

## RESEARCH NOTE

# Remarks on Jake, Myers-Scotton and Gross's response: There is no "Matrix Language"\*

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*This research note presents reactions to Jake, Myers-Scotton and Gross's (2005) response to MacSwan (2005), the latter offered as a response to Jake, Myers-Scotton and Gross (2002) and as a general critique of the MLF model of Myers-Scotton and colleagues. The note responds to the authors' analysis of various linguistic examples and to their continued assertion that the MLF model (and "matrix language" concept in particular) is necessary to any successful analysis of codeswitching data. The authors' critique of MacSwan's (2005) analysis of some Spanish–English DP facts is shown to fail, demonstrating that there is no "matrix language". The note advocates that researchers reject across-the-board constraints on codeswitching in favor of a research agenda which relies upon independently motivated principles of grammar for the analysis of bilingual language data, with no codeswitching-specific mechanisms permitted.*

I am pleased to comment on Jake, Myers-Scotton and Gross's (2005, this issue) response (henceforth, JMSG<sup>2</sup>) to MacSwan (2005), written in reply to Jake, Myers-Scotton and Gross (2002) (henceforth, JMSG<sup>1</sup>). I will address the six objections raised in JMSG<sup>2</sup>, following their order of presentation.

1. JMSG<sup>2</sup> charge me with seeking to explain bilingual language data using a monolingual theory. A better characterization of my view is that I believe principles of language design urge us to begin with the simplest assumption, namely, that there is no difference in the way language is represented in the mind/brain of a bilingual and of a monolingual; we should admit additional mechanisms or design assumptions which specifically apply to bilinguals only when compelled to do so by the evidence. The simplest assumption, then, is that bilingualism is a kind of linguistic illusion owing, perhaps, to sociopolitical factors. In defending the "matrix language" (ML), JMSG<sup>1</sup> assert that it is universal, existing "in monolingual language as well as bilingual language" (p. 88). This appears to be a version of the hypothesis that monolingual and bilingual language are governed by the same underlying structure, but in this instance we are additionally burdened with the ML construct.

An analysis of the CS data leads to the conclusion that the simplest assumption – that bilingualism is an illusion – is false, and that some components of the linguistic system

(e.g. phonology, lexicon) are differentially encapsulated in bilinguals, a point made in MacSwan (2000). The idea that there are no CS-specific constraints is also present in Woolford (1983) and Mahootian (1993), among others.

JMSG<sup>2</sup> additionally take issue with my use of the terms "union" and "mixed grammars", claiming that MacSwan "never elaborates on what this 'UNION' might be" (p. 277). I use the term UNION in its standard sense in formal linguistic theory, deriving from set theory: The union of A and B ( $A \cup B$ ) is the set of all elements which appear in both A and B. The meaning should have been clear from the formalism used in (1), from MacSwan (1999, p. 148).

$$(1) \text{ Numeration} = \{\langle w_1, w_2, \dots \rangle \mid w \in \{Lex(L_1) \cup Lex(L_2), \dots\}\}$$

The Numeration is constructed by the application of *Select* to either lexicon; the derivation proceeds as in the monolingual case, with *Merge* and *Move* building and rearranging structure to the extent uninterpretable features may be checked, with no CS-specific mechanisms permitted.

The term "mixed grammars", which JMSG<sup>2</sup> view as being at odds with other notions, is admittedly ambiguous between a collective and distributive reading; the phrase is used in the context of my statement of the CS research program, given in (2).

(2) Nothing constrains CS apart from the requirements of the mixed grammars.

\* Many thanks to Kara McAlister and Kellie Rolstad for commenting on an earlier draft of this paper. Any errors, of course, are my own.

JMSG<sup>2</sup> appear to interpret (2) to refer to multiple grammars, each mixed, rather than to separate grammars, mixed during CS (the intended meaning).

2. JMSG<sup>2</sup> protest my claim that negative evidence is needed in the construction of a generative grammar in order to satisfy Chomsky's (1957) definition: The grammar of L is "a device which generates all of the grammatical sequences of L and none of the ungrammatical ones" (p. 2). Without negative evidence – examples of ill-formed expressions – we cannot evaluate whether a grammar erroneously sanctions ill-formed constructions as well-formed. This observation is made in connection with the essentially exclusive reliance of the MLF model on positive evidence and its corresponding failure to rule out numerous ill-formed CS structures. JMSG<sup>2</sup> claim that grammaticality judgments on CS are unreliable because of "the negative evaluation that some bilinguals have of CS" (p. 277).

