Pinning down the concept of interface in bilingual development

A reply to peer commentaries

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My article "Pinning down the concept of 'interface' in bilingual development" appeared in LAB 1.1 and was followed by 18 commentaries. I am grateful to the commentators for raising many insightful issues, and to the editors of LAB for inviting me to respond.

Before I address the commentaries in detail, let me provide a short summary of the original article. The article was an overview of research on interfaces carried out within three bilingual domains: bilingual first language acquisition, adult second language (L2) acquisition, and native language (L1) attrition. The term "Interface Hypothesis" (IH) was coined by Sorace and Filiaci (2006) as a provisional label for an attempt to unify findings from these traditionally separate research subfields. The focus of much of the research underlying the IH is on the syntax-pragmatics interface and, within this, on anaphora resolution in different languages. Convergences among the three bilingual populations are attested for null subject languages, pointing to an asymmetrical overextension of the overt pronoun option to contexts requiring the use of null subject pronouns. In bilingual speakers of a null-subject language and a non-null subject language, this phenomenon was first attributed to the 'bleaching' of pragmatic conditions on the overt pronoun in the null subject language (e.g., Italian), due to the effect of the language that does not operate a choice of pronominal forms based on these conditions. (e.g., English). However, the attested overextension of overt pronouns in bilingual speakers of two null subject languages (e.g. Spanish and Italian) casts doubts on this explanation as the only basis of the phenomenon. More recent research indicates the possibility that the overt pronoun may function as a default form employed to compensate for inconsistent efficiency in mapping pronominal choice and pragmatic conditions. The inconsistency may in turn be caused either by the competition for processing

resources created by the need to separate the two languages, or perhaps by a specific difficulty in integrating information in real time.

The commentators address issues of *definition*, *scope*, and *explanatory power* of the IH. Several commentaries (Duffield, Gürel, Pérez-Léroux) correctly point out the danger of circularity in the existing definition of interface. According to Duffield, we can't "let the speakers' behaviour decide" what counts as an interface phenomenon: problematic phenomena may be regarded as belonging to interfaces because they are difficult for the bilingual speaker, and difficult because they require interface conditions. I would suggest that we use the term 'interface' as a descriptive device that allows us to capture different types of conditions on syntactic realization. This would mean giving up neat dichotomies such as 'narrow syntax vs. interfaces' but also neat distinctions between syntax-pragmatics, syntax-semantics, etc. and focusing on computational complexity as a key factor, as Hopp suggests. While this move might appear to entail a loss of explanatory and predictive power, it goes hand-in-hand with a stronger empirical basis and will ultimately lead to stronger and more explanatory theoretical accounts. It may well emerge not only that the syntax-pragmatics interface is a typical locus, rather than a special locus of difficulty, but also that narrow syntactic properties and internal interfaces can be computationally complex and resource consuming too, as Gürel observes. I would concur with Hopp that we should investigate systematicity across interfaces in bilinguals, because problems at one interface may be caused or exacerbated by inefficient computations at another. Avrutin (1999) was among the first to stress the need to look beyond grammatical representations in first language acquisition and examine real-time ease of access to knowledge as a cause of protracted optionality. In this respect, there are two sides of the question: (a) overall processing resources, which may not be available for all kinds of computations (and may be drained by syntactic operations that are 'proceduralized' in L1 but not in L2, for example), and (b) specific interface mapping operations that might require different types of cognitive resources. I think it may be possible to determine a hierarchy of computational difficulty in highly proficient bilinguals, with structures requiring more proceduralized 'internal' mappings being less taxing than structures requiring the rapid integration and updating of contextual information. We need more experimental data and also (*pace* Gürel) more 'data-driven' hypotheses before we are in this position. It is quite clear that once we take into account the interactions of linguistic and non-linguistic factors, individual differences (in working memory and executive function, for example) assume a much more important role than before. Moreover, Schwartz raises the important point that if bilingualism itself is the main cause of some interface processing problems, then these problems should be mirrored in both languages: we are now actively testing this prediction.

