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Discussion

Monotonicity and syllogistic inference: a reply to Newstead

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Newstead's critique is an exercise in carpet-bombing: he doesn't raise one big objection but a host of smaller ones. I hope I will be excused if I address only a handful.

Let me start with a brief synopsis. Experimental investigations of human reasoning invariably use linguistic tasks. Subjects are to say whether this or that sentence follows from a given set of sentences, or to draw their own conclusions, etc. Hence, human reasoning, as studied by Newstead and his fellow psychologists, is at least partly a matter of linguistic processing. In particular, since inferences are drawn on the basis of what the premisses *mean*, human reasoning, as studied by psychologists, is at least partly a matter of semantic interpretation. We don't know how big that part is, nor whether it can be neatly separated from "genuine" processes of inference (as is often assumed), but it is there.

As interpretation precedes inference, it is only to be expected that reasoning is affected more by salient semantic properties than by others. By "salient" I mean such properties that play a prominent part in the interpretation of language. My paper explored the possibility that reasoning with quantifiers is influenced by what are known to be the salient properties of quantifying expressions, and especially by their monotonicity properties. In order to flesh out this suggestion, I presented a simple inference system, which was embedded in an equally simple processing model, with a view to showing "that even a crude processing model can produce reasonable predictions" (Geurts, 2003, p. 243). My idea was that if a toy model does reasonably well, a more sophisticated model is likely to do better.

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A large part of Newstead's critique is devoted to arguing that my model is simplistic and that its predictions aren't perfect. However, there really was no need to argue for this, because I had already conceded as much. More interesting is the question whether the model's imperfections matter, and here Newstead and I appear to disagree, although the disagreement is more superficial than Newstead suggests.

A case in point is the axiom (Newstead calls it a "rule") that "all" entails "some", to which Newstead objects on two grounds. First, "this principle seems implausible since it breaches Gricean conversational implicatures: 'some' would not normally be used if 'all' were the case." Secondly, Newstead rejects my assumption that this "rule [read: 'axiom'] can be applied with no cognitive effort." The first objection cuts no ice, because if a conclusion is evidently weaker than its premisses, people are generally reluctant to *say* that it follows, even if they treat the inference as valid (cf. my remarks on "or"; Geurts, 2003, p. 248, n. 21). For example, most people will accept that the first of the following arguments is valid, which implies that they accept the validity of the second argument, as well, even if they are reluctant to explicitly endorse it:

If anybody is sick, the meeting is cancelled.
 Everybody is sick.
 ∴ The meeting is cancelled.

Everybody is sick.
 ∴ Someone is sick.

As noted in my paper (Geurts, 2003, p. 226, n. 2), the inference from "all" to "some" is not an ordinary inference. It is a presupposition, hence part of the lexical content of "all", and a salient part at that. Which is to say that processing the word "all" will suffice to trigger it. This justifies the assumption that the "inference" becomes available at negligible cost. I admit that modelling this presupposition as an axiom was a makeshift, though in the context of my paper it was an innocuous simplification. See Geurts (1999) for a treatment that is rather less naive.

Although Newstead lavishes attention on secondary features of my theory, he also objects to its main tenet, which is that monotonicity plays a role in human reasoning. To begin with, Newstead suggests that on my account monotonicity inferences are complex. This suggestion is misleading. First, my theory is, if anything, simpler than its competitors. This becomes especially clear when my monotonicity rule is compared with its counterpart in Ford's (1995) theory (which Newstead seems to approve of), which is less general and much more complex. Secondly, Newstead's objection blithely ignores a crucial argument in my paper, namely, that there is weighty independent evidence for the claim that people routinely and effortlessly use monotonicity properties (of any type of expression). In my paper I mustered various sorts of linguistic evidence for this claim, but there is experimental evidence as well. For example, Crain and his colleagues have demonstrated that children as young as

3;11 know that the first of the following arguments is valid whereas the second one is not (e.g. [Boster & Crain, 1993](#)):

Every troll who ordered French-fries or onion rings got some mustard.
 ∴ Every troll who ordered French-fries got some mustard.

Every ghostbuster will choose a cat or a pig.
 ∴ Every ghostbuster will choose pig.

Apparently, even very young children are sensitive to the monotonicity properties of the universal quantifier, which entail that “or” licenses a conjunctive inference if it occurs in the first argument of “every” but not if it occurs in the second argument. As always, when taken on its own this fact doesn’t prove much, but in conjunction with other findings amassed by linguists, philosophers, and psychologists, it is compelling evidence for the claim that monotonicity is a fundamental concept in interpretation as well as inference.

It is important to note that, amidst a barrage of objections, Newstead actually concedes that there is evidence that people use monotonicity inferences like the ones I propose. This means that he and I agree on what I take to be the main point. For even if he argues that my theory is flawed in all sorts of ways, we agree that one of the strategies for syllogistic reasoning involves replacing set-denoting expressions. But then Newstead can hardly fail to support the main objective of my paper, which is to show that this strategy follows naturally from general principles emerging from semantics, language acquisition, and so on.

Based on protocol studies by [Ford \(1995\)](#) and [Bacon, Handley, and Newstead \(2003\)](#), Newstead claims that there is a “basic distinction between verbal and spatial reasoners”, from which he concludes that my theory can at best explain the behaviour of every second reasoner. Although I fully agree that there are likely to be different modes of reasoning, and I would be happy to have accounted for any one of them, I don’t think this is correct. What Ford and Bacon et al. have shown is that if subjects are given pencil and paper for solving syllogistic problems, some of them will draw pictures in the process. But this doesn’t even begin to show that underlying this behaviour there is a special mode of reasoning. Indeed, granted the existence of a substitution strategy for solving syllogistic problems, there is no principled difference between manipulating set-denoting terms and set-denoting diagrams, like Euler circles, for example. Hence, Ford and Bacon et al.’s results fail to establish that there is a distinctive spatial style of syllogistic reasoning.

Newstead directs several objections against my discussion of “at most”. To begin with, he doubts my finding that arguments with “at most” are sometimes more complex than corresponding arguments with “at least” or “some”. Newstead considers this a dubious result because he didn’t obtain anything like it in a study of his own, which isn’t surprising because already in my own experiment the effect showed up only in two-premiss arguments, not in single-premiss arguments. Apparently, “at most” can make a comparatively difficult task more difficult, but has no measurable effect on easy tasks. Furthermore, the data I reported have in the meantime been confirmed by a new experiment (not yet published). So I cannot agree with Newstead on this point. On the other hand, I concur with his criticism of the tentative explanation I suggested; but I think I have a better story now. If a sentence contains several quantifiers, their monotonicity

properties may or may not mesh. For example, “some” has the same monotonicity properties as “more than three”, while “less than three” is different, so the combination “some/more than three” is more harmonic than the combination “some/less than three”. The experiment referred to above provided evidence that harmony, in the sense just explained, is a measure of complexity. For example, the first of the following arguments (both of which are valid) proves to be much easier than the second (96% vs. 51% correct):

At least 3 reporters played against more than 2 foresters.

All foresters were communists.

∴ At least 3 reporters played against more than 2 communists.

At least 3 reporters played against less than 2 foresters.

All communists were foresters.

∴ At least 3 reporters played against less than 2 communists.

What I would like to suggest is that quantifiers like “at most three” and “less than three” are *themselves* not perfectly harmonic: they are downward entailing but contain an expression that is upward entailing, namely “three”. It is for this reason that they are inherently more difficult than the other quantifiers. If correct, this analysis shows, in yet another way, how the concept of monotonicity helps to explain why some arguments are more difficult than others.

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