

Syntactic Evidence for Semantic Claims

Can syntactic considerations provide evidence for semantic claims? More specifically, can the fact that an expression exhibits certain sorts of syntactic behavior provide evidence for the claim that it has a certain semantic feature? Let's call the claim given by an affirmative answer to this question the Syntactic Support for Semantic Conclusions Thesis, or SST for short. One way to defend SST would be to provide an argument (some of) whose premises are claims about the syntactic behavior of an expression and whose conclusion is about the semantics of that expression such that the argument is worthy of our endorsement. To get clear on the kinds of arguments one might put forward to this end, consider three candidates for such arguments. First, suppose one thought that when 'There' insertion sentences, such as the following, are "bad" (indicated by *), they are ungrammatical:

(1a) *There is every student.

(1b) There is a student.

Then if one thought that the determiners that allow for grammatical 'There' insertion sentences (here 'a') have a certain semantic feature (e.g. that the determiner's denotation has a certain mathematical feature as suggested in e.g. Barwise and Cooper (1980), Keenan (1987), and Keenan and Westerstahl (1997)), the syntactic fact that a determiner allows for grammatical 'There' insertion sentences would provide evidence for the claim that the determiner had the semantic feature in question. One version of the relevant argument here would look schematically as follows:

Argument TI

(i) All and only determiners (expressions of syntactic type S) with semantic feature A can occur grammatically in 'there' insertion sentences.

(ii) Determiner d (expression d of syntactic type S) can/can't occur grammatically in 'there' insertion sentences.

(iii) So determiner d has/lacks semantic feature A.

If this argument is sound, it seems clear that SST is true. For though TI's premises include semantic claims (premise (i)), it certainly seems reasonable to say that TI takes the syntactic behavior of an expression (noted in premise (ii)) as evidence for a semantic claim about the expression.

Consider a second example. Suppose one thought that when negative polarity items (NPIs) aren't licensed, the result is an ungrammatical sentence:¹

(2) * John has any money

Then if one thought that expressions that license NPIs are precisely those that have "monotone decreasing" elements to their meanings², the syntactical fact that an expression licenses/doesn't license NPIs would provide evidence that it does/doesn't have a "monotone decreasing" element to its meaning. For example, the grammaticality of the first of the following sentences and the ungrammaticality of the second would provide evidence that the determiner 'every' has the semantic features that Heim and Kratzer (1998) call left downward monotone and right upward monotone, respectively:

(3a) Every student who has any money owns a computer.

(3b) *Every student earns any money.

Here the argument being deployed would look schematically as follows:

Argument NPI

¹ Cheirchia and McConnell-Ginet (2000) endorse this view (see pps. 517-522, and especially p. 521)

² Cheirchia and McConnell-Ginet (2000) endorse this view as well (see pps. 517-522, and especially p. 521). Thus presumably they would endorse Argument NPI below, and so endorse SST.

(i) All and only expressions with semantic feature A license negative polarity items, (where licensing such items means allowing grammatical sentences containing such items).

(ii) Expression e licenses negative polarity items.

(iii) So expression e has semantic feature A.

Again here, if the argument is sound, it seems clear SST is true. For again, it seems reasonable to say that argument NPI takes the syntactic behavior of an expression (noted in premise (ii)) as evidence for a semantic claim about the expression.

A final example. Suppose one thought that the distinction between stage level and individual level predicates was a semantic distinction between predicates that express transitory properties of things, like being available, and predicates that express more enduring properties of things, like being altruistic, respectively. Further, suppose one thought that the fact that stage level predicates can and individual level predicates cannot occur as follows in ‘there’ insertion sentences was a syntactic fact (i.e. that the second sentence below is ungrammatical):

(4a) There are fireman available.

(4b) *There are fireman altruistic.

Then one would think that the syntactic fact that a predicate can/cannot so occur in ‘there’ insertion sentences was evidence for the semantic claim that it is a stage level/individual level predicate.³ Here the argument would run as follows:

Argument SI

(i) Stage level predicates can and individual level predicates cannot grammatically occur in the relevant way in ‘there’ insertion sentences.

