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AUTHORS' RESPONSE

What counts as (contact-induced) change

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We are most grateful to our commentators for their careful reading of our Keynote Article (henceforth KA) and their incisive observations on contact-induced change, and for the many challenging and thought-provoking issues they raise. We welcome the opportunity to respond to (some of) them, especially since, perhaps not surprisingly, these are symptomatic of the very issues in the field of contact linguistics that prompted us to write this KA in the first place.

In this response paper, the following abbreviations are used to refer to the commentary authors: GAK = Georg A. Kaiser, ME = Martin Elsig, PM = Pieter Muysken, RO = Ricardo Otheguy, RTC = Rena Torres Cacoullos, YR = Yves Roberge. Full bibliographic details for the commentaries and the keynote article will be found in references at the end of this paper.

Issues of method

We are delighted with the commentators' evaluation of our methodology as "masterly" (RO), a "milestone" (RTC), "outstanding" (GAK), of "tremendous usefulness" (YR), and displaying "extraordinary empirical accountability" (ME). All of them profess acceptance of our conclusion that convergence had not taken place, calling it "strongly warranted" (RO; ME; GAK), and based on "convincing evidence" (RO; GAK). But despite acknowledging the validity of our analysis, by way of rebuttal, several of them (i) offer examples of convergence occurring elsewhere, in the same construction (YR) or in the same relative clause context (PM), (ii) hypothesize that convergence COULD HAVE occurred here (PM, RO, YR), and (iii) theorize as to why it DID NOT (YR, PM).

These observations betray the persuasion that convergence is such a foregone conclusion of language contact that its very absence requires explanation (theorizing, experimentation). The point of this KA was to try to undermine that preconceived notion by bringing objective measures to bear on the assessment. In the absence of such measures, the provenance of linguistic features in languages in contact must remain a matter of opinion, or, as RO puts it, conviction. We reiterate, along with GAK, ME, RTC and Poplack and Levey (2010), that given the availability of synchronic data and accountable methodology, the burden of proof rests with those who claim that convergence did occur. As we explain below, we see no sign of such proof in those commentaries on the KA.

Convergence occurred elsewhere

Characterizing relative clauses as a "vulnerable area for language contact", PM lists four cases (in addition to the one we discuss in the KA) in which contact has been claimed to have an effect on some element within them. The subtext here is that if contact-induced change had occurred in Konkani, Turkish, and Southern Peruvian Quechua relative clauses, it should also have occurred in their Quebec French (QF) counterparts. But this does not follow. At the current state of our knowledge, we are very far from being able to identify a linguistic feature that can be predicted to change in all situations, contact or other, regardless of language pair, intensity of contact, socio-cultural situation, bilingual proficiency, and a host of other intervening variables. On the contrary, it is far more likely that change in one bilingual community will have no influence on another. Why should it?

In fact, PM himself proceeds to express reservations about the evidence brought to bear on three of the four changes he discusses (echoing, apparently, the authors themselves). He concludes from the five examples that "the strength of the evidence for contact-induced language change in relative clauses varies". It has been accepted in Konkani, according to him, but not (as a sole explanation) in the case of other languages. What makes the Konkani case admissible, and the others questionable? There is no difference in the QUALITY of the evidence, as far as we can tell. Absent the requisite demonstrations and proofs, in such cases, the reader is left to decide on faith alone whether or not convergence has occurred.

While also accepting our analysis of the QF bare prepositions studied in the KA, YR maintains that convergence involving this same structure is "not only hypothetically possible but also attested" in other

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varieties of North American French. We agree that it is hypothetically possible; that hypothesis motivated this study. But the method we have proposed and the (variationist) framework within which we operate dictate that we go beyond the two criteria invoked by YR: attestation of bare prepositions (a) with semantically weak prepositions (*à*, *de* "to, from"), and (b) in "true extraction contexts" (pseudo-passives and *wh*-interrogatives).