However, one cannot exclude grammaticality judgments based on such considerations. First, it is by no means universally true that CS is stigmatized behavior; indeed, for some communities it is very prestigious. Second, there are numerous languages which are at least as stigmatized as CS is anywhere (i.e. indigenous languages in colonial settings), and researchers have used grammaticality judgments to study these for years. Thus, a balanced perspective is required – one which makes careful use of all linguistic data, attending to their inherent messiness and special limitations; however, negative evidence is indispensable to a generative theory of CS, as we would never know if our grammars are too powerful without it. (See Toribio, 2001, pp. 210f. for related remarks.)

JMSG<sup>2</sup> further charge me with declaring "which of the existing CS examples are acceptable, which are to be rejected, and which are to be reanalyzed, especially in response to [JMSG<sup>1</sup>]" (pp. 271–272). Of course, linguistic analysis is the task at hand, and offering a new analysis for proposed counter-examples in JMSG<sup>1</sup> is an invitation to scholarly discussion. Although I did not accept a German–English example in JMSG<sup>1</sup> on the grounds that it was missing from its referenced source document and was at odds with other published data, I actually did not contradict the authors' presentation of the grammaticality of any of the items they discussed.

3. JMSG<sup>1</sup> produced numerous proposed counter-examples to the PF Disjunction Theorem of MacSwan (1999, 2000). Adhering to (2), the PF Disjunction Theorem posits that ordering relations among rules or constraints within a language-specific phonology prevent switching from one system to another except at the boundaries of syntactic heads. A counter-example to the PF Disjunction Theorem should thus exhibit a change in phonological systems below X<sup>0</sup> – either word-internally or between adjoined syntactic heads.

The bulk of JMSG<sup>1</sup>'s proposed counter-examples consisted of single-word switches. I indicated that the authors erroneously attributed to me the view that such items should be ill-formed, an error which JMSG<sup>2</sup> graciously admitted (p. 275). In addition, JMSG<sup>1</sup> presented a Croatian–English item from Hlavac (2000) purporting to show word-internal switching; here too, the authors erred in their reading of the source document and misinterpreted the datum, as I previously pointed out; JMSG<sup>2</sup> also graciously admit this mistake (p. 275). Four plausible counter-examples remained, each addressed in MacSwan (2005). JMSG<sup>2</sup> discuss two of these.

One is a Swahili–English example gleaned from Myers-Scotton's unpublished corpus in which Swahili agreement affixes are attached to a phonologically English stem. I noted, based on other data, that the expression was well-formed only if the English verb was barren of inflectional material. Hence, we get **m-tu-evaluate** (2PL.SUB-1PL.OBJ-evaluate, "You evaluate us") and **m-tu-call** (2PL.SUB-1PL.OBJ-call, "You call us") but not **\*a-tu-evaluates** (3PL.SUB-1PL.OBJ-evaluate-s, "He evaluates us"), **\*m-(li)-tu-evaluated** (2PL.SUB-(PAST)-1PL.OBJ-evaluated, "You evaluated us"), **\*a-tu-calls** (2PL.SUB-1PL.OBJ-call, "He calls us") or **\*m-(li)-tu-called** (2PL.SUB-(PAST)-1PL.OBJ-called, "He called us"). Drawing on Pollock (1994), who shows that English stems remain *in situ* unless inflected, and Cocchi (2000), who argues that Swahili agreement morphemes are clitics as in Romance rather than agreement affixes, I posited that in the ungrammatical inflected cases, the verb raises to check features with T and adjoins with the clitic heads, violating the PF Disjunction Theorem; but in the case of the bare stems the verb remains *in situ*, and no such syntactic compound is formed.

Although they do not dispute the grammaticality facts, JMSG<sup>2</sup> mention that they were "puzzled" as to why I created "non-occurring examples" (the ill-formed Swahili–English cases) and then say "they do not occur" (p. 272). Of course, I did not say these items "do not occur", but that "grammaticality judgments rendered by a Swahili–English bilingual" indicated that they are ill-formed (MacSwan, 2005, p. 14). The authors further complain that MacSwan "implies that because Pollock (1994) argues that verb raising does not apply to English verbs, it, therefore, does not apply to an English verb in a Swahili frame" (p. 272). I actually did not IMPLY this, but said it directly. In Pollock's theory, whether a verb raises is determined exclusively by relevant grammatical properties; if the absence of verbal inflection correlates with non-raising, then the facts would obtain regardless of the "language frame".

