

A Response to Collins' Note on Conventions And Unvoiced Syntax

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This paper takes up the two main points in John Collins "Note" (2008b), which responds to my paper, "Explanation and Reality in Linguistics" (2008). (1) Appealing to what grammars actually say, the paper argues that they primarily explain the nature of linguistic expressions. (2) The paper responds to Collins' criticisms of my view that these expressions have many of their properties by convention.

Key words: Key words: linguistic expressions, conventions and PRO, linguistic realism, unvoiced syntax

I will take up the two main points in John Collins "Note" (2008b), which responds to my paper, "Explanation and Reality in Linguistics" (2008). Both papers are in this issue.¹

1. What Does a Grammar Explain?

According to my "linguistic conception" a grammar explains the nature of linguistic expressions. These expressions are concrete entities external to the mind, exemplified by the very words on this page. Thus a grammar helps explain the nature of 'Bob's brother loves himself' by yielding the claim that 'Bob's brother' c-commands 'himself'. I accept that a grammar also contributes to the explanation of a competent speaker's cognitive life because that speaker must "respect" the rules posited by the grammar. Still, the contribution is not that great and it is not what a grammar primarily and straightforwardly does. I label Collins contrary claims that the *only* interesting explanations in linguistics are of cognitive matters "*ex cathedra* pronouncements."

Collins now claims to have *argued* (in 2008a) "that the explanations of generative linguistics neither target nor essentially presuppose ex-

¹ All unidentified references to Collins are to his "Note" and, to me are to this paper.

ternal entities" (###). But those "arguments" are all on a par with his present claim that "structural ambiguity appears to be a cognitive phenomenon, whose explanation, for a given class of constructions, does not appear to involve appeal to an external language" (###). I don't think the ambiguity appears to be this at all. Rather, it appears to be a linguistic phenomenon found in certain arrangements of expressions. Collins gives no argument here or earlier.

How can we *tell* what grammars explain? A good way to start is to see what a grammar *actually says*. As I have pointed out, grammars are full of claims about NPs, anaphors, pronouns, and the like. *Prima facie* these claims are explaining the properties of linguistic expressions.² But Collins objects to the idea that "one could simply read off the linguists' metaphysical commitments from such an uncritical reflection on the language the linguists use" (###). Now, *ultimately*, we have no alternative but to "take language for granted" in assessing the metaphysical commitments of *any* theory, no alternative to simply understanding the theory in its context. Still, Collins is surely right that we should not do this uncritically. So let us look further into a grammar. Opening Liliane Haegeman's popular textbook at random I find a discussion of "layered VPs" (1994, 88–9). 'VP' is of course a technical term but our expectation of what it means, given that it is short for 'verb phrase', is confirmed by its use in the text to refer to the part of 'Miss Marples will read the letters in the garden shed this afternoon' that follows 'will'. And that part is a linguistic expression. So the discussion is explaining the nature of such expressions. Collins criticizes me for not dedicating "a good deal of effort to mak[ing] sense of current syntactic theory" according to my linguistic conception (###). Well, I've just spent a little bit of effort. I did not spend much before because it seems *obvious* how we can make sense of syntactic theory according to my conception: we can treat just about any page of any syntax text in the way I have just illustrated. *Simply taking grammars to mean what they say, without any revision or reinterpretation, they give empirical explanations of linguistic expressions*. Collins wants to be shown that "the appeal to apparently external languages is an *essential* presupposition of the explanations" (###). Well, of course it is: external languages are *what are being explained!* Collins sees a problem for the linguistic conception where there is none (but see below).³

We should note further that there is nothing explicitly cognitive in these two pages of Haegeman. Of course, if we supposed that VPs really were mental states, then the pages would be cognitive. But why suppose

² There is room for argument whether this should be taken as about linguistic types—abstract objects—as Jerry Katz prefers, or as really about tokens of that type as I prefer (2006a, 26–7).

³ This nonexistent problem dominates Collins response to my book as became more and more apparent in lengthy email exchanges. These exchanges, which also involved Barry Smith and Georges Rey, were very helpful. I am indebted to all parties.

that? Nothing that goes into grammar construction warrants the robust psychological assumption that there are mental states with the properties the grammar ascribes to VPs.⁴ This having been said, we could of course use the explanation of VPs to explain something cognitive: it provides the beginnings of an explanation of a competent speaker's use of VPs (##13). And Collins and I pretty much agree on what is thus explained (##7–8).⁵ But this does not undermine the point that grammars, as they stand, explain the nature of expressions.

The linguistic conception is not at odds with generative syntactic theories. It is at odds with the Chomskian theory of those theories. For that meta-theory is the psychological conception of linguistics, precisely what the linguistic conception opposes. The wonderful success of generative grammars supports the grammars not the meta-theory.