Consider first the weight assigned to ATTESTATION as opposed to DIFFUSION. Bare semantically weak prepositions are also attested in our data . . . twice (KA, example (15)). But when we situate these two attestations with respect to all the contexts where weak prepositions could have occurred but did not (N = 260), we observe an overall rate of occurrence below 1%. Does this count as change? The PROVENANCE of the attestations invoked by YR must also be taken into account. Did they occur in spontaneous speech, and if so, how frequently? Or were they culled from speaker judgments? If so, how many, and how consistent were they? What is the relationship between grammaticality judgments and usage (a question YR himself touches on)? And what is the relationship between the behaviour of an individual and that of the community? The answers are crucial, because language change, contact-induced or otherwise, does not reside in the head of one individual; rather, as noted by RTC and ME, as well as in the KA (under section heading: "Preposition placement in the English of bilingual francophones"), it arises and spreads from regular interaction in the speech community.

Moreover, even if weak prepositions did represent a substantial proportion of those occurring bare in some variety of North American French, and even if they occurred freely in pseudo-passives and *wh*-questions, we would argue that this is still not unambiguous evidence of convergence, as discussed in the KA. RTC also observes that surface similarities may mask underlying differences; it would thus also be necessary to demonstrate that the weak prepositions behave in the same way as their putatively borrowed counterparts in the source variety (criterion (iv) in the KA, under section heading: "Introduction"), and differ in non-trivial ways from the superficially similar construction in the host language (i.e. orphaning; criterion (v) in the KA). In both connections, to qualify as stranded, they should be used according to English grammar, and not as in French, i.e. weak and strong prepositions should not be distinguished according to semantic weight. The existence of such patterning cannot be ascertained from isolated attestations. This brings us back to the emphasis placed in the KA on CONTEXTUALIZING the candidate for convergence with respect to the rest of the linguistic system into which it is incorporated. As ME points out, the actual role a particular variant plays can only be understood when contrasted with its main opponents. The EXISTENCE of a form cannot be equated with its BEHAVIOUR.

Why didn't convergence take place here?

In the same vein, each of PM, YR and RO speculate as to why convergence did not occur in the situation we studied. PM conjectures that while relative clauses are vulnerable, preposition stranding itself may be immune to contact-induced change, without suggesting why this might be. Given the state of our knowledge, we cannot speak to these conjectures beyond the present KA. He also questions whether the SALIENCE of the construction causes speakers to avoid convergence. Studies of speech communities show that salience need not constitute a deterrent to using non-standard features; witness the fate of the conditional in protases of hypothetical *si*-clauses (*si j'aurais su* "if I would have known"; LeBlanc, 1999; LeBlanc & Poplack, 2003). Despite centuries of prescriptive opprobrium and overt correction, it is not only increasing rapidly, ousting the standard imperfect, but its selection is not even sensitive to style shifting. In any event, recall that speakers are NOT avoiding bare prepositions; they are simply using them at the same rate (i.e. approximately 10% of the time), and in the same way as they use native French orphans (i.e. implicating the same small cohort of lexical prepositions, and distinguishing them according to semantic weight).

YR proposes two mechanisms to explain why preposition stranding has arisen elsewhere: borrowed prepositions brought with them the property of stranding, and contact with English triggered reanalysis elsewhere in the grammar. But these mechanisms do not necessarily give rise to the same outcome in other ostensibly similar situations. For example, in the variety of QF studied in the KA, English prepositions are also borrowed (KA, under section heading: "Preposition placement in the English of bilingual francophones"), but, as we have shown, WITHOUT the property of stranding. Louisiana French displays a different pattern from Prince Edward Island French (King, 2000; King & Roberge, 1990), although both are varieties of Acadian French, and arguably in equally intense contact with English. In Louisiana French, bare prepositions are attested in more environments than in QF, including those that YR considers crucial to establishing English influence, but they continue to adhere to the French semantic weight constraint (Kevin Rottet, p.c.). To fulfill this requirement, a dual strategy is employed: weak *de* is replaced by a strong counterpart (usually *après*/*pour*), as in (1), while weak *à* tends to be absorbed. Has convergence occurred here?

