

★ FORUM ★

Where is the Conflict between Internalism and Externalism? A Reply to Lohndal & Narita (2009)

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In a recent issue of *Biolinguistics*, Lohndal & Narita (2009, henceforth L&N) reply skeptically to my attempt to sketch a theory that takes both internalist and externalist inquiry seriously (Lassiter 2008). My reply serves two purposes. First, L&N seriously misrepresent my work, and a certain amount of correction is needed. In particular, they seem to be laboring under the misconception that my proposal was intended as an argument *against* internalism, rather than an argument for a science of language that makes use of both internalist and externalist modes of inquiry and attempts to relate them systematically. A more important issue, however, is the evaluation of L&N's positive claims. I argue that their highly restrictive vision of the methodology of linguistics is dubious in light of the current landscape of the field, and potentially harmful to the development of a proper biolinguistics. I identify three separate notions of internalism that are lurking in L&N and argue that two of them are harmless but relatively uninteresting, while the third is both unmotivated and dangerously parochial. I conclude with some speculations about the place of 'externalist' inquiry in the emerging interdisciplinary field of biolinguistics.

1. Corrections

Let me begin by emphasizing that, although I focus here on weaknesses in their account, I do think L&N make a real contribution to the discussion. In particular, they argue that my characterization of internalism is not faithful to Chomsky, and they offer an alternative characterization. This may well be right, and I will return to the characterization of internalism in section 2. Further, they state a widely held, but rarely articulated, set of assumptions with remarkable clarity, and this makes the task of evaluating these assumptions easier. With these positive aspects in mind, I will proceed here to what I see as flaws in their portrayal of my own position, and then to flaws in their own.

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There are numerous inaccuracies in L&N's rendition of Lassiter (2008), indeed more than I can respond to in this space. I can only ask the reader, rather than judging my contribution from L&N's selective and distorted summary, to refer to the original. However, two important points of clarification are in order, without which it will be impossible to understand the following discussion. First, L&N claim repeatedly that my paper advocates "Dummett-type externalism"—a serious charge in some quarters. In fact I expended considerable energy to refute precisely this type of externalism, using Dummett as the prototype of an externalist whose theory is unworkable (Lassiter 2008: 611-617). The type of 'externalism' that I argued for in the remainder of the essay has little in common with Dummett except that it takes seriously the *intuitions* on which he bases his theory, rather than dismissing them out of hand. My theory was no more 'Dummett-type' than any theory whose practitioners make use of grammaticality intuitions is 'Chomsky-type'.

Secondly, L&N take my paper as a "criticism of the internalist project" and as an attempt to "suggest an alternative to the internalist science of language" (L&N: 330). This was not even remotely my intention, nor is this reading supported by my words (except under a certain extremely narrow definition of 'internalism', cf. section 2.3 below). It is true that my discussion did not *pre-suppose* internalism: This would have been problematic for my goal of convincing externalist-inclined philosophers that they can coexist with internalists. But I did not argue against internalism—far from it, my theory depended crucially on the fact that speakers possess internal linguistic representations that play a causal role in their linguistic productions, as I repeatedly emphasized.

The thrust of the paper was that the obvious difficulties in accommodating semantic externalism within linguistics—for example, problems about dialect continua identified by Chomsky (1986, 2000), and the usual dismissal of mentalism by externalists—can be resolved by careful attention to social and sociolinguistic details. The negative claim (not original to me) was that there are linguistic facts which cannot be explained solely by reference to the internal states of individuals; the main positive claim was that these same facts *can* be explained by reference to individuals' internal states and the way that individuals interact with each other. This is obviously not the same as saying that there are no internalistic facts about language, or that internalist inquiry is not an important, indeed crucial, aspect of the study of human language. My paper claimed that semantic externalism and internalism are, contrary to appearances, compatible and in fact complementary. This is not an argument *against* internalism under any reasonable construal of this term.

L&N's critique consists of at least three strands. One is a rather general defense of internalist inquiry which, since I am not an opponent of this methodology, misses the mark entirely. The second strand is a handful of specific criticisms of the proposal in Lassiter (2008), which I will not answer in detail because they are either beside the point (e.g., criticizing me for not defining 'accommodation' when that term is clearly defined and discussed in detail in references cited) or they do not seriously engage the details of the proposal under evaluation and its philosophical back-

ground, so that a reply would be little more than a rehash.