A presupposition of this discussion is that there is a realm of “external” linguistic expressions. If there are these expressions then we have good reason to take a grammar as literally descriptive of them.⁶ I shall not rehearse the arguments I have given for this “linguistic realism” (2006b, 2008) in the face of earlier criticisms by Collins and, particularly, Georges Rey. However, I will address Collins' further remarks on the matter in his “Note.”

2. *Conventions and PRO*

I accept that linguistic expressions have some of their syntactic properties as a result of innate constraints on the human mind (2006a, 248–52). But any linguistic expression must have many properties by convention. So if there could not be the required conventions linguistic realism would have to be abandoned. Collins seems to have three concerns about this talk of conventions.

- (i) He seems to doubt that talk of conventions could be theoretically respectable *anywhere* until it has been adequately explained. In

⁴ Of course the language-of-thought hypothesis might yield that assumption. I favor that hypothesis (2006a, 145–7) but it is not something that you get for free with a grammar.

⁵ Although Collins thinks that this explains something about *the language faculty*. In my view, a grammar tells us no more about a language faculty than an economic theory tells us about an economic faculty (###). And I am dubious of the existence of the language faculty for other reasons (2006a, 163–74, 256–66).

⁶ There is a parallel between us and other animals. Many of them produce behaviors that deploy representational systems to communicate. So do we: that is linguistic realism. Cognitive ethologists study the nature of those animal systems. Linguists study the nature of ours: that is the linguistic conception. I emphasize that the study of the nature of a representational system is not to be confused with the study of the cause of the behavior that produces the system (##14), and even less with a *behaviorist* study of that cause. (In this respect, it is worth noting that symbols often, although not always, have a life beyond the behavior that produces them. This is obvious with inscriptions.) Collins hates it when I talk of animals, but our interest in human representational systems, different as they undoubtedly are from all others, should not be divorced from our interest in other such systems in nature.

presenting the beginnings of an explanation I gesture toward David Lewis' classic account (1969) without committing to it because I find it "highly intellectualized." I acknowledge that I have not given a full account of my own (2006a, 179–80). Because of this, Collins claims that "the relevant notion of convention remains wholly opaque" (###4; see also 2008a, 35) and hence, by implication, unacceptable. This is simply not so. Lewis has given a range of examples which show well enough how there can be conventions without any explicit agreement (1969, 36–42).⁷ We need to talk of conventions to explain much human behavior—most pertinently, human communication—and even some animal behavior (###14). Science is replete with notions that have not yet been fully explained.⁸

- (ii) Whatever his attitude to talk of conventions in general Collins is uncomfortable with the talk having any place in linguistics *in particular*: he expresses "a certain consternation at the very idea that syntax is conventional" (###3). Yet it would seem almost perverse to deny that any syntax was conventional. Consider my example (2006a, 181): Americans speak an SVO language, Japanese, an SOV language. What could explain this but different conventions?
- (iii) He is particularly doubtful that linguistic expressions could have unvoiced syntactic elements like PRO by convention. In the standard analysis, 'Bob tried to swim' has two instances of PRO, one being a copy of the other:

Bob tried [PRO to [<PRO> swim]]⁹

"[T]he pair of PROs is posited for theoretical reasons" (###3) which Collins had earlier explained as follows: "We take PRO to move from its verbal position to SPEC of infinite TP position in order to check the uninterpretable EPP feature of the tense head" (2008a, 33). The problem for me is that these theoretical reasons "are quite distant from the communicative intentions of speaker/hearers that, presumably, are supposed to fix the conventions...the two instances of PRO in infinitive constructions lack any clear semantic/communicative rationale of the kind to which a convention might likely attach" (###3). They are posited for "internal syntactic reasons that communicators have never cared about and presumably never will" (###4). So, the alleged problem is that the

⁷ Collins' criticism of my discussion of ellipsis makes me wonder whether he is under the misapprehension that conventions have to be by explicit agreement: discussing 'Mary went to the bank and so did Bob' he asks: "Are we to imagine parents agreeing with their children to use *do* so that it picks up a particular antecedent?" (2008a, 36 n. 45).

⁸ An example is the notion of *social play*, important in cognitive ethology. One is tempted by accounts of this that make social playing too intellectual an activity to be plausibly attributed to many of the organisms that engage in it (Allen and Bekoff 1994). In this respect, playing is like participating in a convention.

⁹ More precisely: [TP [DP Bob] [T' [T -ed] [VP [V try] [TP [DP PRO] [T' [T to] [VP [DP <PRO>] [V swim]]]]]]]. This analysis may, of course, be false, but Collins and I are accepting it for the sake of argument.

posits are so highly theoretical that they are not the sort of thing that speakers might intend, notice, or care about. Yet, interestingly, Collins adds: “With a single PRO, one can at least imagine a story where an initial reflexive reading (Devitt’s ‘speaker meaning’, if one likes) of the subject position of the infinitive clause would support a conventional introduction of PRO” (###4). But his imagination fails with the two instances of PRO. These arise from the quite general requirement that “all arguments start in verbal internal positions” and so we must see the “first” PRO as having moved from that position (2008a, 34).