(1) l'homme que je rêvais **après/pour**
the.man who I was.dreaming after/for
< *l'homme que je rêvais **de**
"the man I was dreaming of"

Convergence could have occurred here: Hypotheses and facts

PM and RO further intimate that English influence could be operating, but methodological shortcomings on our part obscure its effects. PM suggests that our reservations about convergence might be attenuated if we had participated in the "growing trend in typology to consider areal explanations for the distribution of linguistic features". Just what constitutes a LINGUISTIC AREA has long been controversial in historical linguistics (Campbell, 2006), but a recurrent theme is that the genetically unrelated but geographically contiguous languages located within it share structural features as a result of contact. The national capital region of Canada (described in detail in Poplack, 1989) was specifically selected as a research site because it harbors two geographically contiguous languages of different immediate language families that have been in contact for about two centuries. It therefore fulfils the external preconditions for a linguistic area. The research presented in this KA constitutes the legwork required to determine whether it can be thus qualified on linguistic grounds too, i.e. whether the languages do in fact share structural features as a result of contact. Areal linguistics is the study of the RESULTS of contact, and the notion of linguistic area is not an explanation, but rather a (post facto) description. Campbell (2006) argues persuasively that the processes leading to its formation are none other than those involved in borrowing. In any event, we submit that this is the kind of work that must be done on contact languages SYNCHRONICALLY in order to decide whether they constitute a linguistic area. On the basis of the behaviour of the bare prepositions we study here, the Ottawa-Hull region is not one, bolstering rather than diminishing our reservations.

PM also suggests that the central mechanism for change may be cross-linguistic priming, rather than the code-switching "hinted at") in our KA. But since the bare prepositions in QF could have been primed by orphans OR strandeds, we are still left with the task of determining which. Granted, the PROPORTION of stranded prepositions in English is exponentially greater than that of orphans in French, but relative clauses in running speech are still quite rare in both languages (KA, under section heading: "Stranding", fn. 9). We can therefore expect previous mentions capable of priming to occur at a far remove from the target. So we would want to know much more about the latency of priming (i.e. how close the prime has to be to activate the bare preposition), before implicating it in an analysis. In any event, since we have documented no change, there is no need to speculate about the mechanism, or to undertake further experimental work, as suggested by PM.

Frequency is in the eye of the beholder

RO cites vagaries in our interpretation of the facts, abetted by (a) dismissal of the evidence from rates, that, according to him, points to the inference of convergence, (b) concomitant exaggeration of the importance of conditioning, which supports the analogy analysis, and (c) a preconceived determination to favour an internal explanation (analogy) over an external one (contact).

The cornerstone of RO's argument, we think, is the assumption that preposition placement in the ancestral variety (Metropolitan French) differs from the contact variety (QF), consonant with the received wisdom that transplanted varieties/varieties in contact will diverge from the ancestor that remained in situ. This is suggested by his estimates of their respective stranding rates, which in his characterizations range from "frequent", "high frequency", a "new high" for QF, to "the FACT (emphasis added) that French–English bilinguals show some of the highest rates of stranding in the Francophone world". This despite our demonstration that the actual incidence of bare prepositions in the data is in fact far inferior to popular opinion (including, apparently, that of RO), at 12%, and on a par with that of the native orphaning (10%). This rate is qualified by RTC as LOW. In any event, no one, RO included, KNOWS what the rate of bare prepositions in Metropolitan French is, since to our knowledge, there has been no quantitative study of actual speech (although of course, as also observed by both GAK and RO, the very fact that they are sanctioned prescriptively points to their existence there (KA, under section heading: "Preposition placement in a pre-contact stage")). The unwarranted inference that the QF rates represent a change, qualitative or quantitative, motivates the quest for locating its trigger.

Even if, for the sake of argument, we could document substantive rate differences, we would still be unable, ON THE BASIS OF SYNCHRONIC EVIDENCE ALONE, to determine which variety had changed, unless we adopt a priori the twin assumptions that (a) the offshoot diverges from the ancestor that remained in situ, and (b) contact leads to change. The fate of direct question formation studied by ME (Elsig, 2009; Elsig & Poplack, 2006) illustrates how misleading these assumptions can be. Contemporary Metropolitan French differs massively from this same variety of contemporary QF in forming almost all *yes*/*no* questions in the same way (i.e. with rising question intonation: *Tu veux venir?* "You want to come?"). This variant accounts for no more than 35% in QF. But analysis of the pre-transplant stage revealed that the earlier multi-variant system of 17th century French had been RETAINED in Canada; contra the received wisdom, it was the repertoire of Metropolitan French that had changed.