³ Kratzer 1988 endorses all these claims and hence would presumably endorse argument SI, and so SST.

(ii) Predicate P can/cannot so occur in such sentences.

(iii) So predicate P is a stage level/individual level predicate.

Once more here, if the argument is sound, it seems clear SST is true. For again, it seems reasonable to say that argument SI takes the syntactic behavior of an expression (noted in premise (ii)) as evidence for a semantic claim about the expression.

Other examples of candidate arguments that, if sound, entail that SST is true could be given as well.⁴ I hasten to add that I am not claiming that the arguments TI, NPI and SI show that SST is true. In giving these examples I wrote things like: “Suppose one thought that when ‘There’ insertion sentences, such as the following, are “bad” (indicated by *), they are ungrammatical” etc. and I never gave reasons in favor of these suppositions, some of which, this one included, are controversial. I simply display arguments TI, NPI and SI as examples of the sorts of arguments one would have to put forward to defend SST. I also wish to note that one who puts forward arguments like TI, NPI or SI need not be claiming that the semantic feature mentioned in the conclusion explains the syntactic behavior mentioned in the premises. She need only hold that possession of the semantic feature is correlated with possession of the syntactic feature. Finally, because it will be relevant later, it is worth remarking that one might put forward an argument that is more cautious than TI, NPI or SI and still be properly viewed as endorsing SST. Suppose, for example, that one put forward an instance of the following argument schema:

Argument C

(i) All and only known, uncontroversial examples of expressions with semantic feature A exhibit syntactic behavior R.

⁴ For example, if one thought that sentences in which quantifiers fail to support donkey anaphora are ungrammatical (on the relevant indexing--*‘If Sue owns every donkey₁, she feeds it₁.’) and if one thought that quantifiers that support donkey anaphora share a certain semantic feature, one would endorse an argument that entails SST.

(ii) Expression e (whose semantics is in question) exhibits syntactic behavior R.

(iii) (i) and (ii) provide evidence that expression e has semantic feature A.

(iv) Thus, there is some evidence that expression e has semantic feature A.

It seems clear that anyone putting such an argument forward is thereby endorsing SST, and that if the argument is sound, SST is true. After all, SST is merely the claim that the fact that an expression exhibits certain sorts of syntactic behavior provides evidence for the claim that it has a certain semantic feature. And if (an instance of) argument C is sound, this claim is true. This should make clear that SST is a fairly weak claim. Those who deny it hold that the syntactic behavior of an expression never provides any evidence for the claim that it has some semantic feature.

When I reflect on the above arguments and others that would support SST, it seems to me that SST is quite plausible. Indeed, I confess that I think it is true. In fact, I am committed to its truth: in my book Complex Demonstratives: A Quantificational Account (henceforth CD), I claimed that there is syntactic evidence that complex demonstratives are quantificational. By contrast, in a recent paper, Kent Johnson and Ernie Lepore (henceforth JL) express a surprisingly strong skepticism about SST.⁵ The purpose of the present work is to show that in the end JL provide no reason for any degree of skepticism about SST.

Let's begin by looking at passages in which JL express skepticism about SST and the reasons for their skepticism. It is clear that JL mean to express strong skepticism about SST. Part of the title of their paper is 'Does Syntax Reveal Semantics?', and the tone and content of their remarks clearly suggest that the answer is 'no'. Consider the

⁵ Johnson and Lepore 2002.

following remarks from the opening of JL's paper, (I quote them at length to avoid distorting their views):

Studying the syntax of natural language was fueled by the belief that there is a conceptually tight connection between the syntax of our language and its semantics, and the belief that there is a similarly tight connection between the semantics of our language and metaphysical facts about the world. We are less confident than our colleagues about the relation syntax has to semantics and metaphysics. In particular, we do not think that the current status of theoretical syntax (or semantics or metaphysics) provides much support for either of the above two beliefs. We will illustrate our view with a case study regarding the status of complex demonstratives. We will show that a recent and particularly subtle syntactically based argument for the semantic/metaphysical status of complex demonstratives does not in fact show what semantic category complex demonstratives are in. Since the devil always lies in the details, we cannot extract a general method for undermining any argument that is similar in spirit. However, our case study will act as a cautionary note against any theory that attempts to derive important philosophical consequences from the shapes of sentences.⁶

