

This is an extended version of my response to the commentaries on my essay ‘The Illusion of Language Acquisition’ (*Linguistic Approaches to Bilingualism* 3.3). Section 4 is new; length restrictions prevented its inclusion in the published response.

Facts and Issues: Response to the Commentaries^{*}

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Thanks are due to the twenty scholars who commented on my essay. Each commentary merits a more detailed response than limitations on space permit. Out of necessity, I will restrict my remarks here to what I take to be the most urgent matters raised by the commentators; a more comprehensive response is available at my web site.

I will begin by focusing on questions relating to the particular type of processor that I envision, and then turn to issues involving scope and language acquisition. I conclude with a general remark about the relationship between grammar and processing.

1. The processor

My essay advanced two closely related theses. First, language exhibits the particular properties and options that it does because of the processing resources that are required to use it. Second, language emerges in the course of childhood in the way that it does because efficient processing requires the creation of routines that map form onto meaning and vice versa. Some of these routines (e.g., those involving basic word order) develop largely in response to experience, although the precise relationship between processing and the fabric of the input is yet to be fully understood, as Wulff notes. Other routines (e.g., those involving scope) appear to be shaped, at least initially, just by internal considerations relating to processing cost.

A number of commentators call for more information about the processor that I have in mind. As I have noted elsewhere (O’Grady 2005:210), my use of the term ‘processor’ embodies an abstraction. In reality, there is just *processing*—the activation of networks of neurons. Working memory too is a convenient abstraction, a cover term for the ability to maintain the activation of a neuronal network (ibid., 2005:6).

I do not take the processor (that is, processing) to be domain-general. Far from it, in fact. The network of neurons that is activated to execute the mapping between strings of words and semantic representations is surely different from the neuronal network that maps sound waves onto phonemes, and the network that maps concepts onto words, let alone the network that categorizes an image on the retina as an instance of a tree. What is domain-general, uncontroversially, is the existence of limitations on the capacity to maintain neuronal activation (‘working memory’). However, I take no position on whether these limits are comparable across different types of cognition; that issue is irrelevant to what I propose.

Although I focus in my essay on the role of working memory and processing efficiency in the creation of the routines that map strings of words onto semantic representations, I do not believe that processing is driven just by word order. As is

^{*} I am grateful to Kamil Deen, Kevin Gregg and Theres Grüter for their questions and suggestions.

- (4) a. Nobody ate all the cookies. (*not* > *all* interpretation allowed)
 b. A friend of nobody ate all the cookies. (*all* > *not* only)

Let us assume that items such as *nobody* and *nothing* negate a predicate of which they are an argument in roughly the way that *not* does, thereby creating the conditions for a *not* > *all* interpretation. Then, (4a) can have that interpretation, but not (4b), in which the verb's first argument is a friendless person, not *nobody*.

Processing-based accounts of grammatical phenomena raise a fundamental question: can the relative uniformity of a community's judgments of acceptability and interpretation be reconciled with the fact that individuals are known to differ in their working memory capacity? In this regard, both Omaki and De Cat draw attention to Sprouse et al.'s (2012) finding that differences in working memory appear not to correlate with judgments of the severity of island violations—contrary to what a processing-based approach would predict, they believe. However, this line of reasoning is incorrect, at least for the view that I hold.

With Hawkins (2004:192ff, 266), I take the position (O'Grady 2005:203ff, 214ff; 2012:497) that processing difficulty helps define a continuum of potential 'break points' beyond which language learners do not proceed without the pressure of experience. A helpful example comes from a simple typological contrast: English permits a filler-gap dependency to extend across a (tensed) clause boundary, whereas Russian does not.

- (5)a. English:
Who do you think [_S that Ivan invited]?

- b. Russian:
 ?***Kogo** ty dumaeš [_S čto Ivan priglasił]?

There is little doubt that processing factors are involved here, as it is well known both that filler-gap dependencies are taxing and that their maintenance across a clause boundary is especially difficult (O'Grady 2012:496 and the references cited there). The presence of a clause boundary thus provides the break point that prevents Russian children from producing patterns such as (5b) in the absence of exposure to them in the input.