In the absence of useful information on the rates of bare prepositions in the ancestral source, we would venture to suggest that a more (the most?) pertinent distinction between Metropolitan French and QF is the CONVICTION of French Canadians that bare prepositions come from English, and the (not unrelated) stigma they attach to them (nicely illustrated in PM's opening paragraph). Indeed, such is the linguistic insecurity surrounding this construction in francophone Canada that native orphaning is popularly assumed to have come from English too. (We suspect that the reason more Google hits for *la fille que je sors* "the girl I go out with" come from Canadian sites is because most of the mentions are metalinguistic complaints about it. We have not turned up any of these emanating from Europe.) Again, for linguists and laypeople alike, the default assumption is that where a pair of languages are in close geographic proximity, the minority member will change.

RO further queries why we privilege the 10% orphaning rate as a model for stranding in QF, when the massive (98%) stranding modeled by English could as easily have acted as the trigger for a still limited incursion into French. RO is of course correct that stranding could have come from either source. We undertook this study to determine which. But, as ME's aforementioned work on question formation illustrates, rates are ultimately silent as to directionality. The lack of evidence that could help us decide in a principled way between the two (equally reasonable) scenarios is at the root of our reluctance to base conclusions on rates alone. Here we draw on rates IN CONJUNCTION with the conditioning of bare prepositions, i.e. the way they are USED in bilingual discourse. This turns out to be manifestly different from the English pattern and undeniably similar to the native recipient-language grammar of orphaning. Had the analysis of conditioning showed bare prepositions in ALL permissible English environments, even if at a lower rate, a contact explanation would have been warranted. Here the facts that the specific lexical prepositions that appear bare in French relative clauses are the same ones that orphan, and are mediated by the same semantic weight constraint – both conditions absent from English stranding – point to a French model. This, in conjunction with the finding that the same speakers adhere to this French model while speaking French, and to the English model while speaking English is, to the minds of ME and RTC, as well as our own, the strongest evidence in favour of the analysis presented in the KA. We invite RO to offer an alternative explanation for these inconvenient facts.

RO also argues for a convergence scenario by asserting that the differences between the structure of French and English bare prepositions are only "lexical", whereas those between "stranding" and orphaning in French are "syntactic". According to him, a syntactic barrier should be more difficult to breach. Yet even cursory examination of the CONDITIONING of preposition placement makes clear that the differences between French and English go far beyond the lexical. As detailed in the KA, there are important syntactic differences (French only strands in relative clauses headed by *that* and zero, whereas English also strands in *wh*-complexes and in pseudo-passives), differences in frequency (categorical or near-categorical stranding in *that*/zero relative clauses and *wh*-constructions in English vs. only 12% overall in French), and differences in variant inventories, in addition to lexical restrictions on bare prepositions and relativizers in French, but not in English. In fact, the only feature SHARED by French and English is the surface identity of the bare preposition, whereas French bare prepositions in relative clauses and French orphans also share the same rate AND conditioning.

Thus the answer to RO's query as to why we "privilege" analogy over contact is not because we have independently and arbitrarily adopted this stance. It is because the results point to an internal explanation. Had the results gone in the opposite direction, we would have concluded in favour of contact. RO's critique fails to acknowledge that the method we proposed (and applied) here incorporates all of the competing hypotheses. Testing them removes speculation and theoretical leanings from the picture and replaces them with findings. This KA is not about what could have happened, imponderable in any event. It is about what did (and did not) happen to one linguistic variable in one situation of intense long-term contact, and it lays down explicit criteria for deciding whether what could have occurred in other contexts did in fact occur here.

Does contact trigger change?

This having been said, RO, YR and RTC all raise the important issue of whether contact, even if not implicated directly, may play some ancillary role, by triggering and/or accelerating internal change. Certainly many scholars of bilingualism assume that it does. But the mechanism by which this could transpire remains unexplored. It is not possible to reconstruct the act of triggering. And we cannot determine whether contact accelerates change when we do not (and cannot) know what the rate of change would have been in its absence. So what kind of accountable methodology can we marshal to address this question? Scholars who have attempted to deal with this issue have enlisted intervening external variables.

The external measure of degree of contact adopted here, as well as in Torres Cacoullos and Travis (2011), is rate of code-switching. Elsewhere, it has been operationalized as bilingual ability (Lealess & Smith, 2011; Mougeon & Béniak, 1991; Poplack, 1997). Otheguy, Zentella and Livert (2007), Budzhak-Jones and Poplack (1997) and Silva-Corvalán (1994), invoked speaker generation (first or second). Recency of arrival into the contact situation has also been measured (Otheguy et al., 2007). The (implicit) assumption is that if contact is a motivating force, such external measures must play a distinguishing role.

