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AUTHOR'S RESPONSE

Bi- and multilingualism as a metaphor for research

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I would like to begin by thanking the editors of the journal, and in particular Carmen Silva-Corvalán, for inviting such a wonderful group of commentators, united in their commitment to spirited yet courteous academic debate and diverse in their academic allegiances, theories, and methodologies. I also thank the commentators for the kindness and generosity of their comments, which offered rich material for reflection. Reading the comments for the first time, I became concerned that a coherent response that addresses the Emotion Stroop Effect in the same breath as the textual explorations of the "third space" may be all but impossible. I thought about the resemblance between the academic enterprise and the phenomenon of bi- and multilingualism, with academic paradigms establishing their own regimes of truth and creating languages that are often mutually incomprehensible. For some, true answers can only come from quantifiable empirical data, others look for veracity in texts and personal testimonies. Some take for granted that "monolingual controls" are a necessary component of solid research on bilingualism, while others doubt the meaningfulness of such a comparison, reminding us that languages are artificial constructions (Makoni and Pennycook, 2007) and questioning the very existence of crosslinguistic influence (Hall, Cheng and Carlson, 2006). And so we proceed with our parallel lines of research inspired by similar questions about the human condition and yet carried out in ways so distinct as to be incompatible.

But are we doomed forever to speak past each other? A closer reading of the comments suggests that the twain may meet after all. The psycholinguistic notion of context availability intersects in interesting ways with Bakhtinian heteroglossia and multivoicedness, and so do conceptual convergence and the notion of the "third space". Experimental psychologist Catherine Harris raises the same issues about the meaning of "word class" as a cognitive linguist Anna Wierzbicka, while Claire Kramsch, a literary scholar, an applied linguist, and a committed poststructuralist, and a neurolinguist Michel Paradis both engage in reflections about universality and diversity of the human experience. The fact alone that a single text could engage scholars from such distinct fields of expertise suggests that a meaningful interdisciplinary dialog is not impossible. Then, perhaps all we have to do is to get together around the same table, be it real or virtual? In my view this is a necessary but not a sufficient condition for true interdisciplinarity. Rather, engagement with larger questions of basic research, such as the relationship between language and emotions, requires academic multilingualism, that is the ability to speak and read in more than one "academic language", the ability to translate between them, and a willingness to interact with speakers of "other languages". And it is this fluent academic multilingualism that all of the commentators display to perfection.

I too attempt to live as an academic bilingual, writing on issues of language and identity one day, carrying out experiments on object categorization the day after. On good days (and those are few and far in between), this scholarly bilingualism seems like a strength, because, as Claire Kramsch so perceptively noted, questions coming from one paradigm enrich the work in the other. On bad days (and those are numerous), the dual allegiance to sociolinguistics and psycholinguistics seems like a disabling condition – not unlike bilingualism itself – and one that prevents me from reaching full mastery of either language. And yet – also like bilingualism – this multiple point of view is also a privilege, precisely because it allows one to learn from colleagues working across academic paradigms and fields of expertise and to participate in conversations, like the one on the pages of this journal. I will attempt to contribute further to this conversation by addressing concerns and suggestions put forth in the commentaries.

The first important issue raised in the commentaries concerns definitions of emotion and emotion-laden words and my arguments about their place in the mental lexicon. Harris and Wierzbicka rightly question: What does it mean for a group of words to be considered a separate class of words? What are the unifying principles of such a class? What are the implications of having such classes of words for the architecture of the mental lexicon?

These questions identify the weakest point in my argument, the grouping of emotion and emotion-laden words as a unitary class of words. Following a decades-old tradition in experimental psychology (cf. Anooshian and Hertel, 1994; Brierley, Medford, Shaw and David, 2007), this grouping underscores the link between emotions and the two categories of words, yet I also highlight the differences between the two categories which are rarely taken into consideration. Emotion words, in this view, constitute a more or less unified semantic category, while emotion-laden words represent a heterogeneous grouping

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whose organizing principle is not category structure or boundaries, but a presence of an affective dimension that may in fact be present in all or nearly all content words at one point or another. The commentators agree that the two types of words are distinct, although they may disagree with the proposed definitions. I in turn fully agree with Wierzbicka's argument that defining "emotion words" via words like "affective" that themselves require definition is not an optimal way to proceed.

Now, should both word categories be reflected in the models of the mental lexicon? I have argued positively in the case of emotion-laden words, suggesting that language processing models need to incorporate an affective dimension in order to account for phenomena such as affective priming or code-switching for affective reasons. The category of emotion words, on the other hand, is one of many semantic categories and as such is not necessarily equivalent to larger classes of concrete or abstract words. Altarriba and associates used this category to challenge the commonly accepted concrete/abstract dichotomy, showing that emotion words do not fit neatly in either group. Admittedly, this point could have been made with other semantic categories, such as action verbs or verbs of motion (e.g., *running*, *jumping*), that refer to concrete and easily imaginable phenomena (and thus are not prototypically abstract) on a level of abstraction (and thus are not concrete in the same way as *table* or *apple*). What this research suggests is that the concrete/abstract dichotomy oversimplifies the structure of the mental lexicon, thus, it may be best seen as a continuum rather than a dichotomy, and as one of several organizing principles rather than as a single one.