O'Grady makes the interesting (and empirically testable) suggestion that overt pronouns may be over-extended because of a locality principle: the processor may prefer computations drawing on information that is locally and immediately available. So for example in *Mentre Gianni mangia, parla al telefono* 'While Gianni eats, ø speaks on the telephone,' the antecedent for the null pronoun is in the previous clause; if speakers don't have sufficient processing resources to track agreement, they will produce an overt pronoun. This account predicts that overt pronouns shouldn't be overused when both languages lack agreement or when both languages allow object drop, or in coordinate structures where the two verbs are in the same clause (*Gianni mangia e parla al telefono* 'Gianni eats and speaks on the telephone'). Like Hopp, O'Grady assumes that the problem at the root of the phenomenon may not be specifically related to the syntax-pragmatics interface but rather indirectly caused by a morphological difficulty. Let's do the experiments to find out whether these predictions are on the right track.

Tsimpli observes that the solution employed by late bilinguals to compensate for inefficiency in syntax-pragmatics mapping involves the use of a 'learner default,' which is not the same as the 'linguistic default': in fact, the null pronoun is the linguistic default but the overt pronoun is the learner default. Learners 'void' the marked form of its context/discourse sensitive features and broaden the range of contexts in which it can be used. The default form thus comes to be associated with more than one interpretation. The question then is why the default is more accessible to less automatic processes: Tsimpli leaves open the possibility that the cause may be representational, i.e., that discourse-interpretable features are not narrowed down to target language options in the bilingual lexicon. These observations open up the interesting hypothesis that structures that are typically acquired late are those presenting a discrepancy between the linguistic default and the learner default, which we have explored in recent work on simultaneous vs. consecutive child bilingualism (Unsworth et al., 2011).

The scope of the IH is also a recurrent theme in the commentaries. Some (Duffield, Tsoulas, Schwartz) focus on the linguistic scope, i.e., its empirical basis and/or the classification of phenomena as pertaining to specific interfaces; others comment more generally on the domain(s) of applicability of the IH. The special attention paid to anaphora resolution in research on the IH seems to be a contentious point. In a field that often presents a fragmented picture consisting of small-scale unreplicated studies, the fact that we now have a body of results on the same phenomenon across bilingual groups can only be to our advantage, in my view. Furthermore, anaphora resolution is an area that has been extensively studied not only from a theoretical linguistic perspective but also from a processing perspective (Burkhardt, 2005; Trueswell et al., 1999; Snedeker & Trueswell, 2004; Kazanina & Phillips, 2010, a.o.), providing a rich interdisciplinary background

against which we can compare results. Despite the emphasis of much research on anaphora resolution, however, it is definitely not the case that the IH has been applied only to anaphoric expressions (see Slabakova & Ivanov, 2011; White, 2011 for overviews) or that it predicts difficulties only at external interfaces, as Pires & Rothman contend. Sorace and Serratrice's (2009) comparison between intuitions on pronominal forms and bare plural nominals in subject position indicates that bilingual children adopt different strategies at the two interfaces, resulting in different outcomes: crosslinguistic influence (from English to Italian) applies only to the syntax-semantics interface but not to the syntax-pragmatics interfaces. This discrepancy in the same children clearly shows that syntax-semantics interface properties are not immune to problems — but the processing operations required and the resources needed by bilingual children might be different from those involved at the syntax-pragmatics interface. Our studies support Pires & Rothman's very point that "external interface asymmetries should be dependent on L1/L2 pairings whereas external interfaces should not…"