Regrettably, JMSG<sup>2</sup> do not present any empirical or theoretical arguments suggesting that my analysis of these structures is incorrect. Rather, they cite a few traditional analyses of Swahili morphosyntax which they believe to

be preferred, but no argument is made as to why they might be better, and they conclude with a brief account of the Swahili–English facts using the MLF model. However, if the analyses of these CS facts are empirically equivalent, with no specific evidence offered to decide between them, then we are left only with parsimony to aid our selection, and the MLF model brings along considerably more machinery than the alternative view.

The other example which JMSG<sup>2</sup> discuss is an English–German item (*ge-cured*) said to exhibit phonological switching. JMSG<sup>2</sup> assert that I reject this datum because I question “the veracity of the examples that occur in unpublished corpora” (p. 272). While I do raise questions about the authenticity of this example, I believe the questions are legitimate; however, it is incorrect to state that I questioned it because it originated in an unpublished corpus. Rather, I did not engage in a discussion of the item because neither it nor any analogous form could be found in the work cited – namely, co-author Gross’s (2000) dissertation. I therefore dubbed the item “spurious” (that is, lacking authenticity in origin) and turned to other data to evaluate the matter. I recognize that this particular choice of words caused some personal offense to the authors, a matter I regret; however, I did suggest that the item “may be part of Gross’s larger corpus, or may be an informal recollection, approximately cited as his dissertation”, but “[g]iven the detailed claims about the unusual phonetic shape of this word, and that it is cited as an ‘instance’ of a more general pattern, we reasonably expect to see a non-anecdotal report involving at least one documented occurrence” (p. 13, note 10).<sup>1</sup>

I addressed the question of whether one might find a German past participle with a phonologically unintegrated English stem by reviewing other data sources and by consulting two German–English simultaneous bilinguals. The published examples were from Gross (2000) (*gefıxt*, but not *gecured*), Fuller (1999),<sup>2</sup> and Cantone (2005) (German–Italian). These sources report data consistent with the grammaticality judgments of the German–English bilinguals, who found the forms in question acceptable only if the stems were phonologically integrated into German.

JMSG<sup>2</sup> attempt to counter these observations by quoting an item from Fuller’s unpublished corpus, and then quoting a personal communication from Fuller advising against relying upon her transcripts as a guide since “they may not be reliable on this point [of phonetic detail]” (cited in JMSG<sup>2</sup>, p. 272). JMSG<sup>2</sup> similarly quote a personal

communication from Clyne indicating that his corpora are full of examples of this sort.

I certainly agree with Fuller’s comment, and was reluctant to accept the undocumented *gecured* example with similar considerations in mind: CS researchers have historically not paid close attention to the phonetic character of their data (with the notable exception of Grosjean and Miller, 1994), and relying upon corpora which were not collected specifically with this question in mind may be misleading. In MacSwan (2005), I examined what published data I could find and noted only that “the orthography suggests” (p. 13) in all cases that the stems are phonologically integrated into German if prefixed with the German past participial *ge-* morpheme, as predicted by the PF Disjunction Theorem.

The German–English simultaneous bilinguals I interviewed in the U.S. gave strongly negative reactions to *gecured* and other items produced with final voicing of /d/ as [d], as in English, as reported in MacSwan (2000, p. 13). JMSG<sup>2</sup> dismiss this finding by asserting that MacSwan “gives us no information about [the German–English bilinguals’] linguistic histories nor about their attitudes toward CS” (p. 272). But this is false; the information is given in note 11 on page 13, and to my mind the data remain persuasive.

Careful study of the phonological characteristics of CS promises to lead to a better understanding of how a bilingual’s phonological systems may or may not interact in CS. The PF Disjunction Theorem is an attempt to characterize these constraints without relying upon any CS-specific mechanisms, based on our current very crude understanding of the phonological facts in bilingual speech. We are certain to discover that the PF Disjunction Theorem is false in its current form, but it appears to be the right kind of device; it looks to independently motivated principles of language design to characterize a broad range of facts in CS. Refinements of the mechanism are currently being pursued (e.g. Colina and MacSwan, 2005).