The measure tested in the KA (rate of code-switching) revealed no effect. And we generally do not find one elsewhere either. But this is an issue that merits much further exploration. We thank the commentators for raising it and hope that it will spur further discussion on ways to measure the triggering or accelerating effect of contact on change.

What counts as change? An excursus on rates vs. conditioning

An all-encompassing question is what counts as change? RO charges us with treating rates of occurrence as epiphenomenal and irrelevant to understanding change, a stance he characterizes as a "conviction" and a matter of "principle". This is a straw man. Nowhere in the KA, or elsewhere, did we "dismiss" the role of rate differences in change. It would make no sense to do so, since change by definition is the gradual replacement of one or more competing variants by another. As some variants are admitted into more environments and others are eschewed, their relative rates of occurrence will be altered. This is the nature of change, and, as observed by RTC, evaluating it requires quantitative argumentation. In the earliest stages of the spectacular change in the Brazilian Portuguese future paradigm studied by Poplack and Malvar (2007), the synthetic future and *haver*-periphrasis variants together accounted for 96% of all references to the future. By the 20th century, they accounted for 1% in speech, having been virtually ousted by the incoming periphrastic *go*-future (85%). There can be no doubt but that this is a change.

To the extent that we know what the earlier stage was like, there is usually a fair amount of agreement about changes that have gone to completion. In the Brazilian Portuguese case, for example, it is plain to see that we now have one variant where before there were three. But there is typically a long transition period during which competing variants jockey for position in the system before a change goes to completion. Synchronically, this is manifested as variability. As RTC reminds us, variability is a necessary precursor to change, but need not in itself constitute change. Witness the many cases of stable variability that have perdured over centuries with no sign of going to completion (such as the case of English double negation cited by RTC, among many others).

What is contentious in studies of convergence are CHANGES IN PROGRESS. These are the ones that are detected synchronically, in apparent time, as instantiated by different generations of speakers. Variability in apparent time is what most contemporary reports of contact-induced change are based on (when any data is brought to bear at all). Generally, differences between cohorts (older and younger, more and less bilingual, etc.) in rates of occurrence of some variant are construed as changes. What counts as change? RO suggests that we should privilege rate differences, and ignore conditioning or treat it as secondary.

In this connection, recall that the bulk of our analysis is in fact based on rates. We compare occurrence rates for each of the four French strategies (KA, under section heading: "Multivariate analysis of the contribution of factors to preposition placement strategy", "Factors conditioning the selection of orphaning"), we compare rates of bare prepositions in weak and strong categories (KA, under section heading: "Stranding"), and among copious and sparse code-switchers (KA, under section heading: "Code-switchers as agents of convergence"). Because (as RO deduced), the lack of variability renders multivariate analysis otiose, the entire analysis of English preposition placement is based on rates (KA, under section headings: "Comparing preposition placement in source and host: English vs. French", "Lexico-semantic conditioning of variant choice" and "Preposition placement in the English of bilingual francophones").

Nonetheless, frequency differences can be "spurious and misleading", as RO himself admits. This is because they may be epiphenomena of other differences, and not indications of change. For instance, they may be masking regional differences, as in Otheguy et al.'s (2007) report of Spanish overt subject marking ranging from 19% to 41% depending on the speakers' national origin. Alternatively, they may be a function of genre, style or situation. For example, higher rates of Spanish subject-pronoun expression in Colombian Spanish conversational data than in New Mexican narratives cited by RTC are due to the prevalence of topic shifting in conversation, in conjunction with the subject continuity constraint, which favours subject expression when the preceding subject is non–co-referential. These rates are a function of a genre-driven distributional difference. Another such case involves expression of the negative particle *ne* in 19th-century French. Martineau and Mougeon (2003) report a retention rate of up to 60% in informal WRITTEN documents, in stark contrast to the 0.1% found by Poplack and St-Amand (2007) in SPOKEN French of the same period. A more subtle, but quite common, source of frequency differences is a fortuitous preponderance or dearth in one data set of some very propitious context. For instance, Poplack (1997) found that apparent differences in rates of subjunctive use across bilinguals were actually due to uneven distributions across proficiency cohorts of extremely (un-)favourable verbal and non-verbal matrices. Likewise, Dion and Blondeau (2005) show that the strong negative correlation between fluency in L2 and choice of the inflected future was in fact due to a coincidental scarcity of the most favourable negative contexts for the inflected future in the speech of individuals with the lowest L2 proficiency. Finally, RTC discusses intra-linguistic rate differences with no associated difference in conditioning, and inter-linguistic differences in conditioning where rates were the same. This brings us back to the question of what we want to count as change.