There is no reason at all to think that Russian speakers should differ in their assessment of the unacceptability of (5b) in ways that correlate with their personal working memory capacity. What counts in the calculation of break points on my view (O'Grady 2005:206) is the *relative* cost of particular processing routines. The key prediction is simply that the processing routine required to carry a filler-gap dependency across a clause boundary makes greater demands on resources than the routine required to implement an intraclausal dependency (as in *Who did Ivan invite ?*). If this is right, then learners of English and Russian will avoid the more costly routine unless and until its need is established by experience. Ten dollars is twice as much as five for a frugal millionaire as well as for a pauper; a

relatively more costly routine is resisted by language learners regardless of personal differences in working memory resources.

A similar point holds for Grüter's observation concerning the cost of the *not > all* interpretation in Korean. Because of the backtracking that it requires, this interpretation should incur a greater processing cost than the *all > not* reading in that language (section 4 of my essay). But it does not follow that Korean speakers who allow the more costly interpretation have a superior processing capacity, any more than there is reason to think that English speakers have more robust processing resources than Russian speakers for filler-gap dependencies. The more likely scenario in both cases is that exposure to particular patterns, however infrequent, triggers the creation of the more demanding interpretive routine.

Tanner and Herschensohn point to qualitative differences in how syntax is processed in the course of development, noting evidence for dual item-based and combinatorial processing streams. I acknowledge this contrast (O'Grady 2005:3) and welcome the finding that development might involve a transition from an item-focused approach to a more routine-based style of processing. On the view I propose, this is exactly what language acquisition should involve.

Tanner raises the additional possibility that adult *native speakers* may differ with respect to the type of processing in which they engage, even for classic syntactic phenomena such as agreement. If this turns out to be true, a crucial question will arise: are the two modes of processing nonetheless sensitive in parallel ways to considerations of cost? I predict that they should be. Regardless of which networks of neurons are activated to deal with agreement, binding, scope or filler-gap dependencies, no processing stream should support a default preference for cross-clausal filler-gap dependencies over intraclausal ones, for long-distance binding over local antecedents, for a scopal interpretation that requires backtracking over one that doesn't, and so on.

2. Scope

Crain questions the generality of the relationship between processing and scope that is embodied in the core example I consider—the contrast between SVO languages such as English and SOV languages such as Korean with respect to the scopal patterns in (6).

- (6) a. SVO language
Ted didn't eat [all the cookies].
- b. SOV language
Ted [all the cookies] didn't eat.

In English, the negative is available to the processor before it encounters the quantifier, allowing either the *not > all* or the *all > not* reading to be derived without backtracking—which is why children initially allow either interpretation.¹

¹ As this example reveals, I do not think that scope is isomorphic with word order in general (a totally implausible idea); I hold only that scopal interpretations that require backtracking are costly. The need for backtracking arises in certain word order patterns, of course, but that does not

In Korean, though, the quantified NP comes first and is immediately assigned a ‘full-set’ interpretation, creating a mental model (in MacWhinney’s sense) of an event involving Ted and the full set of cookies. Any subsequent attempt to derive a *not* > *all* interpretation requires a change to that model, resulting in increased processing cost.

Crain observes that the correlation between word order and scopal preference seems to break down in the case of disjunction.

- (7) a. SVO language
Ted didn’t eat [pasta or sushi].
- b. SOV language
Ted [pasta or sushi] didn’t eat.

Especially worthy of attention is the contrast between disjunction and universal quantification in SOV languages: whereas wide scope for negation in ‘*all*-patterns’ such as (6b) is costly and therefore avoided, there is no such effect for ‘*or*-patterns’ such as (7b), as Crain notes. Why should this be?

The quantifier *all* makes a fixed contribution to a sentence in which it occurs: *Ted ate all the cookies* is true only in a situation where Ted consumes each and every one of the cookies; it cannot describe a situation in which Ted ate just some of the cookies. It therefore seems reasonable to assume that, in the absence of negation, a universally quantified NP receives a full-set interpretation as soon as it is encountered, consistent with the usual assumptions about incremental processing. As noted above, any subsequent change should be costly—hence the difficulty of a *not* > *all* reading for (6b).

Things work differently for disjunction. A sentence such as *Ted ate pasta or sushi* is true (in all languages) under any one of three conditions:

- Ted ate pasta.
- Ted ate sushi.
- Ted ate both pasta and sushi.