The second set of issues raised in the commentaries concerns emotion concepts. Kramsch and Dewaele ask: To what degree are these representations shared by speakers of a particular language? Are they affected by individual differences, gender, generation, social class, regional language variation, and so on? How can they be researched? Can they be pinned down in actual interaction? What does it mean to have or not to have a particular concept? Some of these questions already have preliminary answers (see e.g., Paradis's comments), while others might inspire future research. Before this research is to proceed, however, we need to disambiguate the conflation of emotion concepts with pragmatic scripts seen in some of the commentaries.

PRAGMATIC SCRIPTS and speech acts are conventionalized scripts of verbal display of particular emotions. In turn, EMOTION CONCEPTS, in the view adopted here, are prototypical scripts of how certain emotions arise and are lived. These scripts are formed as a result of repeated experiences and involve antecedents known to cause particular emotions, appraisals of these emotions, physiological reactions commonly linked to these emotions, their consequences, and means of regulation and display. The function of pragmatic scripts is to inform verbal behaviors, while the function of emotion scripts is to guide everyday categorization processes, such as inferences that X appears to be in love, Y might become angry if we choose a particular course of action, and Z looks or sounds sad. Displays of particular emotions are an intrinsic constitutive component of these concepts but do not represent the whole.

The conflation of emotion concepts with pragmatic scripts is problematic on two counts. First, it limits our understanding of the cognitive process of emotion categorization, reducing it to interpretation of verbal expressions. In reality, our judgments are not based uniquely on verbal and non-verbal displays, they are guided by the larger scripts that include other components of emotion concepts, such as our knowledge of causality (e.g., rejection of a marriage proposal may lead to disappointment or even anger) or our beliefs about the links between emotions and physiological states (e.g., fluttering signals attraction). As Dewaele points out, the presence of these schemas is skillfully exploited by writers who manage to convey the feelings of their characters without explicitly naming them. Based on their behaviors and reactions, we may infer that one character is in love with another even if they never confess their love.

Secondly, this conflation targets verbal displays of particular emotions as evidence of concept internalization. In contrast, in the view adopted here, evidence is sought in lexical usage rather than in emotion displays. Thus, using Dewaele's example we can say that Tim Parks has internalized the concept of *fare festa* because he displays an ability to spontaneously categorize certain behaviors as belonging to this category. How he feels about this custom and whether he participates in these displays is beside the point. What is at issue is not how speakers go about declaring or hiding their feelings, but whether they identify these feelings similarly to other members of the target speech community.

It is likely that members of any speech community share only core representations of emotion concepts, while peripheral aspects of these conceptual categories, including means of emotion regulation and display, are shaped by their individual experiences, personality characteristics, age, gender, conventions shared by a particular speech community, and a multitude of other factors. The purpose of various naming, sorting, and categorization tasks is to identify representations shared by members of a particular speech community, rather than to elicit an "entire" concept or script of any particular individual or group of individuals. In fact, if we accept a view of concepts as dynamic constructions that are never activated in full (Barsalou, 2003; Malt, Sloman and Gennari, 2003), an attempt to elicit a "complete" script becomes a futile enterprise, because concepts function in a context-dependent manner, readjusting their

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boundaries in accordance with the demands of different tasks and communicative settings. It is precisely this dynamic nature of conceptual representation, underscored in Paradis's comments, that allows for the ongoing conceptual development in monolinguals and bilinguals alike. To state this in another academic language, cultural concepts, indexical in nature, are performatively enacted and (re)created in context, serving to shape our interaction and be reshaped by it; as such, they are forever in a dialectical process (Silverstein, 2004).

The nature of conceptual representation is also involved in commentators' reflections on the modeling of the mental lexicon. Both Harris and Altarriba point out that De Groot's (1993) Distributed Conceptual Feature (DCF) model accommodates conceptual (non-) equivalence I have argued for. I agree that the model allows for full or partial overlap, or lack thereof, yet I also have concerns about its current limitations. To begin with, the DCF model lacks a developmental component and thus cannot predict what may take place in the process of L2 learning and conceptual restructuring. Second, it relies on the feature-based view of concepts, which has been largely cast aside in contemporary cognitive psychology because it does not account for prototype effects (Murphy, 2002), and yet these effects play an important role in L2 vocabulary learning and use (Ijaz, 1986). It is also not clear how the model would go from the general to particular, namely to deciding on the constitutive features of particular concepts. Instead of conceptual features, research in this paradigm focuses on word type properties (word frequency, concreteness, cognate status) that do not reflect characteristics of particular concepts. Several recent proposals, such as the Shared Asymmetrical Model (Dong, Gui and McWhinney, 2005) and the Modified Hierarchical Model (Pavlenko, in press) attempt to address these issues, combining the non-equivalence reflected in the DCF Model with the developmental perspective of the Revised Hierarchical Model (Kroll and Stewart, 1994). None of these models, however, incorporate an affective dimension.