JL express similar sentiments in the concluding section of their paper:

Until more is known about syntax, semantics and metaphysics, it does not appear that one can read significant semantic or metaphysical conclusions off the syntax of complex demonstratives. We posed our worry about arguments with syntactic premises and semantic conclusions by showing that current syntactic theory renders it possible to hold a variety of positions regarding semantics. This worry about what one might call the syntactic underdetermination of semantics, we think, will find purchase in many areas of research besides just complex demonstratives...Finally, we hope our study of complex demonstratives has helped to explain our skepticism about the supposedly conceptually tight connections between syntax and semantics, and between semantics and metaphysics. Our skepticism issues from the fact that language is always more complicated than it appears at first blush. In fact, we suspect that the relation between syntax and semantics is much more complex than is commonly thought. That is, we doubt that it is accidental that the current state of syntactic research fails to support a thesis about the semantic and metaphysical nature of complex demonstratives. We suspect (although of course we can offer no proof) that when all the syntactic facts are in, syntax still won't answer such questions of semantics and metaphysics.⁷

⁶ p. 17. See next note.

⁷ p. 34-35. I find the mention in both passages of a conceptual connection between syntax and semantics puzzling. I'm not sure exactly what is meant by 'conceptual connection' here, but on any reasonable resolution of what is meant, I doubt that anyone thinks there is a conceptually tight connection between syntax and semantics. I am also unsure what to make of JL's talk in both passages of syntactic arguments or support for claims about the "metaphysical status" or "metaphysical nature" of complex demonstratives.

In both passages, JL express a strong skepticism about syntactic facts providing evidence for semantic claims, that is, about SST; and they clearly think that their “case study” provides the reason for this skepticism.^{8,9} Their idea seems to be that since in their “case study” they allegedly show how one argument from syntactical facts to a semantic claim fails, this should cast doubt on or make us skeptical of the claim that any such argument succeeds.

The first point to make against JL is that it is hard to see why they think their case study should so much as suggest a general skepticism about SST. They explicitly acknowledge in the first quote above that from their case study they “cannot extract a general method for undermining any argument that is similar in spirit” (presumably, “similar in spirit” in the sense of using syntactic facts as evidence for semantic claims).

I wasn't aware that anyone has claimed to draw conclusions about the metaphysical nature of complex demonstratives or any other linguistic expressions (presumably, expression types) from syntactical facts about the expressions. Of course, the question of the metaphysical nature of linguistic expressions is an interesting one. But it is very hard to see why anyone would think that syntactic facts would bear on the question. Since JL are responding to arguments I gave and I didn't claim to draw any metaphysical conclusions from syntax (or semantics), I'll set these remarks about metaphysics aside.

⁸ JL's explicit skepticism about SST may seem hard to reconcile with the fact that they claim that “An important test for the indefiniteness of an expression is whether the phrase can appear in so-called “there'-insertion” [sic] contexts.” (p. 32)

JL make clear that this is a test for the expression having “an indefinite sense”, which presumably is a semantic matter. On the other hand, they do not make clear whether they regard the fact that the phrase “can (or can't) appear” in ‘there’ insertion contexts as a syntactic fact. If they do so regard it (and they certainly don't rule this out), then they themselves are committed to the claim that the syntactic fact that an expression “can appear” in ‘there’ insertion contexts (i.e. the fact that such sentences are grammatical) provides evidence for the semantic claim that it has an “indefinite sense”. But this latter claim entails SST! Hence, to avoid commitment to SST, JL must hold that the fact that a phrase “can (or can't) appear” in ‘there’ insertion contexts is not a syntactic fact (and hence that sentences like ‘There is every man’ are not ungrammatical but have some other failing). It is surprising that JL did not note that they are committed to this latter view, since it is quite controversial and is denied by many who write on indefinites (e.g. Kratzer 1988, Chierchia 1995, and Wilkinson 1995).

