it is common to think, needs to be supplemented by theories of the word-world connections that constitute word meanings. So, I think we need theories of reference. Rey notes this, and notes the doubts that Chomsky and others have "that we will ever get anything like a serious *theory* of the topic."[13] But then Rey continues:

C6P68   What Devitt needs to show in order for his focus on LR to even begin to compete with Chomsky's on psychology is that these mind/world relations are as remotely susceptible to theories as deep as those of I-languages seem to be. (2020b: 201)

C6P69   This is quite false. (a) The linguistic conception (LR) is about grammars and hence only concerned with syntax. (LR) claims that grammars are theories of the syntactic properties of external-to-the-mind symbols. That is true even if a "serious theory" of the reference of those symbols is beyond us. (b) More importantly, *as theories of the syntactic nature of symbols, grammars are as deep as one could want. In contrast, as theories of the mind, grammars are shallow*; see the minimal (M) above.

C6P70   I have taken to concluding my presentations of my argument for the linguistic conception by

C6P71   emphasizing that the linguistic conception does *not* involve the absurd claim that psychological facts have nothing to do with linguistic facts. Some psychological facts *cause* linguistic facts (pp. 23–4), some "*respect*" them (p. 25), some *partly constitute* them (pp. 39–40, 132–3, 155–7), some *provide evidence* for them (pp. 32–4), and some make them *theoretically interesting* (pp. 30, 134–5). But psychological facts are not the *subject matter* of grammars. The dispute is not over whether linguistics relates to psychology but over the way it does.

(2008a: 207; the cited passages are in Devitt 2006a)

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[13] I sympathize with the doubts and come close to sharing them on bad days.

C6P72   The linguistic conception provoked a deal of outrage in Chomskian circles.[14] Some responses by Rey and Collins stand out for the admirable amount of *argument* they contain. First, in a series of papers (2006a, 2006b, 2008), Rey challenged a key presupposition of the linguistic conception: that there really is an external-to-the-mind linguistic reality for grammars to explain; he challenged "linguistic realism." For, according to that conception, grammars explain the nature of that linguistic reality, and one can't explain what doesn't exist. Furthermore, it has to be the case that the *theoretical interest* of grammars comes primarily from such explanations. I reject Rey's anti-realism, of course (2006a: 184–9, 2006b: 597–604, 2008a: 221–9.) but he did a service in arguing for something that is usually just taken for granted by Chomskians. Second, recently both Collins (2020) and Rey (2020a: 306–13), in a volume edited by Andrea Bianchi (2020), have tried to challenge the linguistic conception by offering "paraphrase responses." As Quine points out, a scientist can avoid the ontic commitments of a theory by proposing another one that paraphrases away that commitment while still serving the theoretical purpose well enough.[15] In my reply to Collins and Rey in the Bianchi volume, I noted that those wanting to mount this defense of the psychological conception in the face of my argument need to do three things:

C6P73   First, *they need to acknowledge* that grammars should not to be taken literally but rather should be taken as standing in for a set of paraphrases that do not talk about expressions . . . . Second, *they should tell us what the paraphrases are* or, at least, tells us how they are to be generated from what grammars actually say. Third, *they should give examples of how this paraphrased grammar directly explains cognitive phenomena.*   (2020: 377)

C6P74   Admirably, Collins and Rey have attempted to meet these requirements, Collins, at great length.[16] I gave detailed criticisms of these attempts, including:

C6P75   *The rewriting adds no explanatory power*. And the rewriting is pointless if the grammar is indeed a more or less true account of linguistic reality, as the linguistic conception claims. (2020: 379–80)

C6P76   I concluded with this ringing reaffirmation of the linguistic conception:

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[14] Collins (2006, 2007, 2008a, 2008b, 2020); Rey (2006a, 2006b, 2008, 2020a, 2020b); Smith (2006); Antony (2008); Pietroski (2008); Slezak (2009); Longworth (2009); Ludlow (2009).

[15] In his book, Rey oddly accuses me of having "carelessly neglected" this "crucial caveat in Quine's discussion of these issues" (2020b: 209). Yet I introduce my long discussion of "The Paraphrase Response" with the remark that it would be "a bit flatfooted" to overlook it (2020: 377).

[16] And Rey describes well how what Chomskians *actually say* in presenting grammars poses an ontological problem for them (2020b: 213–16).

C6P77 There are, external to the mind, entities playing causal roles in virtue of their linguistic properties. Grammars are approximately true theories of some of those properties, the syntactic ones. (2020: 380)

C6P78 So one would think that if Rey and Collins wanted to continue challenging the linguistic conception, "Devitt's Alternative," they would focus on this Bianchi exchange on that very subject. Yet that exchange goes virtually undiscussed in their present chapter.[17] What is going on? The aforementioned conflation is central to the answer, as we shall now see.

C6P79 1. Although RC do not discuss the Bianchi exchange they do mention it. They attach a footnote (p. xx) to the quote from Devitt (2008a: 205) that they use to introduce the linguistic conception (LR). The footnote acknowledges that "Devitt (2006a) provides a number of arguments for (LR)" but then say astonishingly that they "won't consider [those arguments] here," citing the Bianchi exchange (p. xx n. 10). *Why not consider arguments for the topic under discussion?!* RC explain: those arguments "are independent of the common-sense conventionalism that is all that concerns us here." But if (LR) is independent of what concerns them, why is it introduced as "Devitt's Alternative" to the Chomskian view of linguistics? And, *as a matter of fact*, it *is* my alternative and the alleged "common-sense conventionalism" of their mantra is *not*, as we shall see.