The third and most important type of criticism L&N raise is a generic indictment of externalist inquiry. Their argument here is interesting, to my mind, even though they do not even attempt to engage the large philosophical literature on semantic externalism that motivates Lassiter (2008), nor to discuss the specific questions work that that paper responds to (e.g., issues in the semantics of names and definite descriptions, or detailed questions about how semantic-externalist intuitions interact with specific patterns of social interaction). Instead L&N focus on the question of whether externalist inquiry in general (N.B.: not just *semantic externalism*, the main concern of Lassiter 2008) is worth pursuing.¹ This is a rather blunt-edged argument, so it is not clear why they chose my paper, of all the huge body of relevant work in philosophy, linguistics, psychology, anthropology, etc., as a target for this criticism. However, their discussion raises a number of interesting questions which are worth responding to on their own merit. This is the issue which was hinted at in the previous paragraph, and which forms the core of L&N's paper: What is a reasonable construal of internalism, and in what sense is it in conflict with externalist inquiry?

2. Three Types of Internalism in L&N

2.1. *Internalism by Definition*

The first kind of internalism lurking in L&N's discussion is internalism by definition: We simply define 'language' as 'internalist aspects of language', thus deriving the conclusion that externalist aspects are not 'language'. This idea seems to be present, for example, in the following quote:

Lassiter claims that we can overcome such difficulties [...] by incorporating some sociolinguistic notions into linguistic theory. (L&N: 321)

Many linguists would have thought that sociolinguistic notions are already present in linguistic theory. (As it happens, sociolinguists do not appreciate being told that what they do is not 'linguistics' or that it is not 'theory'.) But the terminological question is not very interesting to my mind: If someone wants to define the social aspect of language use as part of the study of social psychology, for instance, nothing particularly interesting hinges on this choice.

¹ Note that here and throughout I am using the unmodified term 'externalism' in the very broad sense that L&N do, as a description of inquiry into language use in any fashion. This is *not* what philosophers usually mean by 'externalism', and it is not how the term was used in Lassiter (2008). The latter notion—which I will call 'semantic externalism'—picks out a somewhat heterogeneous class of claims about the relationship between the meanings of words as an individual speaker uses them, and the usage of others with whom she is in contact. (This is intentionally vague, since there are many conflicting implementations of this idea which have little else in common.) Semantic externalism characterizes only a small part of the wide range of inquiry that counts as 'externalist' in L&N's sense.

2.2. *Internalism as Methodology*

I take it that L&N consider this to be their most important argument for internalism, since it contributes the title of their paper. This is puzzling: If the choice to ignore externalist aspects of language is simply a methodological one, then there should be no difficulty in allowing that other researchers, choosing a different methodology, may go about their business, and perhaps even uncover results that complement one's own. Two variants of this position appear in L&N. First:

Chomskyan internalism [...] just amounts to “the *methodological decision* [...] to study *less*, prior to studying *more*”. (L&N: 329, citing Hinzen 2006: 161)

Can this really be all that internalism amounts to? If so, it is harmless but theoretically impotent. You decide to study less, hoping to get deeper; I decide to study more, hoping to get a broader view. Eventually, we may converge. In any case, it makes no sense to argue about which methodology is right. For example, you might study cellular biology without taking an interest in the evolutionary history of the relevant organism. I might study evolution, either in isolation or along with cellular biology. It would be strange indeed for one of us to argue that the other has chosen the wrong methodology.

On the other hand, “internalism as methodology” might mean this:

We need a more complete understanding of the internal properties of I-language before we can even attempt to try to understand how individuals utilize them to deal with all sorts of E-language phenomena. (L&N: 328)

Perhaps. But the claim that L&N are trying to make needs more justification than their plea that their own field (syntax, I take it) is not sufficiently well understood. First, a good deal has been learned about language use in sociolinguistics and pragmatics that is independent of the specific grammatical theory employed, undercutting the claim that a complete understanding of I-language must come first. But, more to the point, it's simply not true in general that explanatorily more basic areas must be fully understood before profitable research in higher-level fields can be undertaken. Darwin did a great deal of extremely important work on evolutionary theory without having any idea what the mechanisms of descent with modification were. Likewise, no one would seriously argue that all research in ecology should be put on hold until every detail of the structure and function of mitochondria is understood. As far as I can see, neither of L&N's arguments for “internalism as methodology” convey any more than their personal preference for investigating facts that can be given an account in strictly internalist terms. This is all well and good, but hardly justifies the dismissive attitude toward externalist inquiry that the authors display. Something more must be at play.