4. JMSG<sup>2</sup> claim that my interpretation of the MLF model is limited to Myers-Scotton (1993) and fails to consider more recent developments; they address four of the counter-examples I presented regarding the MLF model.

With respect to my presentation of the MLF model, JMSG<sup>2</sup> write:

All [MacSwan’s (2005)] quotations come from the Myers-Scotton (1993) version, even though his list of references include more recent publications that might have kept him from going astray. For example, MacSwan criticizes the notion of identifying the Matrix Language by counting morphemes, a notion which was dropped in the 1997 second edition of *Duelling languages* and does not appear in any subsequent articles or books (pp. 272–273).

<sup>1</sup> In addition, because Gross’s (2000) study concerns language attrition rather than CS, a careful review of the speakers’ linguistic histories should be considered.

<sup>2</sup> JMSG<sup>2</sup> indicate that I source these data to Fuller (1997), but I actually referenced Fuller (1999). The data may be the same.

Although I would not claim to know the MLF model as well as its proponents do, I believe I gave it a fair presentation and consulted recent publications in doing so, such as Myers-Scotton and Jake (2001), Myers-Scotton (2001, 2002), and JMSG<sup>1</sup>. While I mentioned the morpheme-counting approach to identifying the ML of Myers-Scotton (1993), I specifically dub it a product of “earlier work”, and congratulate Myers-Scotton and colleagues for abandoning it in favor of a structural definition “in more recent work” (MacSwan, 2005, p. 8). Hence, the characterization of my treatment of the MLF model by JMSG<sup>2</sup> is demonstrably false.

The real controversy here may again relate to differences of opinion regarding the nature of a linguistic theory. Just as the construction of a formal grammar is crucially dependent on the availability of negative evidence (i.e. a corpus of ill-formed expressions), it is also crucially dependent on the criterion of EXPLICITNESS: Once a principle is formulated, its truth or falsity depends on whether its specific formulation is consistent with the available evidence. As Chomsky (1957, p. 5) put it,

The search for rigorous formulation in linguistics has a much more serious motivation than mere concern for logical niceties or the desire to purify well-established methods of linguistic analysis. Precisely constructed models for linguistic structure can play an important role, both negative and positive, in the process of discovery itself. By pushing a precise but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy and, consequently, gain a deeper understanding of the linguistic data.

Thus, it is not sufficient for JMSG<sup>2</sup> to assert on the basis of their authority that a particular interpretation is correct or incorrect, or that a specific lexical item is or is not a system morpheme; rather, they must demonstrate this to be the case based on a consideration of the formal definitions involved.

Unfortunately, tenets of the MLF model are so rarely stated in clear, structural terms they can scarcely be evaluated. Rather, Myers-Scotton and colleagues tend to talk about how various morphemes behave in terms of Levelt’s (1989) *Speaking Model*. While we are often told when elements become “activated”, we are never precisely told what they *are*. For instance, regarding system morphemes, Myers-Scotton and Jake (2001) report that

“early” system morphemes are activated at the lemma level, but they do not assign or receive thematic roles. Such morphemes are “indirectly elected” because content morphemes “point to” them (cf. Bock and Levelt, 1994, p. 98).

We are nowhere given a sense of what an “elected” morpheme is nor what it means to “point to” a morpheme. Although we can try to imagine some formal construal of these terms, the notions are difficult to evaluate empirically in the absence of clear structural definitions.

The 4-M Model of Myers-Scotton and Jake (2000, 2001) distinguishes four types of morphemes: (i) content morphemes, (ii) early system morphemes, (iii) bridge late system morphemes, and (iv) late outsider system morphemes (Myers-Scotton and Jake, 2000, p. 1062). According to the MLF model, in its current instantiation and as represented in MacSwan (2005), the “late outsider system morphemes” comprise the subtype of system morphemes relevant to the MLF model of Myers-Scotton (1993) (see JMSG<sup>1</sup>, p. 74).

Hence, a clear definition of “late outsider system morpheme” is essential if we are to evaluate the empirical predictions of the MLF model. According to Myers-Scotton and Jake (2001), this class of morphemes “depends on grammatical information outside their own maximal projection” (p. 100). The definition is similar in Myers-Scotton’s (2002, pp. 75f.) latest independent work:

[C]onsider the two types of late system morphemes; they have different syntactic specifications from each other. They differ in regard to the feature [+/-looks outside its own immediate maximal projection for information about its form] . . .