C6P80 Set aside the "common-sense" part of the mantra until §6. We see here the first clear sign of the conflation that bedevils RC's critique, the conflation of the linguistic conception of grammars with my alleged "conventionalism," the other part of the mantra. More signs of the conflation are to come.

C6P81 The conflation matters because my view of the role of conventions is *irrelevant* to the linguistic conception. This was apparent from the start given the place of the bee's dance in my argument for the linguistic conception: for, as I noted, "the bee's competence to dance is surely innate" (2006a: 39). I later added the example of prairie dogs in bringing home just how irrelevant it is to the linguistic conception whether linguistic rules are innate or conventional:

C6P82 The rules of the prairie dog's language seem to be partly learned and, perhaps we should say, "conventional": its alarm calls vary a bit from colony to colony; and when an experimenter used a plywood model to simulate a new sort of predator, the prairie dogs introduced a new call (Slobodchikoff 2002). In any case, whether a language used to communicate information is innate or conventional, we have a powerful theoretical interest in that language and its rules. Serious scientists

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[17] The exception is in footnote ## where they rightly object to my claim (2020: 379) that their paraphrases of grammatical claims are "very different." But this minor matter is quite incidental to the linguistic conception.

work to discover the natures of the symbols in these representational systems, to
discover their *meanings*.   (2013a: 95–6)

C6P83   That the rules of human languages "differ from those of the bees in being
conventional not innate . . . is beside the point" (p. 99).

C6P84   Contrast this with Rey's claim that Chomskians:

C6P85   offer a substantial theory that the innate properties are *the main and fundamental
properties of grammar*, . . . those properties arguably *constitute* what a grammar *is*!
This is precisely why they regard grammar as part of human psychology.
(2020b: 210)

C6P86   Even if *all* the properties that a grammar attributes to a language were innate that
would not make the grammar "part of human psychology." The grammar is about
the syntactic properties of the expressions of a language whatever the cause of
their having those properties.

C6P87   2. RC follow the quote that introduces the linguistic conception with an
account that nicely identifies some views that are *not* part of the conception.
They conclude:

C6P88   Nor does Devitt have any quarrel with any of the specific principles of grammar
that linguists propose. Rather he simply objects to their regarding their
theories as psychological theories rather than as theories of "linguistic reality"
per (LR). (p. xx)

C6P89   All well and good. But then, remarkably, RC go on:

C6P90   This seems to be partly due to his insisting upon a common-sense conception
about the reality of standard linguistic entities ("SLEs," such as words, phrases,
sentence, phonemes, and/or phonological properties) and the conventions gov-
erning them. (p. xx)

C6P91   RC follow this by quoting my claim that "symbols [SLEs] are social entities"
(2006b: 583).

C6P92   Let us take stock. RC are speculating that my reason for the linguistic concep-
tion (LR) lies in my view that SLEs are conventional. Yet my reason *obviously does
not*: the conception would apply even if our language were entirely innate; see
above. More importantly, *why the uncharitable speculation? Why not just look to
my actual arguments to find my reason?* (I will return to this point in §6.) RC are
very familiar with these arguments, having responded to them at length in print.
Yet, as noted a page back, these are the very arguments that they set aside as

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"independent of the common-sense conventionalism that is all that concerns us here." Well, the arguments are obviously *not* independent of the *linguistic conception* and we have already seen ample evidence that this conception *does* concern them. And so it should, if they are concerned to rebut "Devitt's Alternative" view of "the explanatory scope of linguistics." For, whereas Chomskians think that linguistics' scope "flows from its positing of complex internal structure to the mind/brain" (p. xx), my alternative view is that much of its scope "flows from" a grammar's explanation of the syntactic natures of external-to-the-mind symbols in a linguistic system. *That* is my real alternative. And it is the linguistic conception. RC are conflating it with "conventionalism."

C6P93   3. RC note that I am not urging "an alternative set of grammatical principles" nor denying "some of their innate determinants." "Devitt is only concerned to deny…commitment to those innate determinants being the proper focus of linguistic theory" (p. xx).[18] RC continue: Devitt "wants to insist on a non-psychological focus on *the social and conventional facts about language*" (p. xx; emphasis added). But this is *not* what the linguistic conception insists on. It insists that the focus of grammars is on *facts about linguistic expressions*, whether those facts are caused by conventions or not. RC have welded the view that these facts are conventional into the linguistic conception: the conflation again.

C6P94   4. The conflation is very evident in RC's discussion that follows, particularly in this:

C6P95   Scouring [Devitt's] writings, it's hard to find an argument other than…: the syntactic differences between public languages *show* that much syntax is not innate (2006a:180; emphasis ours). (p. xx)

C6P96   But this quote is part of an argument for my alleged "conventionalism" not an argument for the linguistic conception. Yet it is that conception, not "conventionalism," that is "Devitt's Alternative," which is what §2.3 is supposed to be about. And finding an argument for that conception doesn't require any scouring: they can just look at the arguments that they decided not to consider!