⁹ JL make clear that they take their case study to in some sense underwrite their skepticism about SST at the end of the first quoted passage when they write: “However, our case study will act as a cautionary note against any theory that attempts to derive important philosophical consequences from the shapes of sentences.” (The preceding remarks make clear that “important philosophical consequences” include semantic claims.) That they take their case study to in some sense cast doubt on SST is also made clear in the second quoted passage when they write: “Finally, we hope our study of complex demonstratives has helped to explain our skepticism about the supposedly conceptually tight connections between syntax and semantics...”. Further, JL give no reason other than their case study for skepticism about SST.

But if this is so, how can the (alleged) failure of one argument from syntactic facts to semantic claims even as much as suggest that all such arguments fail, so that SST is false? In other words, if there is no reason to think that all such arguments fail in similar ways, and JL explicitly admit this, then it is hard to see how the (alleged) failure of a single such argument casts doubt on SST. Absent other evidence for or against SST, JL's case study should leave one agnostic about it. This consideration by itself, I believe, shows that JL give us no reason to doubt SST.

But let's waive this worry. Without agreeing with JL here for the reason just mentioned, let's simply grant for the sake of argument that if JL are correct about their case study, some skepticism about SST is appropriate. I want to argue that even given this, JL have provided no reason for skepticism about SST. Here, in outline, is how I shall show this. JL's case study consists of purportedly showing that the argument that I gave in CD in which syntactic evidence is given for a semantic claim fails. As we have seen, this in turn is thought by JL to in some sense warrant skepticism about SST.¹⁰ Thus, if JL fail to undermine my argument, they have provided no reason for any such skepticism. I will show that this is in fact the case. I do this by first showing that the argument they attribute to me and successfully undermine is not the argument I gave in CD. Second, I show that none of the points they use to undermine the argument they attribute to me undermines the argument I actually give.¹¹

¹⁰ Again, while not agreeing that this is correct, we assume it is for the sake of argument.

¹¹ I assume here that it will be granted that undermining an argument that if sound would show SST true, where no one endorsing SST has given the argument in question, doesn't cast any doubt on SST. If it did, it would be too easy to cast doubt on a hypothesis by undermining bad arguments that no one has ever given for the hypothesis.

With this in mind, let's turn to JL's case study.¹² In CD, I noted that in two types of constructions, quantifiers and referring expressions behave differently. And in those constructions, complex demonstratives behave like quantifiers. This, I claimed, is evidence that complex demonstratives are quantifiers. The details concerning these

¹² There is preliminary misunderstanding on the part of JL that needs to be cleared up. They include among what they call "King's Grammatical Evidence" considerations concerning Bach-Peters sentences. However, in the book I explicitly distinguished what I variously called syntactical arguments or syntactical evidence from the considerations involving Bach-Peters sentences, (see Introduction p. xii; and note that I introduce "reasons for thinking that 'that' phrases are quantificational, which have to do with their syntactical behavior." (p. 16) only after discussing Bach-Peters sentences). These latter considerations are not simply syntactical, and so I didn't want to include them with the syntactical evidence. Further, JL did not grasp the argument I gave based on Bach-Peters sentences, (which is probably why they took the argument to involve only syntactical considerations). JL give an example of a standard Bach-Peters sentence involving standard quantifiers (I employ JL's numbering of examples here):

(10) Every friend of yours who studied for it passed some math exam she was dreading.

They then correctly say that I note that such sentences can be formed using complex demonstratives:

(11) That friend of yours who studied for it passed that exam she was dreading.