Collins is very dissatisfied with my earlier response to this alleged problem (##15–16). He does not claim that it is “logically incoherent” (##4) that ‘Bob tried to swim’ has two instances of PRO by convention but he does find it unimaginable. I think it is quite imaginable.

Before attempting to say why, a clarification is in order. My earlier response presupposed that the allegedly problematic PRO was *not* determined by *innate* constraints on syntax (UG). For if it were so determined, then of course all natural languages must have PRO, with its two instances, and there would be no need to explain how linguistic expressions come to have this by convention: PRO would be a non-optional feature of speaker syntax (2006a, 180–3). I have since realized that this presupposition may very well be false.¹⁰ Still, it may not be. In any case, PRO serves as a good example of the *sort* of highly abstract syntactic properties that, on my account, must sometimes, at least, be determined by convention. So I will continue treating it as an example of such a property.

Although I think that it is quite imaginable that the two instances of PRO are conventional, I do not pretend to be able to fully explain how this convention might be established. But we are as yet unable to fully explain how *any* convention (that is not stipulated) is established. Yet, as Lewis demonstrates, there are thousands of such conventions. And the more abstract the conventional property, the harder it seems to explain. It is easy to imagine how ‘swim’ became a verb by convention. And it is not so hard to imagine how a certain order of words of certain syntactic categories became a verb phrase or a sentence by convention. The imagination gets a little more strained with the unvoiced elements. Still if Collins can manage a conventional single PRO he ought to be able to manage a conventional double PRO, for the latter is no more problematic in principle than the former. I shall briefly try to say why.

All the posits of a scientific theory, however “theoretical,” must play a causal role else it would be idle to posit them. This applies to the posits of a grammar. On the linguistic conception, the feature that “all arguments start in verbal internal position”—the feature that requires two instances of PRO rather than one—is (we are supposing) a feature of conventional syntax. This feature partly determines (largely) conventional meanings and hence plays its causal role through the causal role

¹⁰ Thanks to Georges Rey and John Collins.

of those meanings in acquisition and communication (##17, 22–26). But, on the Gricean story, this feature of conventional syntax exists only because of regularities in *speaker* syntax (2006a, 179–81): it is because arguments regularly started in verbal internal position in speaker syntax that they do so in conventional syntax. This feature of speaker syntax partly determines speaker meanings and hence plays its causal role through the causal role of those meanings in acquisition and communication (##17, 22–26). If there were a different feature, there would be a different speaker meaning playing a different role. Speaker syntax is causally significant.

Given Collins' psychological conception of linguistics, he must accept that the requirement that arguments start in verbal internal position is causally significant to cognition (else this grammatical posit would be idle). On my view the speaker syntax is determined somehow by the thought expressed and the process of production (2006a, 155–6). So the significance of the requirement to cognition is that the thought and the process must be such as to determine a speaker syntax that meets the requirement.

So, given that arguments starting in verbal internal positions is a regular feature of speaker syntax, a feature that makes a causal difference, how does that turn into a feature of conventional syntax? This must come about from hearers grasping, in some sense, this regular feature of speaker syntax in the process of grasping the speaker meanings of utterances that are partly determined by that feature. But this "grasping" need not involve "high-level-cognitive" intending, noticing or caring about: it could be "implicit" and "procedural" rather than "explicit" and "declarative" (2006a, 210–20). This grasping has been famously exemplified by humans in learning the conventions of "artificial grammars." And it is surely what dolphins exemplified in learning the conventions of a primitive OSV language.¹¹

Linguistic realism is *prima facie* plausible. The case I have presented for it elsewhere (2006a, b, 2008) seems to me powerful. Our commitment to this realism should not be shaken by any difficulties in imagining conventions for unvoiced elements of syntax. Given that there are these linguistic expressions external to the mind, the theoretical interest in studying them is clear. Or so I have argued (## 10–13).¹² The linguistic conception is sustained.

¹¹ A dolphin, Akeakamai, mastered three-item relational sequences like 'Surfboard swimmer fetch' meaning that she was to bring the swimmer to the surfboard, and three-item nonrelational sequences like 'Left hoop through' meaning that she was to swim through the hoop to her left. Then, without training, she was given four-item ('Right hoop frisby fetch') and a five-item ('Right basket left ball in') sequences which combined both syntactic structures. "Her understanding was immediate." (Herman 2006, 445)

¹² My argument is a response to Collins' main objection to the linguistic conception (2008a). He could not, of course, respond to everything in his "Note" but, in the interests of keeping score, I observe that he has not responded to this argument for the theoretical interest of studying linguistic expression.

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