“[O]utsider” late system morphemes depend for their form on information outside their immediate maximal projection. That is, they are coindexed with forms outside the head of their maximal projections.

Although some reference is made to structure and to linguistic relations, the definition remains extremely vague. Crucially, we do not know what kind of information a late outsider system morpheme seeks, and in what sense such information would “determine the form” of an item. We can only assume that the definition refers to any information which might license the item in some way, and so we take a late outsider system morpheme to be any morpheme that has grammatical relations with an element outside its maximal projection and has consequences for its form or allomorphy.

So construed, the MLF model makes reasonably clear predictions which can be empirically tested. Importantly, it should be noted that JMSG<sup>2</sup> are obliged, just as the rest of us are, to provide argument that an element does or does not meet the definitions they offer, rather than simply asserting, for instance, that “English determiners are early system morphemes under the 4-M model”, not late outsider system morphemes (p. 272).

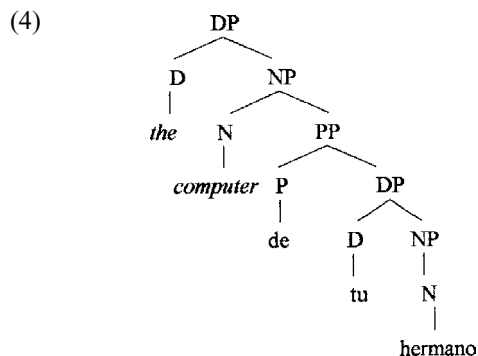
The MLF model posits that in CS all late outsider system morphemes will be contributed by one language within a CP, called the ML (the System Morpheme Principle), and that the order of all morphemes will conform to that of the ML (the Morpheme Order Principle). However, “Embedded Language (EL) islands” may occur; these are maximal projections which are “not inflected with ML system morphemes, although they occur in positions projected by the ML”, and are composed

only of elements from the EL (JMSG<sup>1</sup>, p. 77; cf. JMSG<sup>2</sup>, p. 272).

Let us now turn to the counter-examples which JMSG<sup>2</sup> dispute. The structure of the underlined DP in (3a) is essentially as shown in (4); the underlined DP in (3b) has a similar structure, but would additionally contain an adjective.

- (3) a. ¿Funciona the computer de tu  
 function-3SG.SUB the computer of your  
hermano en la oficina?  
 brother in the office  
 "Does your brother's computer in the office work?"

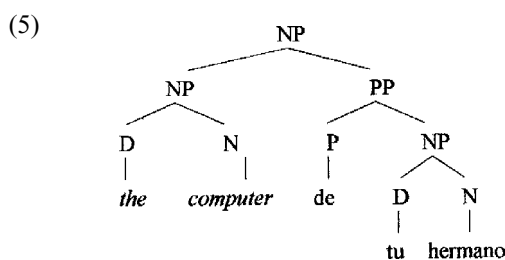
- b. Tell them you'll buy xune jaedid with  
 tell them you'll buy house-POSS new with  
blue shutters when you sell your own house  
 blue shutters when you sell your own house  
 "Tell them you'll buy a new house with blue  
 shutters when you sell your own house."



I argued that (3a) violates the System Morpheme Principle because it contains a late outsider system morpheme (*the*) from English as well as others from Spanish, and the DP cannot be analyzed as an EL island as it contains other-language elements within its maximal projection. JMSG<sup>2</sup> dispute this analysis by asserting that English determiners are not late outsider system morphemes and that [*the computer*] is a self-contained EL island dominated by NP. However, *the* indeed "looks outside its own immediate maximal projection for information about its form", and therefore meets the definition of a late outsider system morpheme. More specifically, the determiner enters into a checking relation with T to value its uninterpretable features. (We return to the structure of the DP directly.)

Similarly, (3b) (uncontroversially) contains late outsider system morphemes from both Farsi and English. Because (3b) exhibits Farsi word order within the DP and English word order elsewhere, it is predicted to be ill-formed by the Morpheme Order Principle, contrary to the facts. JMSG<sup>2</sup> here too assert that the DP in (3b) is a self-contained EL island dominated by a maximal projection.

The authors appear to have the structure in (5) in mind for these phrases.