They then write:

So, complex demonstratives are just like quantifiers in that their nominal material allows for a delicate interaction (quantifying in) with other elements in the sentence. Since proper names lack nominal material, there is simply nothing for them that corresponds to a Bach-Peters construction. Here then is a third case where complex demonstratives behave like quantifiers and unlike proper names. (p. 21)

Apparently, they interpret me as arguing that simply because one can produce Bach-Peters sentences using complex demonstratives, as one can with other quantifiers, complex demonstratives behave here like quantifiers, and so this constitutes evidence that they are quantifiers. Now that would be a purely syntactic argument, since it would simply hinge on (11) being grammatical. But that is not the argument I gave. Here is the argument I gave (see CD pps.12-15). I considered the pair of sentences (11) and (11a):

(11a) Every friend of your who studied for it passes that exam she was dreading.

I noted first that a Kaplan style direct reference account would have trouble with each of these sentences. (11) is a problem, because to determine the character of either of the 'that' phrases requires securing a referent for the pronoun in each phrase. But securing a referent for the pronoun in one 'that' phrase requires having secured a referent, and hence a character, for the other 'that' phrase. Thus, we get caught up in a vicious circle trying to fix the character of either 'that' phrase. I then noted that even if the direct reference theorist could somehow get around that problem, it would be no help in handling (11a). For (11a) has a reading on which it asserts that every friend passed that exam she was dreading, possibly different exams for different friends. But since that 'that' phrase on this reading doesn't refer, much less directly refer, to any particular individual, the direct reference theorist cannot hope to handle (11a) on the reading in question. The upshot is that each of (11) and (11a) taken separately is problematic for the direct reference theorist. And she cannot hope to give a unified account of them. By contrast, if 'that' phrases are quantificational, they can be subsumed under existing semantic accounts of "standard" Bach-Peters sentences such as (10). Thus, the quantificational theorist can give a unified account of (11) and (11a),

constructions are unimportant; so let me simply display the data. First, antecedent contained deletion (ACD):

ACD:

Quantifiers

(5a) Tiger birdied every/some/no hole that Michael did.

Referring Expressions

*(5b) Copp thanked Holmes/him, who Jubien did.

Complex Demonstratives

(5c) Tiger birdied that hole that Michael did.

Here the examples involving quantifiers are grammatical and the nearest examples involving referring expressions are not grammatical. Since the example involving a complex demonstrative is grammatical just like the examples involving quantifiers are, this constitutes evidence that complex demonstratives are quantifiers.

Similarly, consider the weak crossover data:

Weak Crossover

Quantifiers

(6a) His mother loves every/some/no man with a goatee.

Referring Expressions

(6b) His mother loves John/him

Complex Demonstratives

(6c) His mother loves that man with a goatee.

and place them among the broader array of data comprised by standard Bach-Peters sentences like (10). Surely, I claimed, this is the more theoretically satisfying account of the data (10)-(11a). Clearly, contra JL, this argument is not simply a matter of noting the grammaticality of (11) and taking that as evidence that complex demonstratives are quantificational. Thus, contrary to what they say, it is not a purely syntactic argument. Hence I omit consideration of this argument here, since it is not relevant to SST.

In (6a), the pronoun ‘his’ cannot be understood as anaphoric on the quantifier phrase. But in (6b), the pronoun can be understood as anaphoric on the referring expressions (or at any rate, it can be understood as co-referring with it). Finally, I claim that in (6c), it is very difficult to understand the pronoun as anaphoric on the complex demonstrative. So here again, complex demonstratives behave like quantifiers and unlike referring expressions. And again, I claim this constitutes evidence that complex demonstratives are quantifiers. Let me note that JL do not challenge my claims about the weak crossover and ACD data here. So for purposes of the present work and my disagreement with JL, I ask the reader to assume I am correct about the data.

Now consider the argument based on this data that JL attribute to me (“...we think he would appeal to the plausibility argument we turn to presently.”p. 22):

Argument A

- (i) There is a syntactic class, the SYNs, which contains complex demonstratives and lots of other expressions: ‘every F’, ‘most Fs’, ‘some Fs’, ‘no F’, etc.
- (ii) There is a semantic class of expressions, the QUANTs, that lots of SYNs are in (e.g., ‘every F’, ‘most Fs’, ‘some Fs’, ‘no F’).
- (iii) Thus: Since complex demonstratives are SYNs too, the default hypothesis is that they are also QUANTs.