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[18] I may sometimes have written loosely as if making such a denial about "linguistic theory"—"hyperbolic moments"?—but all I ever meant to deny was that those innate determinants are the focus of *grammars*; thus, the title of the relevant chapter of *Ignorance* is "A Grammar is a Theory of Linguistic Reality" (2006a: ch. 2). *Linguistic theory* is a big tent that covers more than grammars. I certainly think that UG, which obviously is concerned with the innate determinants, is *a*, though not *the*, "proper focus of linguistic theory."

Relatedly, consider this: "Devitt's main error is to claim that Chomsky is somehow mistaken in having the internal, psychological interest he pursues" (Rey 2020b: 197). But I do not belittle an interest in UG and the psychology of language. Indeed, almost all of *Ignorance* is about that psychology. I think, however, that there is something else of theoretical interest, which grammars help to explain, external-to-the-mind languages. RC's "main error" is to deny this interest. So, the belittling seems to be on the other foot.

(And, I might add, finding an argument for my alleged "conventionalism" is not hard either; see §5 below.)

C6P97   In conclusion, there is nothing in RC's discussion of the positions they conflate that counts against the considerations leading to my ringing affirmation of the linguistic conception (§4). And the linguistic conception is the real "Devitt's Alternative."

C6P98   I turn now to the other side of the conflation, my alleged "conventionalism."

C6S7   5. The Role of Linguistic Conventions

C6P99   What exactly is the "conventionalism" that RC object to? I have already indicated my view that their disagreement with me over the role of conventions in the explanation of language is not of much significance generally and is *totally irrelevant* to my alternative view of "the explanatory scope of linguistics." Here's why.

C6P100   I start with something that we surely agree on: grammatical rules are not entirely innate. Innate principles of UG are thought to demand the settings of parametric values that yield the grammatical rules of a particular language. As RC put it:

C6P101   Just *which* language an individual speaker will acquire depends upon comparatively contingent circumstances, specifically, which lexical items are learned, and how specific parameters are set, such as whether a language is SVO or SOV. (p. xx)

C6P102   Let us set aside lexical items and consider only the acquisition/learning of syntax. To the extent that the grammatical rules of that speaker's language are constituted by those contingent parameter settings, the rules are not innate but *i*-lucky. My explanation of parameter setting appeals to conventions:

C6P103   Very occasionally an idiolect's parameter settings may be eccentric but almost always they will be conventional. Thus most people in the USA participate in parameter-setting conventions that lead them to speak an SVO language; most people in Japan participate in parameter-setting conventions that lead them to speak an SOV language.   (2006a: 181)

C6P104   This is where conventions get into my story of syntax: the members of a speech community have idiolects with the same grammatical rules partly because they acquire/learn the conventional parameter settings in that community.

C6P105   RC object to my talk of conventions. Set that objection aside for a moment. There seems to be another. With parameter setting in mind, I went on to describe the syntax of a language as "largely" conventional (p. 181). I have often used such

descriptions. RC (p. xx) quote an example (from Devitt 2020: 425) and add emphasis to its "largely." This suggests that RC may object to the view that syntax is "largely" non-innate, independently of their objection to the non-innate part being called "conventional." And, consider this: "*the vast majority* of rules and constraints on grammar are indeed due to innate psychological constraints and not convention" (p. xx). So, why do I say "largely"? Because, although theorists disagree about the list of parameters, they agree that the list is quite long. This yields literally millions of alternative settings for a language. That amounts to an awful lot of non-innate determination of syntactic rules, whether properly called "conventional" or not.

C6P106   But who knows how to count ways of determining syntax? I insist, and Chomskians surely agree, that *some* syntactic properties are not determined innately. Chomskians insist, and I agree, that *some* syntactic properties are determined innately. And we agree that it is theoretically interesting to discover which is which. Maybe "largely" is not the right quantifier to capture the proportion determined non-innately. Perhaps, it should be "a significant proportion." This does not seem to be a theoretically interesting issue.

C6P107   There may be another disagreement. RC may think that, whatever the *quantity* of non-innate syntax, that syntax is altogether not of much theoretical interest. The "deep" theoretical action is with the innate syntax:

C6P108   Despite occasional hyperbolic moments (e.g., Chomsky, 1980: 81–3, 1996: 47–8), Chomsky is not committed to denying that conventions play *any* role in language. He simply thinks they play far less a role than is commonly supposed, and sees no reason to think they play anything like the deep explanatory role of innate constraints . . . Chomsky claims the *laws* of linguistics are to be found in a nativist psychology, and regards social conventions as largely a matter of luck, and so not what linguistic theory targets. We take Devitt to be denying this.   (p. xx)

C6P109   Chomskyans expect that what lawful explanations there are will concern the stable facts about internal human mental structure, not about the complex mass of fortuitous relations speakers may or may not bear to the world around them.   (p. xx)

C6P110   So the idea is that the innate features of language determination differ from the non-innate ones in being "deep explanatory," adverted to in "laws," and "stable." I wonder about this developmental point. The cause of a person developing a language with certain grammatical rules, like the cause of an organism developing any phenotypic property, is to be found partly in what is innate and partly in the environment. Here's an example of the role of the environment. The Himalayan Rabbit, a breed of the Common Rabbit, comes to have white fur when raised in moderate temperatures but black fur when raised in cold temperatures (Sawin 1932). Similarly, a human comes to speak English when

raised in an English-speaking environment but Mandarin when raised in a Mandarin-speaking one. In both cases, the role of the environment seems law-like and stable. Should we say that it is less so than the role of what is innate? And not as deep? Perhaps so, but why? And what hangs on this? We still need the environment to explain the white fur and the English speaking.