The question of whether (3) count as counter-examples to the MLF model appears to depend upon whether (4) or (5) accurately depicts the structure of the XP. The structure outlined in (5) fell into disfavor in the mid-1980s with the emergence of the DP Hypothesis (Szabolcsi, 1983; Abney, 1987; Stowell, 1989) which advocated a structure like (4), creating a new functional level above the NP much as previous researchers had proposed CP as a new functional layer above IP. While JMSG<sup>2</sup> are certainly free to reject the DP Hypothesis, thus avoiding problems which (3) raise for the MLF model, independent evidence must be presented to justify this choice in a way which does not neglect empirical consequences for the original motivation for the DP Hypothesis. Rather than taking up the matter, however, the authors simply present (5) as the standardly accepted view of the structure, attributing it to an unnamed "familiar syntactic textbook" (p. 272).

JMSG<sup>2</sup> also dispute the counter-examples in (6), in which late outsider system morphemes are contributed by both English and Spanish. These data are due to grammaticality judgments of simultaneous Spanish-English bilinguals living in Central Arizona.

- (6) a. ¿Tus *coworkers* no tuvieron vacaciones  
 your coworkers no have/3P/PAST vacation-PL  
 todavía, verdad?  
 yet right  
 "Your coworkers haven't had a vacation yet, right?"
- b. Tus *coworkers* *haven't had a vacation yet, right?*  
 your coworkers haven't had a vacation yet right  
 "Your coworkers haven't had a vacation yet, right?"

In the case of (6a), JMSG<sup>2</sup> assert that *-s* is not a late outsider system morpheme. However, note that *-s* enters the syntax already affixed to its noun *coworker*, and must establish an agreement relation with the verb, an element that is outside its maximal projection; *-s* therefore "looks outside its own immediate maximal projection for information about its form", and is therefore indeed a late outsider system morpheme.

JMSG<sup>2</sup> acknowledge that (6b) is problematic for the MLF model, apparently accepting that *tus* counts as a late outsider system morpheme. However, they "question the veracity" of the example because they find no examples

in their corpus of a Spanish determiner with an English noun in an otherwise English clause (p. 273). The corpus contains 230 NPs from ten bilingual immigrants said to be “fluent in both Spanish and English” and working or attending graduate school in the U.S. (JMSG<sup>1</sup>, p. 80). The sample is very small for the number of consultants, averaging only 23 items per speaker; hence, we might reasonably expect a larger sample or one collected under different circumstances to obtain different results. The predominance of one language over the other could be influenced by topic, social context, peer and identity factors, the nativity status and linguistic ability of participants, or other variables. Although all data should be critically interpreted, I personally do not believe the absence of predominantly English sentences in this small corpus suggests that the grammaticality judgments given for (6b) are erroneous.

Although JMSG<sup>2</sup> do not address them (presumably due to space limitations), there are other counter-examples to the MLF model discussed in MacSwan (2005). Of particular interest are those which demonstrate that the MLF model sanctions as well-formed numerous ill-formed examples, primarily due to the generosity of the EL island provision. Consider, for instance, (7), in which an embedded VP EL island fails to insulate the utterances from ill-formedness (see MacSwan, 2005, p. 10).

- (7) a. \*El no *wants to go*.  
       he not want to go  
       “He doesn’t want to go.”
- b. \*He doesn’t *quiere ir*.  
       he doesn’t want/3SS go/INF  
       “He doesn’t want to go.”

5. A central concern in JMSG<sup>1</sup>, MacSwan (2005) and JMSG<sup>2</sup> is the question of whether the basic thesis of JMSG<sup>1</sup> can be sustained. JMSG<sup>1</sup> claim that a Minimalist approach to CS “may even only succeed partially if it incorporates” the ML/EL distinction. As a proof of concept, the authors offer an analysis of a corpus of Spanish–English DP data. My response demonstrated that the very analysis of JMSG<sup>1</sup> fails to make use of the ML/EL distinction, and I presented an analysis of the DP facts which explains the data with reference to independently established principles of syntax alone and ignores the ML/EL distinction.