As they make clear later in their paper, JL take the SYNs to be the class of determiner phrases or expressions that occur in determiner position (henceforth DPs). Thus, they have me arguing from the claim that complex demonstratives are DPs, as are quantifiers (i and ii in Argument A), to the claim that complex demonstratives are quantifiers.¹³

As I’ve suggested, I do not argue in this way. But let’s see how JL undermine this argument. It is really very simple. They argue that many referring expressions,

¹³ It is curious that JL take me to be arguing in this way, since I never claim in CD that complex demonstratives are DP’s, let alone argue from this claim to the claim that they are quantificational. I do in fact take complex demonstratives to be DP’s but I don’t claim that this is evidence that they are quantificational. This is why I didn’t bother to claim that they are DP’s in CD.

including pronouns and perhaps even proper names, are DPs/SYNs. Thus, the fact that something is a DP/SYN does not support the claim that it is a quantifier. As JL put it:

“...it is plausible that many other expressions besides demonstratives (and quantifier words) are found in the determiner position. If this is so, merely being in the determiner position does not secure the quantificational status of a word.” (p. 27—see also p. 29).

I am actually not convinced by the arguments JL give, which lean heavily on Longobardi (1994), that pronouns and names are DPs. But it really doesn't matter for the main point I want to make. So for the sake of argument, I am prepared to go along with this here.

Now as I said, I don't give anything like argument A in my book. Here is the argument I do give:

Argument A+

(i) Quantifiers exhibit a certain characteristic behavior in ACD constructions and weak cross over constructions.

(ii) Referring expressions do not exhibit this behavior in those constructions.

(iii) Complex demonstratives behave like quantifiers and unlike referring expressions in these constructions.

(iv) Given that complex demonstratives are either referring expression or quantifiers, (i)-(iii) constitute evidence that complex demonstratives are quantifiers and not referring expressions.

(v) Thus, we have some evidence that complex demonstratives are quantifiers, and not referring expressions.

As stated, A+ is deductively valid. Assuming for the moment that (i)-(iii) are true, let's ask whether premise (iv) is true. Do (i)-(iii) provide evidence for or support the view that complex demonstratives are quantifiers? Surely they do! If we are wondering whether

an expression is of semantic type A or B, and we note that all uncontroversial examples of expressions of type A behave one way in certain constructions and all uncontroversial examples of expressions of type B fail to behave that way, surely the fact that the expression we are wondering about behaves in the type A way in the constructions in question is at least some evidence that it is type A and not type B.

Now let's ask whether the considerations that JL use to undermine Argument A undermine argument A+. Does the claim that many expressions other than quantifiers and complex demonstratives are DPs undermine Argument A+ in some way? Since A+ is valid, it would have to do so by showing that one of the premises is false. Does it show that at least one of (i)-(iv) is false? Clearly it does not. Again, for the sake of argument, let's just agree with JL that names, pronouns, indexicals etc. are all DPs (though as I've said, I am not convinced of this). This doesn't in any way suggest that any of (i)-(iv) in Argument A+ is false. The claim that names, pronouns, quantifiers, etc. are all DPs taken together with (i)-(iv), entail that different members of the class of DPs behave differently in ACD and weak crossover constructions. Quantifiers behave one way, referring expressions behave another way. And complex demonstratives behave like quantifiers. Surely this is still evidence that complex demonstratives are quantifiers, and so premise (iv) is unaffected. So agreeing with JL that many referring expressions are DPs has no effect at all on the question of whether (i)-(iv) are true. And of course (v) follows from (i)-(iv). So the considerations raised by JL do not in any way undermine my argument A+.

Having said this, let me hasten to add that one might well challenge the premises of argument A+, or at any rate premises (i)-(iii). The data I would marshal to defend these premises is certainly less neat than I would like it to be. Of course, in the end I do

think it supports these premises. But in any case, as I've already mentioned, it is no part of the attack JL make on the arguments in CD to challenge these premises.