The problem in identifying what counts as 'interface' or 'narrow syntax' both within and across languages is raised by a number of commentaries. Gürel is right in pointing out that we need to test different structures including 'core' syntactic phenomena. This has been done e.g. by Tsimpli et al (2004), who tested different kinds of extractions as the syntactic counterparts of anaphora resolution and found no attrition effects on them. Duffield asks what the IH would predict if the core vs. interface properties in one language are aligned differently in another. For example, in Vietnamese, null subjects are licensed by Topicalization, so Topicalization in this language would seem to be the equivalent of Agreement as a syntactic licensor of null subjects and therefore a 'core' property. However, we should distinguish between syntactic licensing mechanisms (which may vary depending on the language) and the pragmatic conditions governing the distribution of pronouns. So the problem raised by Duffield exists only to the extent that we force a superficially similar phenomenon into the same category. If we take the relevant language-specific alignment as the critical factor, we expect that topics may indeed have a different status in Italian and Vietnamese and that bilinguals speaking these languages (and monolinguals also, to a lesser extent) may display instability not at the level of the syntactic set-up of a null subject language but at the level of use of pronominals in particular contexts.

Both Tsoulas & Gil and Slabakova object to my distinction (based on Gundel & Fretheim, 2004) between Focus as a relational concept and Topic as a referential concept, arguing that both concepts are equally dependent on pragmatic knowledge. The point here is not whether only one or both concepts involve pragmatic knowledge, but rather whether only one of both concepts involves coordination between linguistic and non-linguistic factors. The relational givenness/newness

of Focus is necessarily a property of linguistic representations, while referential givenness-newness of Topic is not specifically linguistic: this appears consistent with some developmental patterns attested in bilinguals, which show more difficulty with external interface conditions than with internal interface conditions. Again, the problem evaporates once we remove a rigid distinction between core syntax and interfaces and instead allow for a range of interface conditions, graded according to their computational complexity and their dependence on extra-linguistic factors.

The scope and domain of applicability of the IH is the target of several commentaries (Lardiere, Montrul & Polinsky, and White). The objections are aimed in particular at my claim that we should be wary of over-extending the IH to domains and stages for which it wasn't formulated: for example, intermediate L2 developmental stages or inter-generational attrition. I may have been less than clear on this point and I take full responsibility for this. The IH is an account of convergent patterns of optionality found in very advanced L2 attainment and individual L1 attrition; by itself it is not a developmental account of L2 acquisition or L1 attrition, although if interpreted correctly it can make predictions about developmental stages in these two domains. Lardiere is therefore correct in stating that "The IH [is] about what is left after almost everything else has been acquired," in the sense that it was proposed to explain the restrictions on residual optionality in near-native speakers. While it is true that ultimate attainment refers to L2 end-states in general and not just to near-native states, it is research on near-nativeness that gives us the most revealing information about the limits of late bilingualism.

Lardiere and White comment that if near-natives have residual optionality at the syntax-pragmatics interface, it is reasonable to expect intermediate learners to have similar problems; as White puts it, interface problems don't "emerge out of the blue." This is certainly true, but the point of the IH is that unlike many of the developmental problems that are reduced or eliminated as L2 proficiency grows, performance at the syntax-pragmatics interface may remain permanently unstable (granted that some difficulties at the syntax-morphology interface, possibly of a different nature, may also be maintained, as argued by both Hopp and Lardiere).

From the perspective of L1 attrition, Montrul & Polinsky question the (ir)relevance of the IH for heritage speakers. As they correctly claim, all heritage speakers are bilingual (even if on a range of proficiency levels); and indeed most bilingual children are heritage speakers of the minority language. They further say that heritage speakers "are reported to lose the pro-drop feature or to use it in a more limited manner." However, loss and more limited use are two very different scenarios and deciding which one holds is an empirical question. The IH claims that during the initial stages of the attrition process in individual speakers removed from their original community, the ability to rapidly coordinate syntactic and pragmatic/

contextual information becomes inconsistent, but the speakers' grammatical representations do not change. These speakers then provide input affected by attrition to second generation speakers (i.e., heritage speakers), who may acquire a different grammar from that of their parents: this is a different stage of the attrition process and does not present the same characteristics as those identified by the IH (see also Sorace, 2004; Rothman 2007; 2009). My claim that heritage speakers are exposed to qualitatively different input (Sorace 2005) is now beginning to be supported by a number of studies (e.g. Pires and Rothman Rothman, 2009; Place & Hoff, 2011; Unsworth, 2011). Montrul & Polinsky are right in claiming that heritage speakers are an important testing ground for the IH: the IH can make predictions for subsequent stages of attrition, as long as the differences between individual and generational attrition are clear.