The analysis, based on Moro (2001), notes that the ungrammatical cases (e.g. \*the *casa*, “the house”) involve Ns whose  $\varphi$ -set is not included in the  $\varphi$ -set of their Ds. The  $\varphi$ -set is the set of  $\varphi$ -features, namely, *person*, *number* and *gender*. Ungrammaticality results when Spanish N’s  $\varphi$ -features *person*, *number* and *gender* attempt to value English D’s  $\varphi$ -features, only to find that English D has only *person* and *number*. As a result, D’s  $\varphi$ -features cannot be valued and the derivation crashes. Examples like *los*

*teachers*, on the other hand, are well-formed because the  $\varphi$ -set of N ( $\varphi = \{person, number\}$ ) is included in that of D ( $\varphi = \{person, number, gender\}$ ); N’s  $\varphi$ -features therefore value D’s in “one fell swoop” (in the sense of Chomsky, 2000).

JMSG<sup>2</sup> erroneously characterize this approach as “based on counting  $\varphi$ -features” (p. 274), as though the strategy consisted in counting the  $\varphi$ -features of N and comparing them to the number in D. Were this the method, CS would be disallowed between Spanish D and English N as well as between English D and Spanish N. As counter-evidence, JMSG<sup>2</sup> identify a small number of extant switches between English Ds and Spanish Ns, predominantly in written texts. Though small in number, written examples of CS are naturally worthy of investigation; an analysis of the phenomenon might additionally consider how reading and writing interact with the grammar of CS, as informed by a psycholinguistic theory. But given the very low incidence of these forms in naturalistic corpora (Moyer, 1993; JMSG<sup>1</sup>; data referenced in JMSG<sup>2</sup>) and reports of negative grammaticality judgments (Lipski, 1978), I will for the time being assume them to be ill-formed.

Of potentially more substantial consequence, JMSG<sup>2</sup> present Italian–German data in which Italian Ds are reported to occur before German Ns; indeed, Cantone and Müller (in press) report among very young simultaneous bilinguals that Italian and German are freely mixed in these contexts. Applying the same method of analysis, we would expect German–Italian DPs to be ill-formed if and only if the  $\varphi$ -set of N were not included in the  $\varphi$ -set of D. Since German and Italian have the same array of features (*person*, *number*, *gender*), the derivations succeed, as Cantone and Müller discuss. (Since D’s features enter the syntax unvalued, differences between possible feature values for German and Italian are irrelevant.)

JMSG<sup>2</sup> conclude that the feature checking analysis which I present predicts that CS would not occur between German and Italian because German determiners “are marked not only for person, number and gender, but also for CASE” (p. 274), and therefore the counts do not match. However, CASE is not a  $\varphi$ -feature (only *person*, *number* and *gender* are), and counting of  $\varphi$ -features is not relevant to the analysis to begin with, so the argument fails. JMSG<sup>2</sup>’s proof of concept likewise fails, as we have shown that the ML is dispensable for the analysis of CS data, leaving parsimony with the task of disposing of the ML/EL distinction along with other CS-specific mechanisms. It follows that there is no ML.

6. Although JMSG<sup>2</sup> recognize that they had misread Hlavac’s (2000) account of data construed as counter-evidence to the PF Disjunction Theorem, they nonetheless assert that Hlavac (2000, 2003) “makes abundantly clear that phonologically unintegrated English words are the norm” (p. 275). However, from his corpus of 148,000

words from 100 Croatian–English bilinguals, Hlavac (2000, 2003) presents only four unambiguous counter-examples to Poplack's Free Morpheme Constraint (the kind of items which would also count as counter-examples to the PF Disjunction Theorem) (Hlavac, 2003, p. 164). Two of these examples involve CS between a Croatian stem and English genitive *-s*; because genitive *-s* is an XP rather than X<sup>0</sup> level affix, these constructions are not predicted to be ill-formed by the PF Disjunction Theorem (see MacSwan, 2005, p. 14). Space limitations prohibit discussion of the remaining two examples, but 2/148,000 (= 0.001%) hardly constitutes a “norm”.

The approach advocated here suggests that CS data may be explained by relying upon independently motivated principles of grammar, with no CS-specific mechanisms required. This approach explores the consequences of our theories about linguistic variation in a dynamic way. By applying precisely constructed linguistic analyses to bilingual data, we aid “the process of discovery itself”, and “[b]y pushing a precise but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy and, consequently, gain a deeper understanding of the linguistic data” (Chomsky, 1957, p. 5). We have learned that across-the-board constraints on CS are inadequate; analyzing CS data without them appears to be a promising alternative.

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