C6P111   But, whatever we say about these developmental issues, a much more important point needs to be made: *there is more to "the explanatory scope of linguistics" than developmental laws. Sciences studies natures as well as laws.* Thus, science discovered that water is H_2O, that genes are DNA molecules, and so on.[19] Why spend scientific energy discovering these natures? Because entities, like water and genes, play their causal roles, hence may feature in laws, in virtue of their natures. Similarly, the symbols of a language play their causal roles, hence may feature in laws, in virtue of their natures, as noted in §4. So it is appropriate to spend scientific energy discovering those natures. And that, on the linguistic conception, is precisely what grammars do: they discover grammatical rules that constitute the syntactic part of the natures of the symbols that play striking causal roles in our lives.[20] So, *insofar as "linguistic theory" consists in grammars, it "targets" all grammatical rules whether innate or acquired, without discrimination*; the developmental story provides evidence about these rules but it is not what *grammars* target.

C6P112   In sum, haggling over the right quantifier to capture the proportion of non-innate syntax is not theoretically interesting. Even if innate syntax plays a deeper explanatory role than non-innate syntax in language development, they are equally explanatory of the nature of symbols. And explaining that nature is the task of grammars.

C6P113   Finally, I turn to the feature of my "conventionalism" that RC seem to find particularly objectionable: my calling the non-innate syntax "conventional." David Lewis claims that "it is a platitude—something that only a philosopher would dream of denying—that there are conventions of language" (1983: 166). RC are clearly not amused. They must also disapprove of this from Jerry Fodor: "think of a natural language as a system of conventional vehicles for the expression of thoughts (a view to which I know of no serious objections)" (1981: 196). Lewis and Fodor seem pretty right to me. When it comes to syntax, these claims need a qualification, of course, but the rest of language, and there is a lot of it, is explained by conventions. Indeed, languages provide a paradigm of explanation

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[19] RC state: "For brevity, we shall use 'laws' as a cover term for what good explanations ought to provide" (## n. 1). Well, explanations of natures—like those of water, genes, and, I claim, linguistic symbols—can be "good," but RC's use of "laws" does not seem to cover them.

[20] Cf. "Whether or not E-languages . . . are a scientifically important focus of attention depends . . . on whether there are actually any stable laws and generalizations about E-languages themselves" (Rey 2020b: 219–20). E-languages are scientifically important because their symbols play causal roles in virtue of their semantic and syntactic properties.

by convention. I have argued for this at length elsewhere (2006a: 178–84, 2008a: 216–21, 2008b: 251–4, 2021b: ch. 5). I shall not repeat these arguments, but RC prompt two further points.

C6P114   First, I have already quoted RC's concessive remark about the role of conventions: "Chomsky is not committed to denying that conventions play *any* role in language. He simply thinks they play far less a role than is commonly supposed" (p. xx).[21] And consider these remarks from Rey on his own:[22]

C6P115   Of course, some aspects of our speech may well be conventional . . . the fact that certain phonemes and not others are used to indicate single word meaning and grammatical structure. Many such facts may be due to people coordinating their speech with those of others.   (2020b: 94)

C6P116   Chomsky certainly has no particular reason to deny that the specific phonological forms that a speaker attaches to meanings and syntactic structures may sometimes be largely conventional.   (2020b: 220)

C6P117   The message I take from such passages is that Chomskians may accept that there are non-innate aspects of syntax explained by conventions, but are rather grudging about the *quantity* of them. Now, on the received Chomskian view of language acquisition, the only possible explanatory role for conventions is in parameter settings. If the Chomskian view is that conventions set *all* these, then we agree. So the grudging tone would just reflect a rather uninteresting haggle over the appropriate quantifier; see above. If, on the other hand, the view is that only *some* of these settings are by convention, then we wonder how the *others* are set.

C6P118   Second, we need to explain the *uniform* parameter settings within a speech community. How does it come about that most people in the USA set parameters that lead them to speak an SVO language, most people in Japan, an SOV language? What else but conventions could explain this? *This is the only theory in town to explain the uniformity.*[23] But is this too hasty? I have admitted that

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[21] Of course, Chomsky does have his "hyperbolic moments," as RC nicely put it. Here are two: Chomsky claims that the "regularities in usage" needed for linguistic conventions "are few and scattered" (1996: 47; see also 1980: 81–3). Furthermore, such conventions as there are do not have "any interesting bearing on the theory of meaning or knowledge of language" (1996: 48). I have responded (2006a: 178–89).

[22] Collins on his own, in contrast, has not been concessive (2008a, 2008b). I have responded (2008b, 2021b: §5.3). It is also worth noting that, not long ago, Rey presupposed an extensive role for linguistic conventions in his ingenious argument for anti-realism about SLEs (2006b: 558–9, 2008: 188–211).