The importance of input in bilingual development is also discussed, in different ways, by Liceras and Paradis. Liceras focuses on Paradis & Navarro (2003) to argue that the extension of overt pronoun found in the bilingual Spanish-English child Manuela is due only to the Spanish input from the child's Cuban father and her mother as a non-native Spanish speaker, rather than to the influence of English. Schwartz also reminds us that recent results by Filiaci (2011; Filiaci et al, forthcoming) show that the overt pronoun in Spanish has a wider distribution than in Italian, and this undermines the account that overt pronouns may be overextended by bilingual speakers as a consequence of bilingualism itself. We should certainly continue to explore differences in interface conditions among null-subject languages (as also suggested by Prévost); it is possible that multiple effects conspire in the overextension of the overt pronoun, so that the phenomenon may have a basis in bilingualism itself but also be reinforced if bilingual speakers have one language that uses overt pronouns more widely than the other. It is likely that input (both qualitatively and quantitatively) may matter more for interfaces involving external conditions that for internal interfaces and, as Paradis argues, the impact of input may be greater for minority languages than for community ones. Recent work by Unsworth et al (forthcoming) provides empirical data that address these issues in greater depth than before.

Some commentators address the issue of taking monolinguals as the point of reference for bilingual speakers. Prévost, quite rightly, points out that optionality exists in monolingual native speakers as well, and Schwartz claims that native Italian speakers don't behave as expected as a control group because they often choose a non-subject antecedent for null pronouns, inconsistently with Carminati's (2002) Position of Antecedent Strategy (PAS) principle and with the assumption of 'stable and consistent preferences.' There is no doubt that variability exists in this domain in native speakers and that the native data are more consistent with a pragmatics account (e.g. Ariel, 1990) than with a strictly syntactic account: so in

a sentence like *Maria saluta Paola mentre attraversa la strada* 'Maria greets Paola while ø crosses the street,' both referents in the main clause are equally prominent and serve as potential antecedents for the null pronoun, hence the sentence is ambiguous. It is still possible that a subject antecedent preference may be more likely to emerge in tasks that tap the speaker's implicit knowledge, such as priming.

Finally, several commentaries mention the role of linguistic theory (Pérez-Leroux, Pires & Rothman, Sharwood Smith, Sprouse, Tsimpli), both as a source of the IH and as an interpretative framework, pointing out either divergences from theoretical notions of interfaces or the dangers of agnosticism with respect to UG. We have to commit ourselves to existing theoretical frameworks and cannot combine then lightly, says Sharwood Smith. According to Sprouse, it is hard to see how an over-arching interpretative framework can be achieved without an assumption of Full Access to UG. The point here is not whether we need linguistic theory: I think it is evident that we do, to the extent that we need to understand the phenomena we target in research on bilingualism. The point is rather whether we can expect to achieve an explanatory account of the range of emerging phenomena without engaging with other research fields: we simply cannot explain everything in terms of linguistic theory. Sometimes an 'either-or' approach and a tendency to think in dichotomies do not serve any useful purpose: phenomena are generally not due to *either* linguistic *or* to processing factors, but are likely to be determined by complex interactions between the two. I would like to think that one of the more general contributions of the IH is to open up interdisciplinary channels between researchers and encourage more regular exchanges of ideas, methods, and results. This way we may achieve not only a better understanding of bilingualism but also, perhaps, make some progress in comprehending the place of language within a general model of cognition.

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