All three options are inherent to the meaning of disjunction, and all three therefore remain active throughout the sentence. Setting implicature to the side for now, there are thus two interpretive possibilities from which a language of any type (SVO or SOV) must choose in cases of negation *regardless of where the negator occurs*:

- i. The *not* > *or* interpretation: The negative rules out all three interpretive options with which it is presented (Ted ate neither pasta nor sushi).
- ii. The *or* > *not* interpretation: The negative rules out just one of the options with which it is presented (pasta was not eaten, but sushi was; sushi was not eaten, but pasta was; both were uneaten).

amount to the claim that all scopal interpretations are determined by word order—contrary to what some commentators seem to believe.

In neither case is there any need for backtracking, and there is thus no reason to expect the sort of typological asymmetry associated with *all*.

The case of *and* is also revealing.

- (8) a. SVO language
Ted didn't eat [pasta and sushi].
- b. SOV language
Ted [pasta and sushi] didn't eat.

In the absence of negation, *and* permits just one interpretation ('both pasta and sushi'), which is presumably assigned as soon as the *and*-phrase is encountered in (8b). Wide scope for *not*—i.e., an interpretation in which Ted ate just one of the foods—will force a revision of that interpretation, with additional processing cost. No such problem arises in (8a), where the negative is encountered before the *and*-phrase, opening the door to either the *not* > *and* interpretation, as in English, or the *and* > *not* interpretation, as in Mandarin—both without backtracking.

A further prediction follows: the *not* > *and* interpretation should be more costly in SOV languages than in SVO languages, with clear-cut typological consequences: SOV languages should strongly favor the *and* > *not* interpretation, whereas SVO language should show variation in the reading they select. This is exactly what Crain's facts show.

Crain makes a second point, drawing attention to a developmental asymmetry that is surely among the most fascinating discoveries in the study of child language: regardless of language type, children initially permit only the *not* > *or* interpretation of the patterns in (7). Crain attributes this fact to an innate 'subset principle' that interacts with inborn parameter settings: children favor interpretations that make sentences true in the narrowest range of circumstances.

Conditions under which the sentences in (7) can be true on

<u>the <i>or</i> > <i>not</i> interpretation</u>	<u>the <i>not</i> > <i>or</i> interpretation</u>
•Ted didn't eat pasta, or	•Ted ate neither pasta nor sushi.
•Ted didn't eat sushi, or	
•Ted ate neither.	

Crucially, though, there is perhaps an alternative explanation: children may initially be drawn to the *not* > *or* interpretation for processing-related reasons. On the assumption that understanding a sentence involves computing the conditions under which it can be true, interpreting a sentence that can be true in three separate situations should be more costly (I predict) than interpreting a comparable sentence that can be true in just one of those situations—the exact contrast between the disfavored *or* > *not* and the favored *not* > *or* interpretations.

The opposite prediction holds for the negated *and* patterns in (8): there, the more demanding interpretation is the *not* > *and* reading.

Conditions under which the sentences in (8) can be true on

<u>the <i>and</i> > <i>not</i> interpretation</u>	<u>the <i>not</i> > <i>and</i> interpretation</u>
•Ted ate neither pasta nor sushi.	•Ted didn't eat pasta, or •Ted didn't eat sushi, or •Ted ate neither.

I therefore predict, as Crain also does, that English-speaking children should initially allow only the *and* > *not* reading for *Ted didn't eat pasta and sushi*, regardless of adult preferences. This is correct, as Crain reports.

3. Language acquisition

Grüter raises the possibility that speakers of Korean who permit the *not* > *all* interpretation in their first language will be more likely to accept it in English. Perhaps, but the Transfer Calculus (section 5.2 of my essay) predicts only that L2 learners will consider transferring the *dominant* processing routine of their first language to the second language. The dominant routine for scope in Korean is the one that yields the *all* > *not* interpretation, so only it is in play; as predicted, it is transferred to English. (Although some Korean adults and children PERMIT the *not* > *all* interpretation in their first language, there is no evidence that anyone PREFERS it, contrary to what Sedivy seems to imply.)

Yoon expresses skepticism about the mechanics of transfer, suggesting that it posits ‘an extremely sophisticated learner who is able to compute the potential cost of transfer on the target language.’ Not so. The cost of backtracking required to derive the *not* > *all* interpretation in Korean is not ‘computed’ by English-speaking L2 learners and compared to alternatives. It is simply felt as an increased burden on working memory at the point where backtracking is required, and for this reason is avoided in the absence of coercive input.