[23] RC complain: "Devitt doesn't begin to establish that convention is essential to explaining these phenomena, many of which could be explained in terms of shared features of I-languages" (##n.11). But "shared features" is obviously an empty explanation of the sharing! Conventions explain the sharing. RC later remark: "just how parameters are set is a complex empirical issue, models ranging from mere 'triggering' accounts, to various forms of entrainment, where the setting one parameter depends upon the setting of another" (## n. 12). And it surely is a complex empirical issue. So too is the learning of many conventions. But how else can we explain the uniformity of much non-innate behavior but by appeal to conventions?

we don't have a complete and satisfactory theory of conventions to hand
(2006a: 179–80, 2021b: §5.3). Perhaps our notion of convention cannot bear the
explanatory burden here. We have been given no reason to believe that it cannot,
but should it turn out that it cannot we will need some similar, but more
"technical," social notion, X, to explain lots of uniform behavior, including
uniform parameter settings. This would certainly require a modification of my
view of language—my recent book (2021b) should have been called '*Overlooking*
X'!—but nothing serious. In particular, the modification is totally irrelevant to
"Devitt's Alternative" view of "the explanatory scope of linguistics." Discovering
that non-innate syntax was to be explained by X and not by convention would
make no difference to the linguistic conception.

C6P119   I quoted RC's statement of Chomsky's position ending with: "We take Devitt
to be denying this" (p. xx). Later RC claim that, in "many articles and three
books," I "emphatically" reject a "core claim" (p. xx) of a complex doctrine,
(PSY), that RC introduce as the "core psychological hypothesis" of generative
linguistics (p. xx). Now, there are many parts to these Chomskian position
statements. In light of our discussion, *the only significant part that I have ever
emphatically denied is the psychological conception of grammars*, the view that a
grammar is about a speaker's linguistic competence and hence about mental
states. My linguistic conception is indeed diametrically opposed to that. On the
linguistic conception, grammars "target" all the grammatical rules, whether
innate or not, that constitute the syntactic part of the natures of symbols. It is
in virtue of those natures that the symbols play their important causal roles. But
beyond that denial, despite RC's "conventionalism" mantra, my view of the role
of linguistic conventions does not deny much if anything of the Chomskian
position (hyperbole aside), and nothing significant. *This mantra is a red herring.*
(a) I do not deny RC's developmental claims about laws and explanatory depth,
though I do wonder about them; they are beside my concerns. (b) I do not deny,
of course, that there are innate aspects to syntax. (c) I do claim that all the non-
innate aspects are explained by conventions. Does this deny a Chomskian
position? If so, what precisely? I don't know of an alternative explanation, let
alone a better one. But if Chomskians come up with an alternative, X, any
argument over whether X is better is surely not of much significance to a view of
language and linguistics.

C6S8   6. Methodology

C6P120   As noted, Collins and Rey have each argued long and hard in earlier works for the
Chomskian psychological conception of grammars and against my linguistic
conception. The present chapter represents a change of tactic, reflected in the
"common-sense conventionalism" mantra. (Were they perhaps thinking, quite

correctly in my view, that the old tactic of arguing the matter wasn't working?) We have just discussed one part of that change: conflate the linguistic conception with "conventionalism." The other part charges me with an awful methodology and uses this to discredit my position. Whereas their Chomskian position is based on science, my position is based on nothing but common sense and so should be dismissed. I shall now discuss this damaging charge.

C6P121   RC sum up my alleged methodology as "reliance on common sense" (p. xx) This charge is not novel: Rey (2020a) has made it at length before, attributing to me a "commitment" to "Moorean Commonsense" (p. 307), and using this to discredit my views not only of linguistics but of much else: indeed, he wonders whether it is "a serious indictment of Devitt's work as a whole" (p. 324).

C6P122   My actual methodology is dictated by my Quinean epistemological naturalism (1998, 2011). This yields my attitude to common sense, or "folk theory." I described this briefly in my response to Rey:

C6P123   Folk theory can be a helpful place to start in the absence of science. We then look to science to discover whether folk theory, so far as it goes, is right. And we look to science to go further, much further. Some past folk theories have turned out to be spectacularly wrong. Still, given that conservatism is among the theoretical virtues (Quine and Ullian 1970: 43–53),[24] being in accord with common sense is an advantage for a theory, though, of course, very far from a decisive one. My most explicit and detailed presentation of this attitude to folk theory and common sense is probably in *Language and Reality*.

(Devitt and Sterelny 1999: 286–7,[25] 2020: 430)

C6P124   After citing a lot of evidence that this is a methodology that I practice as well as preach, I continued:

C6P125   In the face of all this obvious and apparently overwhelming evidence that I am very far from a devotee of "Moorean commonsense"...what we need [from Rey] is evidence that I engage in *arguments resting on* "*Moorean commonsense*." (p. 430)

C6P126   I point out that Rey (2020a) produces no such evidence (pp. 430–2).

C6P127   In light of this, if Rey, now joined by Collins, wishes to continue charging me with this common-sense methodology, we should expect him to produce evidence

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[24] "[T]heoretical conservatism is the only sensible policy for theorists of limited powers, who are duly modest about what they could accomplish after a fresh start" (Lewis 1986: 134).