2.3. *“A Truly Scientific Explanation”*

In the end, L&N make only one argument that is genuinely in conflict with the

approach outlined in Lassiter (2008): The argument that externalist inquiry, in general, cannot be given a scientific basis. Since Lassiter (2008) is an example of externalist inquiry, it follows that Lassiter (2008) is not scientific. For instance:

What internalists doubt is rather the feasibility of providing a serious science of any mind-external phenomena such as [normativity and communicative success].
(L&N: 326)

[G]eneral issues of intentionality, including those of language use, cannot reasonably be assumed to fall within naturalistic inquiry.
(Chomsky 1995: 27, cited by L&N: 326)

[I]nternalists never deny that there are complicated social aspects in the domain of language use; they just decide not to let these *unexplainable* aspects of language use enter into their naturalistic theory at the present stage of inquiry.
(L&N: 330; emphasis added)

These are sweeping claims. If they are correct, a great number of people who take themselves to be doing research on human language and related topics have, in reality, been wasting their time. One would expect that such claims would be accompanied, say, by a thorough discussion of the large body of existing work on language use (see below) and an explanation of this dismissal making use of some clear-cut criteria for what counts as being ‘scientific’. Instead, L&N make this assertion with virtually no argumentation except for a few references to Chomsky’s philosophical works (Chomsky 1995, 2000), and a footnote quoting a definition of “serious science” from McGilvray (2002) with no indication of why it should be accepted, or its relevance to the specific issues at hand.

Frankly, I have a hard time seeing how this argument can be taken seriously. I know of no other discipline where scholars can dismiss each others’ work on purely aprioristic grounds and others will accept this move as ‘scientific’. Other examples of this habit are Chomsky’s (2000) insistence that a theory of language use is impossible because it would have to be a “theory of everything”—never mind that numerous well-developed theoretical approaches to language use were already in existence when this pronouncement was made—or Narita’s (forthcoming) claim that externalist inquiry is invalid because it disagrees with Descartes’ notion of free will. To put it simply: In a “serious science” you cannot dismiss others’ work because it disagrees with your philosophical preconceptions—or at least, you cannot do this and expect anyone to listen. Criticism of others’ work will consist in a demonstration of some actual problem with it—for example, that it makes incorrect predictions about some domain—and not simply the fact that one does not feel comfortable with its assumptions. (Of course anyone can feel however they want about anyone else’s work; they just can’t expect other scholars to take this seriously as an argument.)

Further, L&N’s argument—particularly in the third quote cited above—is uncannily similar to what Dawkins (1986) describes as the “argument from personal incredulity”. As Dawkins observes:

Even if the foremost authority in the world can't explain some remarkable biological phenomenon, this doesn't mean that it is inexplicable. Plenty of mysteries have lasted for centuries and finally yielded to explanation. (Dawkins 1986: 39)

L&N would do well, I think, to be more cautious about what they describe as "unexplainable". In particular, when evaluating a specific proposal (such as Lassiter 2008, or any of the references cited below), it is not enough to declare that they find the phenomena in question mysterious, or that they have already designated that domain as "unexplainable" or "unscientific" and that the proposal is thus wrong. This is bad argumentation, and totally unconvincing; it also undermines the pretensions of being "scientific" that the authors claim to hold dear.

Even worse, there already exists an enormous body of work investigating and theorizing about language use and closely related topics. I do not know whether L&N are unaware of this work, or if they have some reason to dismiss it; but it is irresponsible to make pronouncements like those quoted above without even considering it. I have in mind:

- Gricean and Relevance-Theoretic pragmatics (Sperber & Wilson 1986, Grice 1989, Horn 1989, and much other work)
- decision-theoretic and game-theoretic pragmatics (e.g., Parikh 2001, van Rooij 2003, Benz *et al.* 2006)
- experimental, theoretical, and empirical sociolinguistic work demonstrating speakers' sensitivity to social factors in their language use (e.g. Payne 1980, Le Page & Tabouret-Keller 1985, Labov 1994, 2001, Eckert & Rickford 2001)
- social-psychological work on accommodation, joint action, and related topics (e.g., Giles & Powesland 1975, Giles & Robinson 1990, Clark 1992, 1996)
- work on coordination and social norms in philosophy and economics (Lewis 1969, Skyrms 1996, Bicchieri *et al.* 1997, Young 1998, Bicchieri 2006)
- computational simulations of complex societies, including linguistic communication (Epstein & Axtell 1996, Skyrms 2004, Epstein 2007)