The rate comparisons made in the KA plainly align French bare prepositions with orphans and copious code-switchers with sparse code-switchers. As such, they are consistent with the comparisons of conditioning that we also carry out. Where rates do not agree with conditioning, however, we have found RATES TO BE LEAST EXPLANATORY, AND CONDITIONING MORE REVEALING OF THE LINGUISTIC SYSTEM. This is the context of the following observations made by Poplack and Levey (2010), which RO unaccountably interprets as a dismissal of rates: "differences in rates of variant selection must be used with caution to infer change, contact-induced or otherwise" (p. 400), "differences in overall rates of variant occurrence NEED NOT [emphasis added] be indicative of change, contact-induced or otherwise" (p. 404), and "differences between cohorts or overall rates may be masking other effects that are independent of the contact situation" (p. 409). We stand by these caveats. But we stress that one line of evidence need not be discarded in favour of the other. Rather, we should adduce as many converging lines of evidence as possible. The real question here is not whether rates outweigh conditioning (or whether we should limit ourselves to only one of the two), but how we construe change. We may want to revisit our working assumption that we all agree on what it is.

Other methodological issues

External measures of contact

ME questions our choice of external measure of contact, seeing no "causal connection ... between bilingual fluency and the vulnerability of the participating languages to contact-induced change". He also qualifies measurement of bilingual fluency by the number of switches produced as "arbitrary", "*ad hoc*" and "not linguistically motivated". We agree on both counts. But we clarify that here we do not use code-switching as a measure of bilingual fluency. (To fully test bilingual fluency, we would employ a more holistic measure, e.g. the CUMULATIVE BILINGUAL PROFICIENCY INDEX (Poplack, 1997, p. 307), which takes into account a much wider variety of uses of the two languages.) Rather, our selection of propensity to code-switch as a measure of contact was dictated solely by our goal of putting to empirical test the widespread belief that code-switching leads to structural borrowing (outlined in Section 1.1 of the KA, and illustrated in issue 9 (3/4) of *International Journal of Bilingualism*). We ourselves voiced skepticism in the KA (under section heading: "Discussion"), as did Torres Cacoullos and Travis (2011), about the mechanism by which copious code-switchers could act as agents of change, when their code-switching behaviour is overwhelmingly constrained by linguistic conditions that respect the different grammaticality requirements of both languages. In terms of distinguishing sparse from copious code-switchers, we agree with ME that our cutoff (as any other) is equally arbitrary. We reiterate our caveat (KA, under section heading: "The contact situation") that the labels are RELATIVE only. Nonetheless, as detailed in Zentz (2006), a principled selection of speakers was made to represent both polar opposites in terms of code-switching rates.

The use of prescriptive rules

GAK contends that "prescriptive rules are without any relevance for a study on putative convergence" and wonders why we would we give any weight to the fact that stranding is prescriptively unacceptable. We agree with GAK, and in fact have shown (Poplack & Dion, 2009) that it would be unwise indeed to take prescription to be a reflection of usage, given the huge disconnect between them. Rather, we invoked prescriptive dictates (KA, examples (4) and (24)) only to illustrate that in French, (i) bare prepositions are censured in relative clauses, no doubt contributing to the perception of English provenance, and (ii) bare prepositions are permissible in orphaning contexts. Older grammars (KA, example (31)) confirm the existence of the contentious structure in Metropolitan French since, as GAK correctly points out, the very mention of it reflects its presence in the ancestral source variety. Our choice of *Le bon usage* specifically, albeit "more descriptive in nature than prescriptive" (GAK), was motivated simply by the fact that it is considered THE authority on French grammar.

Criteria for concluding in favour of convergence

GAK inquires whether the five criteria presented in his (1) are ordered hierarchically, and whether all need to be fulfilled in order to conclude that no change has taken place. We should clarify that these are the criteria necessary to conclude IN FAVOUR of change. If any one of them can be shown not to hold, a feature cannot be the result of contact-induced change, if only for reasons of logic (i.e. if there is no change, if the candidate for convergence was present in an earlier or non-contact variety, if it behaves differently from its counterpart in the source variety, or if it behaves in the same way as its counterpart in the host).