Yoon also wonders about how Korean-speaking learners of English come to replace the dominant *all* > *not* interpretation that they transfer from Korean with the *not* > *all* preference characteristic of English. Lee (2009) documents a correlation with overall proficiency, which suggests the effects of experience. But the relevant experience need not be entirely naturalistic—both anecdotal evidence and curricular materials point to an intense pedagogical effort to impose the *not* > *all* interpretation.

Four commentaries raise issues that go beyond the acquisition of *not-all* patterns. Orfitelli & Polinsky wonder why children learning French employ the ‘movement option’ for long *wh* questions (*Où dis-tu que Marie vit?* ‘Where do you say Mary lives’), even though the *in situ* option is common in short questions (*Marie vit où?* ‘Mary lives where’) in early child language and in parental speech. Doesn’t such an asymmetry flout processing considerations, given the cost of cross-clausal filler-gap dependencies alluded to in our earlier discussion of Russian? The likely answer is that the *in situ* option in biclausal questions has a countervailing cost. The key observation is that the *wh* word must have matrix scope (the intended interpretation is ‘Where did you say Mary lives?’, not ‘Did you say where Mary lives?’). Crucially, however, matrix scope for operators in an embedded clause is generally difficult to obtain (e.g., Reinhart 2006:62), arguably

for processing-related reasons—making the cost of cross-clausal movement more attractive than it would otherwise be.

Matthews & Yip observe that the Transfer Calculus predicts that Chinese speakers should carry over to English their native language's preference for the *some* > *every* interpretation of patterns such as (9).

(9) Some tourists visited every museum.

Curiously, an earlier study reports success by Chinese L2 learners at deriving the more costly inverse scope interpretation in English. As Matthews & Yip note, however, it is possible that less advanced learners will show the predicted transfer effect. This matter calls for additional investigation.

Chondrogianni & Tamburelli wrongly infer that I predict no transfer in the interpretation of negated disjunction by Japanese-speaking learners of English or by English-speaking learners of Japanese. In fact, each language has a dominant interpretation (*or* > *not* for Japanese and *not* > *or* for English) and neither interpretation is more costly in the second language than it is in the first, for the reasons noted in my discussion of (7a,b) above. The criteria for transfer are thus met, and the preferred interpretation in each L1 should be carried over to the L2. This is exactly what Grüter et al. (2010) find.

Unsworth reports that adult native speakers of English generally assign a specific interpretation to the indefinite NP in patterns such as *The boy didn't catch a fish*, but that they fail to transfer this interpretation to Dutch sentences such as (10), incorrectly assigning the direct object a non-specific interpretation.

(10) De jongen heeft een vis niet gevangen.
 The boy has a fish not caught
 'The boy didn't catch a fish.'

Unsworth treats this as a counterexample to the Transfer Calculus, but matters are not so clear. As Unsworth herself helps demonstrate (Unsworth et al. 2008), the interpretation of indefinites in both languages is heavily influenced by pragmatic factors. (It is worth noting in this regard that, unlike *all*, an indefinite NP exhibits ambiguity even when there is no negative—*The boy caught a fish* has both a specific and a non-specific interpretation.) If this is right, there may well be no sentence-level processing routine for English speakers to transfer to Dutch, and no reason to invoke the Transfer Calculus.

4. Grammar

Part of the claim developed both in my essay and in my earlier work (e.g., O'Grady 2005) is that grammar in the traditional sense does not exist. The claim is easy to misinterpret, unfortunately, and some exegesis is called for.

To begin, I obviously am not saying that the cognitive system underlying language use does not allow speakers and listeners to distinguish between acceptable and unacceptable sentences. Clearly it does, as repeatedly assumed throughout my essay. If one takes grammar to simply be whatever it is that allows

speakers to distinguish between well-formed and ill-formed expressions, then by definition, there must be a grammar—an uninteresting truism.

I prefer to take as my starting point the distinction made by Jackendoff (2002:57), who proposed that whereas the grammar is concerned with static structural relationships ('patterns'), the processor is devoted to on-line operations. Grammatical rules, he suggests, determine what the structure is (ibid.: 31), whereas processing routines say how it is built in real time. The question that I have asked is essentially this: if one correctly characterizes the processing routines that map form to meaning in the course of speech and comprehension, is there still a need for a separate set of mechanisms to specify the internal structure of sentences? My answer is no: the properties of well-formed sentences emerge from the processing routines that form and interpret them, and the properties of those routines emerge from the interaction of processing pressures with the input—the central theme of my essay.