This list could be much longer, but this should give a sense of how rich, varied, and theoretically sophisticated the study of language use already is and promises to become in the future.² (Sadly there is no room here for a detailed discussion of the results and prospects of this large body of research; but the works cited speak for

² Truth-conditional semantics has been the target of a certain amount of abuse from internalists, and perhaps it belongs here as well. I omit it because I do not think that it is necessarily an externalist enterprise. Nor, in my experience, do many of its practitioners, particularly the ones who work in linguistics departments. Notions such as truth in a model are essentially mathematical notions which do not care whether they are used for internalist or externalist purposes. But if I am wrong, and it is true that semantics is essentially externalist, so much the worse for L&N: They now have the burden of explaining for yet another field how so many researchers have discovered so many systematic facts and compelling explanations in an area which supposedly is not "serious science" or "naturalistic inquiry".

themselves.) It seems strange indeed, in this light, to hold to a speculative notion of what is or is not possible in the study of language use. Unless L&N are willing to dismiss all of this work as “unscientific”, they will have to reconsider their position. Even if they are willing to do this, I see no reason to take such a position seriously, unless they can demonstrate point by point that all of this research is misguided. By any reasonable definition, it seems to me, there are numerous approaches to the study of language use that have claim to the title of “serious science”.

3. The Place of Externalist Inquiry within Biolinguistics

When I began my research into issues about internalism and externalism a few years ago, I knew of externalism only through Chomsky’s writings, and I wanted to show once and for all that there is no place for semantic externalism in a science of language. As I read the classic works in the philosophical literature, however, I realized that semantic externalism was less a theory than a bundle of intuitions about how people would use words in certain counterfactual situations. Even though existing attempts to explain these intuitions were clearly unworkable, it did not follow that no such theory could be constructed, or that the general approach was useless. The realization that I came to is nicely summarized in an epitaph by Langer (1962: ix), quoted by W. Tecumseh Fitch in the same issue of *Biolinguistics* in which L&N’s paper appeared:

The chance that the key ideas of any professional scholar’s work are pure nonsense is small; much greater the chance that a devastating refutation is based on a superficial reading or even a distorted one, subconsciously twisted by a desire to refute.
(Fitch 2009: 286)

Rather than dismissing the ideas of Kripke, Putnam, Dummett, Burge, Lewis, and other professional scholars as stupid or useless, I decided to attempt to explain their insights in a way that was consistent with my understanding of language, as a linguist trained in both theoretical (Chomskyan) and socio-historical linguistics. Lassiter (2008) was the result. Whatever the value of that specific proposal, I simply do not accept that this project is a waste of time in principle, as L&N seem to believe. Judging by his inclusion of the above quote, at least one prominent biolinguist would agree.

Fitch quotes the above passage in a section where he discusses sociological barriers to progress in biolinguistics, in particular citing “terminology and differing conceptions of ‘language’” as impediments. As the discussion in section 2 suggests, I am of the opinion that L&N’s critique of my work, like a great deal of the internalism–externalism debate more generally, deals more with these sociological issues than with genuine theoretical problems. The only real point of contention that I can identify is L&N’s insistence (echoing Chomsky) that there can be no scientific theory of language use. I am unable to locate a substantive argument for this negative claim in L&N or in any of Chomsky’s work; and, if we were to accept it, this would mean

dismissing existing efforts to provide just such a theory—within pragmatics and sociolinguistics, as well as related work in psychology, anthropology, economics, political science, and other disciplines—as “pure nonsense”.

It is much better, I think, for internalists—really, I mean biolinguists, of whom I consider myself one—to try to integrate their theories with neighboring disciplines, and whenever possible to make use of other theories’ insights in our own work. Here again Fitch’s discussion of how to make progress in biolinguistics is relevant:

Theoretical discussions are often dominated by rhetorical battles and ideological or terminological debate rather than constructive attempts to make tangible progress. Much of the criticism that currently divides the relevant fields boils down to “My opponent says we should look to x for answers, but I believe we should look to y instead”. Typically, both x and y are probably important. Given the large number of open questions, biolinguistics will be better off when individual researchers pursue those topics and approaches they believe are important and promising, and refrain from attacking others who have different interests or try different approaches. There is little to be gained from such attacks, and if my experience is any guide, much to be lost. (Fitch 2009: 291)

I think that both internalist and externalist aspects of language are important, and that both will eventually admit of scientific explanation (though probably not in the same terms). Anyone who wants to disagree with either my interest or my optimism should of course feel free; but it makes no sense to deny others the opportunity to look for answers, simply because one personally does not think they will be found. In any case, the proof is in the pudding: As we have seen, many scholars have already developed sophisticated approaches to language use. I suspect that useful and predictive theories of language use will continue to develop, as they have already been developing for many years, uninhibited by the insistence of certain internalists that this simply cannot happen.