[25] Note that the view is not, as RC misreport, that scientists "should be...*obliged*" to pay heed to folk theories (##11; emphasis added). The view is rather that paying heed "can be a helpful place to start"; proto-science can lead to science.

that my arguments really do rest on common sense. RC imply that this is what they will do:[26]

C6P128   Devitt (2020: 429–32) protested, claiming no cases had been cited where he was in fact relying on common sense. In §2 below, we shall set out Devitt's general views about the role of common sense in metaphysics (§2.1), proceeding to indicate in detail how it seems to be the basis of his disagreements with current scientific linguistics.   (p. xx)

C6P129   Now the promise to set out my "general views about the role of common sense" is vague. The promise that they need to make, *given their methodological charge*, is to show that I *rely on common sense in my arguments* in metaphysics. RC do not deliver on that promise. Rather, in §2, "Devitt's Common-Sense Linguistics," they try to convey an impression of giving evidence for their charge while in fact giving none that should be taken seriously. Let us consider §2.

C6P130   In §2.1, RC base their description of my views of the role of common sense in metaphysics on the Moorean strategy that I use (1991, 2010) as *part of* my argument for realism about the external world (p. xx). That strategy does indeed stress that realism about ordinary physical objects like stones, trees, and cats is the core of common sense. RC's discussion of the strategy is similar to Rey's earlier one (2020a: 301–3). In responding to Rey, I emphasized two points. First, the strategy "*involves no commitment to common sense.*" Second, the argument for that realism does not rest on this strategy but on "naturalism" (2020: 431). I expanded on this:

C6P131   "the Moorean response is not of course sufficient" (2010: 63). Indeed, how could it be? "Realism might be wrong: it is an overarching empirical hypothesis in science" (1991: 20). So, I follow up with a naturalistic argument for it (2010: 63–66; see also 1991: 73–82). (p. 431)

C6P132   I concluded that Rey had provided "no basis at all for attributing to me the methodology that [he] disparages with such relish" (p. 431).

C6P133   Obviously, I made these two points to refute Rey's charge that my argument for realism relies on common sense. So, how do RC respond to the points? They may seem to be taking some account of them in remarking that "while Devitt endorses the Moorean stratagem, in his (2020: 430) he endorses a defense of common sense based more on Quine and Ullian (1970: 43–53)" (p. xx). But, we should note, though the naturalistic methodology I have described certainly

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[26]   RC mention Rey's defense (2020a) of his "maxim of 'Explanation first!,' against Devitt's (2010) maxim of 'Metaphysics first'" (##3) as if it were relevant to their methodological charge. It is not, for reasons to be found in my response (2020: 428): I agree with Rey's maxim and it is not in competition with mine. The maxims have different purposes.

implies the (unsurprising) view that science might endorse *some* common sense, the methodology is far from a blanket "defense of common sense." More importantly, if my realism argument exemplifies that naturalistic methodology, as I claim, then that argument is not what RC need: it's not an argument resting on common sense. RC make no attempt to show that my realism argument does not exemplify that naturalistic methodology.

C6P134   Instead, RC make this revealing remark:

C6P135   We should also stress that, while Devitt endorses common sense both broadly and with particular respect to language, he doesn't always endorse these positions *because* they are common sense; he simply thinks that common sense happens to be correct across a range of cases. Our intent is to dispute this.   (p. xx)

C6P136   So RC think that I endorse some common-sense views that I should not, including, of course, views about language (though not including, I gather, realism about the likes of stones, trees, and cats). Fair enough. So RC can argue against those endorsements, as indeed they used to. But their new tactic requires them to show that I *rely on* common sense in endorsing those views. As they nicely point out, endorsing a common-sense view is not the same as endorsing it *because* it is common sense. Their new tactic requires them to show that I do the latter, to show that I *rely on* common sense. In claiming that I don't "always" do so, they imply that they have shown that I sometimes do so. Indeed, the implication is that their very discussion of my realism argument has shown this. Later, they make it very clear that this is exactly what they think that they have accomplished:

C6P137   Still, as part of his general commitment to at least the "posits" of common sense, he would seem to be assuming that we can, by and large, read off much of what exists from ordinary talk. (p. xx)

C6P138   This is a breathtaking misrepresentation. Of course, we cannot "read off much of what exists," or much of anything at all, "from ordinary talk"; see the description of my "actual methodology," and citations, in the long quote (from Devitt 2020: 430) displayed two pages back.[27] For, to repeat, my realism argument exemplifies

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[27] RC have an earlier note containing another serious misrepresentation of my argument for realism: they claim that the doctrine, (R), "Words refer to things in the world"

seems to us implicit in many passages of Devitt . . . especially in his defense of "most" common-sense posits (1984/97: 18, 23, 73), which we presume are what are referred to in common-sense talk. (##n. 2)

This careless note could hardly be further from the truth. Perhaps the most distinctive feature of *Realism and Truth* (1991, cited above as 1984/97) is that it *explicitly excludes* all semantic claims, hence excludes (R), from a defense of realism. Thus, Maxim 3, the book's most active maxim, reads as follows: "Settle the realism issue before any epistemic/semantic issue" (1991: 4). See also the maxim "Put Metaphysics First" mentioned in the previous note, which is also the name of a book (2010).

the naturalistic methodology that I espouse not the common-sense one that RC foist on me. Rather than attempting to show otherwise, RC are simply assuming otherwise.