The use of comparative sociolinguistics for the study of convergence

RO questions the utility of comparative sociolinguistics for the study of convergence, alleging that the lack of multivariate analysis of English preposition placement may have "contributed to an exaggeration of the differences between English and French". To this we can only reply that analytical tools must be appropriate to the data. If there is (virtually) no variability (due to the near-categorical selection of stranding in English noted by RO himself), it is neither necessary nor useful to employ variable rule analysis. The robust variability in French, on the other hand, lends itself well to it. This in and of itself speaks to the differences between the languages. In any event, the factor weights issuing from multivariate analysis are not the only points that can be compared. Here we employed the same method to compare distributions across contexts, which are reflections of constraint hierarchies. Indeed, the criteria we have imposed for the establishment of convergence are built on such comparisons. We thus take exception to RO's claim that comparison "is not available" in the case at hand.

Variable context for orphaning

GAK counters (what he takes to be) our assertion that orphaning does not occur in English with the example in his (5). It is true that some English prepositions (e.g. *in*, *on*, *inside*, [temporal] *before*, and with a few verbs, *with*) are intransitive, where the object of the preposition is implicit (KA, under section heading: "Comparison with a native French model: Preposition orphaning"). And as in French, these are lexically or idiomatically specific (*put it on*, but not **the book is on*). However, the counterparts of the French prepositions that tend to orphan are generally not admissible in English. The bare prepositions occurring in French relative clauses, on the other hand, are a much closer match with those occurring in French orphaning contexts.

We also thank GAK for pointing out that our statement that "[o]rphaning does not occur in relative clauses" (KA, under section heading: "Comparison with a native French model: Preposition orphaning"; GAK) is not entirely correct. We should have specified that it does not occur in OBJECT relatives. His example (6), like example (2) from our own corpus, both subject relatives, do indeed contain orphan prepositions, and were coded as such.

(2) *Si c'était à revenir aujourd'hui, c'est moi qui les mettrais à genoux, puis c'est moi qui les taperais* ***dessus*** *[O], tu sais?*

(OH.03.649)

"If it were to happen again today, I'm the one who [lit. 'it is I who . . . '] would bring them to their knees, and I'm the one who would hit **on**."

Future directions

We close by summarizing what this KA is (not). It is not a repudiation of contact-induced change. We see no reason to reject it either in theory or on principle. But we do agree with ME and Thomason (2001) that for many reports of contact-induced change, conclusive proof has been lacking. For instance, we are cognizant that much of the relevant evidence can no longer be reconstructed for changes gone to completion in the remote past; this KA is silent with respect to those. Our intent is thus not to "reject all the research by historical linguists on contact-induced change", as Thomason (2011, p. 146) charges. Instead, intrigued by the extraordinary number of changes reported to have resulted from CONTEMPORARY contact situations, often in time spans as short as a generation, we have sought to develop criteria that would substantiate the case for convergence. This KA is an operationalization and application of these criteria to one salient and stigmatized grammatical construction, used in a situation of intense and long-term contact between French and English, and widely thought to have arisen from such contact. The goal was to develop a method to pursue this inference systematically, and use the results of the analysis to decide amongst alternative hypotheses.

Now, although we emphatically do not reject the concept of contact-induced change outright, PM is right to say that we are skeptical about it. We stress that this is not due to personal inclination or theory-driven dictates, but rather to the weight of the empirical evidence. In applying our own strict criteria to a large number of candidates for convergence culled from the speech of large numbers of bilingual speakers, in large-scale community studies we have carried out under circumstances most propitious to change, we have failed to find unambiguous proof of it. On the contrary, when subjected to detailed empirical analysis, many of the non-standard features that initially appeared to be changes turned out not to be changes or not to be contact-induced (e.g. Dion, 2003; Leroux & Jarmasz, 2006; Poplack, 1997; Poplack & Levey, 2010). On the other hand, most of the myriad reports of change, including those offered by PM and YR, do not enjoy the same empirical proof.

We hope that the discussion engendered by these stimulating commentaries will deflect attention away from the questions of why convergence did not take place and whether it could have, and channel it towards the more pressing problems of whether it did take place and by what mechanism. For this program to go forward, we need to establish what counts as (contact-induced) change.

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