What would the place of externalist inquiry be within biolinguistics? An important tradition in sociolinguistics, associated in particular with the work of Dell Hymes (e.g., Hymes 1974), deals with the notion of ‘communicative competence’. Hymes suggests that the ability to use language in socially appropriate ways is part of individuals’ knowledge of language, broadly construed. A great deal of empirical and experimental study in sociolinguistics has confirmed that speakers have detailed knowledge, not just of grammatical features of language, but also of *how to use language appropriately in a social context*. Biolinguists who make use of the model of Hauser *et al.* (2002) need not dismiss this notion as “unscientific”—rather, communicative competence can be treated as part of the ‘faculty of language in the broad sense’ (FLB), that is, as one of various areas where non-linguistic cognitive skills interface with grammar narrowly construed. Likewise, pragmatics both in the tradition of Grice (1989) and the more recent attempts to provide game-theoretic foundations for pragmatics (Parikh 2001, Benz *et al.* 2006) treats language use as the interface between purely linguistic knowledge and domain-general social reasoning and decision-making abilities. (The latter, by the way, also provides a precise, testable,

and mathematically rigorous theory of language use, again contrary to the claim that no such theory is possible.)

I see no conflict between externalist lines of inquiry like these and internalism, beyond the exclusionary attitude of some internalists against those who pursue other approaches to language. Rather, they fit naturally within the framework proposed by Hauser *et al.* (2002) as aspects of FLB. My attempt in Lassiter (2008) to articulate a notion of ‘social meaning’ was an extension of these aspects of FLB: In particular, I focused on word meaning as it related both to grammar (the narrow faculty) and pragmatic and communicative competence as social skills. Communicative competence is an inherently social skill, and so it comes as no surprise that, if two language users with identical internal states were embedded in different social environments, one might be judged communicatively competent while the other is not. This is really all that Burge’s ‘arthritis’ story shows: Our intuitions about what constitutes appropriate language use does not depend exclusively on the internal states of language users, but also on who they are in communication with and whether their present internal states will allow for successful communication. Many philosophers, in particular, tend to include such facts in their notion of ‘meaning’, a practice which I followed in Lassiter (2008). This may not agree with some linguists’ use, but it is a mere terminological issue, and perfectly acceptable usage if flagged appropriately.

The notion of social meaning is simply not a challenge to internalist explanations of linguistic competence. The situation is the same as it would be, say, in decision theory: The claim that individual agents attempt to make the best possible choices given their information and preferences is not threatened by the fact that agents sometimes make mistakes due to bad information or processing constraints (see Gintis 2009 for discussion of the competence–performance distinction in this domain). The distinction between the mechanism and how the mechanism is put to use in concrete circumstances is not threatened by the existence of research attempting to connect the two. Further, my construal of notions like ‘normativity of meaning’, ‘deference’, etc. was an attempt to embed a theory of individual agents’ internal states within a social context and derive predictions. L&N and anyone else should, of course, feel free to disagree with or ignore this attempt; but it is fundamentally opposed to the spirit of scientific inquiry—not to mention bad for the future of the science to which this journal is dedicated to cultivating—for L&N to trivialize the effort or discourage further investigation simply because it is not what *they* do, or because they personally dislike such efforts.

More generally, linguists simply cannot afford to ignore issues of language use. Those who do will be deprived of the insights of highly relevant and increasingly intricate theories—from the realms of pragmatics, psychology, and computational modeling, to name a few—many of which are compatible with, or even presuppose, the biolinguistic thesis that language acquisition and structure are explained in large part by biological facts about humans. If biolinguists ignore or trivialize these efforts on the basis of *a priori* claims about what kinds of theories are possible, I fear that biolinguistics itself will come to be ignored and trivialized as

theories of the supposedly impossible type are actually developed. It would be better to give up this provincialism and acknowledge that there is no principled conflict between biolinguistics and theorizing about language use—in other words, that there is no irreconcilable conflict between internalism and externalism.

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