C6P139   So RC have not yet produced any evidence of my offering arguments that rest on common sense. This is important because their discussion of my view of linguistics, in "2.3 Devitt's Alternative," proceeds as if they had shown this: as if their discussion of my realism argument has made it plausible that my alleged reliance on common sense in metaphysics is "the basis of [Devitt's] disagreements with current scientific linguistics." Indeed, that is the point of their discussion of the realism argument.

C6P140   So, let us return to RC's discussion of "Devitt's Alternative," this time looking critically at its claimed evidence that my linguistic views rely on common sense. I have already labeled one of these claims "uncharitable speculation" (§4) and that fits them all. RC produce no real evidence. But proving an absence is tricky and in danger of being tedious. I shall content myself with discussing the three passages where RC most clearly represent themselves as providing the needed evidence of my methodology.

C6P141   These passages are of two sorts. In the first sort, RC *disparage* my actual argument that does not rest on common sense to speculate that my real reason does. In the second sort, RC *ignore* my actual argument that does not rest on common sense to speculate that my real reason does. I'll start with the first sort.

C6P142   (1) RC make a claim of this sort in discussing my view of how conventions might explain unvoiced syntax: "At one point [Devitt] does provide a sketch of how he thinks conventions could have evolved regarding inaudible elements, such as PRO." After quoting the sketch, RC comment: "But it's hard to believe he thinks this is a serious explanation." RC go on to indicate why they have such a dim view of the explanation before concluding:

C6P143   For lack of any serious argument or alternative theory of these standard linguistic phenomena, it's extremely hard to see how Devitt is relying on anything more than Lewis's common-sense "platitude" (p. xx)

C6P144   But disparaging an argument that makes no mention of common sense, and that I give every sign of believing, is clearly not a good basis for claiming that I am really relying on common sense. Furthermore, their disparagement is mistaken as I have, in effect, argued in a response (2008b: 252–4) to a similar criticism from Collins alone (2008b: 244–5). This argument of mine provides further evidence that the explanation that RC disparage, not their claimed insight into my mind, is indeed the reason for my view of unvoiced syntax. RC do not mention this argument.

C6P145   One further comment is called for. My sketch was of how PRO might be acquired conventionally, but I allowed that it might in fact be innate (2008b: 253). Suppose

it is. Still, surely some unvoiced syntax is not innate. How is it to be explained? My discussion of PRO is a sketch of how any unvoiced syntax might be acquired conventionally. Given their disparagement of this, RC owe us an alternative. How do they explain the *uniform* acquisition of non-innate unvoiced syntax? What is their *X* (see §5 on "the only theory in town")? Or do RC claim that all unvoiced syntax is innate? Even that in, "Mary went to visit the zoo, John, the museum"? That would be a bold claim.

C6P146   (2) In the second sort of claim, RC ignore rather than disparage an actual argument that does not rest on common sense to speculate that my real reason does. We have already come across an example of this sort in §4. After a nice account of what the linguistic conception (LR) opposes, RC offer this as the reason for my opposition:

C6P147   This seems to be partly due to his insisting upon a common-sense conception about the reality of standard linguistic entities ("SLEs," such as words, phrases, sentence, phonemes and/or phonological properties) and the conventions governing them . . . (p. xx)

C6P148   This uncharitable speculation is manifestly false: my actual reasons for the linguistic conception are to be found in many places and are known only too well to RC because they used to argue against them. See §4 for further information and discussion.

C6P149   A second claim of this sort follows another quote from my discussion of unvoiced syntax. RC comment on this discussion as follows:

C6P150   Note that, quite apart from its disregard to the complexities of syntax, Devitt's work contains no discussions of phonology and doesn't engage in most of the semantic issues that typically occupy linguists (e.g., NPIs, conditionals, (a)telic verbs, quantifiers, adverbs, compositionality) . . . . So it's puzzling that Devitt is prepared to second-guess linguists about the nature of their task. Although Devitt would vehemently protest this charge, it is hard not to regard his proclamations as mere armchair, virtually *a priori* "common-sense" speculations that he (2010: 276–7) otherwise often reasonably deplores.   (p. xx)

C6P151   There is no puzzle about why I urge my allegedly "second-guessing" view of the grammatical task: simply look at my arguments for the linguistic conception, arguments that RC used to try to answer and are now carefully ignoring.

C6P152   What about the criticism that I do not discuss various linguistic topics—phonemes, NPIs, and so on—topics that have yielded many interesting discoveries? I anticipated this sort of criticism in the "Preface" of *Ignorance of Language* and responded: "what needs to be asked is whether the discovery is relevant to the issue in question" (2006a: vii). The issue in question here is the linguistic

conception of grammars. One key feature of the mentioned topics is very relevant to this issue, the feature that the topics are *all about linguistic expressions*. Thus, commenting on the grammatical claim, "An NPI must be *c*-commanded by a licensor," Rey rightly remarks: "An NPI would certainly appear to be a word . . . , tokens of which are uttered or appear in print on a page" (2020b: 213). Exactly. And, of course, I certainly discuss, indeed, emphasize this feature. So far as I know, no other feature of those topics is relevant to the linguistic conception. If RC think otherwise, they should demonstrate.