Admittedly, however, my terminology invited misinterpretation, as I freely used terms such as 'clause,' 'noun,' 'verb', NP, and so forth for the sake of descriptive convenience. This led a number of commentators (e.g., Chondrogianni & Tamburelli, Costa, De Cat, Yoon) to rightly wonder whether in fact I had surreptitiously retained the very grammatical mechanisms whose existence I was denying.

Reference to part-of-speech classes is arguably benign, as it entails nothing other than recognition of the fact that words differ in their distributional properties. I said nothing in my essay about what the basis for those differences might be. Suffice it to say for now that I believe that part-of-speech contrasts can be traced to conceptual distinctions (e.g., Langacker 1987, 2008; O'Grady 1997:312ff; and Croft 2001, among many others). I do not subscribe to the view the syntactic categories are innate primitives (see, e.g., Pinker 1984, 1987 for extensive discussion), nor do I believe that they arise entirely on the basis of co-occurrence patterns in the input (e.g., Elman 2002, 2004).

But what of phrases? Here the stakes are somewhat higher, as reference to such elements appears to imply the sort of hierarchical structure found in traditional syntactic representations. But this is just an illusion. Consider in this regard the sentence *The man left*. In the system that I have adopted elsewhere (e.g., O'Grady 2010, in press), the processor operates in a slightly more anticipatory manner than assumed in the essay. In particular, as is widely acknowledged in the literature, past experience generates predictions about what lies ahead in a sentence. Thus, in the default case, exposure to the word *the* at the beginning of an utterance triggers the expectation of an argument, which in turn creates the expectation of a predicate. All other things being equal, this gives the preliminary semantic representation in (11), in which the definiteness feature of the determiner is immediately associated with an as-yet-unfilled first argument position in the grid of an anticipated predicate.

- (11) PRED
 < ₋ ... >
 +def

Next, comes the noun *man*, which contributes its denotation (represented here as *m*) as well as linguistically relevant features such as singular, human, masculine, etc.

- (12) PRED
 <*m ...*>
 +def
 +Sg
 ...

Finally, the processor comes upon the word *left*, and (based on its lexical properties) identifies it as a good candidate for the predicate position in the semantic representation.

- (13) LEAVE
 <*m ...*>
 +def
 +Sg
 ...

Are there syntactic constituents here?

Obviously, the semantics of *the* and the semantics of *man* have come together in some sense. But there is no corresponding syntactic phrase: at no point did *the* combine with *man* to form the unit *the man*. Rather, separate processing routines, invoked at separate times, allowed each word to independently contribute to the specification of the same argument in the sentence's semantic representation. Only in this very abstract sense are the two words linked to each other.

Equally obviously, there is a semantic unit corresponding to the clause *The man left*. Crucially, however, at no point is there a representation in which *the man* combines with *left*, let alone a VP projection headed by *left*. Clauses, like NPs, exist virtually at best. What we think of as constituents are just strings of words that contribute, via the mediation of separate processing routines, to constructing the same part of a semantic representation.

In recent decades, grammars have come to be thought of as cognitive systems that are represented in the minds/brains of a language's speaker. I believe that a much earlier view is more apt: grammars exist only in books, where they serve the valuable purpose of capturing in static form the effects wrought by the processing routines that shape language and ultimately explain why it is the way it is.

5. Conclusion

Snyder observes that the thesis at the heart of my proposal may not be as radical as it initially appears to be. I agree. Ultimately, everything that we know about our language has to be integrated into the processing routines that (uncontroversially) underlie the production and comprehension of sentences. Moreover, because learning a language involves learning to produce and understand sentences, a great deal of development on *anyone's* account involves processing improvement. The key question is whether there is a residue of

linguistic knowledge (and, therefore, of language acquisition) that is independent of processing.

There certainly *seems* to be, but that may be an illusion. The goal of my essay was to consider a phenomenon normally assigned to the residue in order to probe the problems and prospects of an approach to language that takes processing as a unifying explanatory construct for the study of acceptability, interpretation, variation and development. And, contra Ambridge, I wanted to do so for a phenomenon for which there are firm judgments in the absence of rich experience—because that is where the deepest disagreements lie. As the commentaries and my response reveal, I hope, there is lot to think about on both sides of the issue and little reason to draw firm conclusions just yet.

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