This leads me to a final point concerning JL's discussion. First, obviously, I don't think that it deductively follows from (i)-(iv) in A+ that complex demonstratives are quantifiers. What follows is only the weaker claim that we have some evidence that complex demonstratives are quantifiers. Nor do I think that this data by itself in some looser sense "shows" that complex demonstratives are quantifiers. I tried to be careful in the book to talk about the facts recorded in (i)-(iii) as providing some evidence for or supporting the view that complex demonstratives are quantifiers, (see pps. 18, 19, 27 of CD). I emphasize this point because JL claim that they will show that the syntactic data "is compatible with a referential treatment of complex demonstratives" (p. 19, my emphasis); and they ask "whether syntax demands that we assign complex demonstratives" quantificational meanings (p. 19, my emphasis); and they say that they disagree with me "that the evidence shows that complex demonstratives are quantifier phrases" (p. 19-20, my emphasis). By saying these things, they suggest that I claim that the syntactic arguments by themselves in some sense show that complex demonstratives are quantifiers. But I claim no such thing. Indeed, the point of my book, and the point I wish to stress now, is that if one looks over a large range of data, the overall behavior of complex demonstratives across that data is more like that of quantifiers than of referring expressions. The syntactic arguments are one small, but not insignificant, piece of that data. No individual bit of data is telling by itself, but when you put all the data together the picture of complex demonstratives as quantificational emerges. In this sense, the argument of CD was intended to be rather like a strong case a prosecuting attorney presents to establish the guilt of someone on trial. Various bits of evidence are presented,

which taken together establish the guilt of the person being tried. Seen this way, and putting aside the fact that JL misunderstand what my syntactic arguments are, JL's claim that the syntactic data doesn't show that complex demonstratives are quantificational and is compatible with a referential treatment of them is just like a defense attorney objecting that the fact that the defendant's finger prints are the only ones on the murder weapon doesn't show that the defendant is guilty and is compatible with his innocence. The prosecuting attorney should of course agree and continue to lay out the rest of her case.

Returning to the main theme, we have seen that JL's case study fails to undermine my argument A+ that employs syntactic evidence in support of a semantic claim. As indicated above, this means that they have failed to provide any reason for skepticism about SST. Further, it seems clear that if A+ is sound, SST is true. Abstracting from particulars, here is the form of that A+:

Argument Form FA+

- (i) Expressions of semantic type A exhibit syntactic behavior R.
- (ii) Expressions of semantic type B do not exhibit R.
- (iii) Expressions of syntactic type S exhibit R.
- (iv) Given that expressions of syntactic type S are either of semantic type A or semantic type B, (i)-(iii) constitute evidence that expressions of syntactic type S are of semantic type A.
- (v) Thus, we have some evidence that expressions of syntactic type S are of semantic type A.

Of course, the "premise schemata" of FA+ are not "purely syntactic". They mention semantic types A and B. So it isn't as if FA+ pulls a semantic rabbit out of a syntactic hat. Still, even JL must agree that an argument that uses syntactic evidence for a

semantic claim, and so if sound shows SST to be true, may mention semantic types or categories in its premises. For the argument A they criticize in an attempt to undermine SST mentions the semantic class QUANTS in its second premise. In this respect, it is no different from argument A+. In any case, I think it is fair to say that anyone endorsing an instance of FA+ holds that syntactic considerations provide evidence for semantic claims and hence endorses SST. Since I endorse A+, whose premises are defended in CD, I take the answer to the question with which this paper began to be 'yes'.

In summary, though I haven't shown that SST is true here (since I have not here defended the premises of TI, NPI, SI or A+--of course I do defend the premises of the latter in CD), I have shown that JL provide no reason for skepticism about it. I have also made clear the kind of argument that one would have to defend to show SST to be true. By so doing, I hope to have convinced the reader that SST is at the very least a *prima facie* plausible hypothesis.¹⁴

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