C6P153   A word about the "second-guessing." There is no doubt that Chomskian philosophers of linguistics embrace the psychological conception of grammars (it's a price of survival). Perhaps most linguists working in the Chomskian tradition will say that they do too:[28] it is, after all, what they are told firmly in Syntax 101. But, as the argument for the linguistic conception shows, the psychological conception is at odds with *what linguists actually do in constructing grammars*. For, the claims that fill grammar books, as Rey has just illustrated, are about linguistic expressions (SLEs) not about the mind. *Prima facie*, the scientific task that these linguists are actually performing, whatever they say about it, is the task of explaining the syntactic nature of expressions. If these linguists *really believe*, rather than *just say*, that grammars are about the mental, then they should have in mind a thorough-going rewrite of their grammar books. Is there any evidence that they do?[29] As I recently remarked:

C6P154   Given the centrality of the psychological conception to the promotion of Chomskian linguistics, it is striking that there has been so little sensitivity to [the need for paraphrase] in presentations of grammars. Sensitivity to the ontology of one's theory is a mark of good science. (2020: 377–8)

C6P155   In a note to the just-quoted passage from their chapter, RC acknowledge that I "would certainly appear to be pursuing" a "naturalistic" approach in much of my work. But they add: "We are only questioning the extent to which [Devitt] appears nevertheless to be *also* implicitly relying on a traditional armchair or common-sense methodology, despite his best naturalistic intentions" (p. xx n. 12). But where is the *evidence* of this implicit reliance? What we have just discussed is the best RC have to offer. It is not evidence to be taken seriously. Given the arguments I actually provide for my views of language and linguistics, RC need much more to justify their methodological charge than what they claim to find "hard to see" and "hard not to regard" as they peer into my mind.

[28] But some eminent linguists certainly do not; see, e.g., Gazdar et al. 1985.
[29] One is reminded of the old debate over scientific realism. It was apparently once common for physicists, when asked, to assent to the sort of positivistic instrumentalism that they had been taught in undergraduate philosophy classes. Yet, as realists were fond of pointing out, the actual practice of those physicists showed that they were, deep down, realists not instrumentalists.

C6P156   RC, like Rey alone, have not produced a single example of an argument of mine, let alone one on linguistics, that relies on common sense. How then should we explain their persistent baseless misrepresentation of my methodology? My hypothesis is that RC are driven by a sort of "transcendental argument": "The scientific case for the Chomskian psychological conception of grammars is so overwhelming that we know *a priori* that rejection of that conception must be based solely on appeals to common sense." We have a case of *just knowing* that the election was stolen.

C6S9   ## 7. Conclusion

C6P157   §I of the chapter was concerned with two questions: To what extent are linguistic tokens in utterances accidental relative to linguistic rules and hence are "*r*-lucky." To what extent are linguistic rules, hence expressions governed by them, accidental relative to human nature and hence are "*i*-lucky."

C6P158   Linguistic pragmatists (contextualists) have argued, in effect, that there is a lot more *r*-luck around than has customarily been thought. Using a range of striking examples, they argue that a token's message is never, or hardly ever, solely constituted in the traditional way of truth-conditional semantics. Rather, it is always, or almost always, constituted pragmatically in context. I presented a case, drawing on more detailed presentations elsewhere, that the pragmatists are not right about this (§2). So I am urging a sort of "anti-luck semantics."

C6P159   Chomsky has argued, in effect, that there is a lot less *i*-luck than used to be thought. For, he thinks that some fundamental rules of syntax, the rules of UG, are innately determined. So they are *i*-unlucky. I agreed (§3).

C6P160   In §II, I considered Rey and Collins' (RC's) critique (Chapter 5 of this volume) of my views of language and linguistics, views that underlie my theses about linguistic luck. They are particularly concerned to rebut my alternative view to the Chomskians' of "the explanatory scope of linguistics." That alternative is the "linguistic conception" of grammars according to which a grammar is about a non-psychological realm of linguistic expressions, physical entities forming a representational system. This contrasts with the received Chomskian psychological conception according to which a grammar is about a speaker's linguistic competence and hence about mental states.

C6P161   Collins and Rey have each, independently, argued long and hard in earlier works for the psychological conception and against my linguistic one. The mantra of their present joint chapter, "common-sense conventionalism," represents a change of tactic. With one half of the mantra, RC conflate the linguistic conception with my alleged "conventionalism." "Conventionalism" is a red herring. Such disagreement as there may be over the role of conventions is not of much significance generally and is *totally irrelevant* to my alternative view of the scope

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of linguistics. RC's discussion does not even bear on my real alternative, the linguistic conception (§§4 and 5).

C6P162   The other half of RC's mantra charges me with an awful methodology and uses this to discredit my position. Whereas their Chomskian position is based on science, my position is alleged to be based on nothing but common sense and so should be dismissed. This charge about my methodology is baseless. My actual methodology, both practiced and preached, stems from my Quinean epistemological naturalism. RC do not present a single argument from my work that shows otherwise. Rather, ignoring or disparaging my actual arguments, they claim, looking into my mind, to *just see* that the real reason for my views about language and linguistics is an appeal to common sense. This is not an adequate basis for their methodological charge, to put it delicately (§6).

C6P163   In sum, RC's critique is a combination of red herring and egregious misrepresentation.[30]

C6S10   **References**

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