The commentators agree on the need for more complex representation and processing models that incorporate emotionality. In fact, productive directions for future inquiry were at the center of the majority of the commentaries, and it is to this issue that I turn next. In what follows, I synthesize the proposals made in the commentaries and add practical suggestions on where one might look for relevant theoretical, methodological, and analytical frameworks or for particular types of data.

To begin with psycholinguistic research, future studies of single words need to examine differential effects of positive and negative emotions, and more specifically to determine when and to what degree certain processing effects may be limited to negative emotions, elicited by the perception of potential threat (as is the case with swear and taboo words). Experimental studies of individual words could be well complemented by corpus linguistics explorations, as pointed out by Viberg. Corpus-based typological inquiry allows us to go beyond definitions of word meanings grounded in intuitions or in experimental studies with middle-class undergraduates, and to explore the domains of reference and patterns of actual use of particular words across a variety of speakers, texts, genres, registers, and language varieties.

Schrauf points out that most of the studies of bilingualism and emotions have been carried out in the absence of the actual emotional experience and may have been compromised by the notorious lack of reliability of self-reports. He argues that future research should examine how linguistic choices are made in natural interaction, in the presence of felt emotion. Current lack of such data is easily explained through difficulties of recording private conversations. And yet researchers in a variety of fields have developed creative methods of data collection and analysis of natural conversation that can be transferred to inquiry into emotion in bi- and multilingual interaction. Some researchers interested in language and emotions record interviews with the study participants (e.g., Galasiński, 2004; Koven, 2006), others tape-record participants talking to each other (e.g., Kuebli, Butler and Fivush, 1995; Kyratzis, 2001; Cervantes, 2002), and yet others ask participants to tape-record themselves in the absence of the researcher (e.g., Eisenberg, 1999; Piller, 2002). Corpora, as pointed out by Viberg, also contain a variety of useful data, including transcripts of naturalistic conversations. All of these data, however, may be limited to particular contexts and genres (e.g., interview), and participants' linguistic choices may be further constrained by their awareness of the researcher's real or virtual presence (see Piller, 2002, pp. 47–52).

To circumvent these issues, an intriguing new approach has been recently developed by Mehl and Pennebaker (2003; Mehl et al., 2001; Mehl et al., 2007), who assembled large natural interaction corpora with the use of electronically activated recorders that record 30-second segments every 12 minutes during participants' waking hours. While offering access to more naturalistic and varied interactions, this method of data collection also has a major shortcoming in that the interactions recorded are limited to short snippets and do not offer extended stretches of talk for conversation analysis. Thus, we may have no choice but to proceed with triangulation of a variety of tape-recorded conversations with interviews and other self-report data.

Other promising research directions involve diachronic studies. One of the issues the commentators repeatedly emphasized was the dynamic nature of word meanings. From this perspective, it is important to examine ongoing changes in emotion vocabulary across languages and in particular the phenomenon of globalization of semantic

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space (Pavlenko, 2005, p. 229), or the worldwide adoption of Anglo concepts and notions such as *frustration* (see also Wierzbicka's comments). The study of language change in the emotion lexicon could productively draw on Stearns's (1989, 1994; Stearns and Lewis, 1998) groundbreaking research on the history of emotions. Data-wise, corpora of historic materials offer a powerful tool for such research (a variety of historic corpora has been assembled by the Research Unit for Variation, Contacts, and Change in English (VARIENG) in Finland; see www.helsinki.fi/varieng)

Harris, Schrauf, and others point out the need to better understand the development of connections between language and emotions. One way to do this is to examine the process of affective socialization in bi- and multilingual children. This inquiry could adopt analytical tools from cross-linguistic studies of affective socialization of monolingual children, both in peer-interaction (e.g., Kyratzis, 2001) and in interaction with guardians, most commonly mothers (e.g., Eisenberg, 1999; Kuebli et al., 1995; Cervantes, 2002; King and Melzi, 2004).

Dewaele underscores the need for longitudinal studies of affective socialization and conceptual development in adulthood, in – and we might add outside – the foreign language classroom. These studies could clarify the limits of teachability, telling us which aspects of conceptual representation and interpretive and expressive abilities can be internalized through classroom instruction and which require immersion in the target language and culture. Longitudinal studies of affective socialization in adulthood could also tell us when, under what conditions, and to what degree adults could internalize affective valences of second language words. This is perhaps the most challenging area of future research as it has no precedent in the past.

A few words also need to be said about triangulation of findings from different paradigms. This triangulation has potential to illuminate particularly complex issues such as the relationship between context and word emotionality. What does it mean for words to be emotional? Is emotionality an intrinsic characteristic that can be studied out of context? Or is it a perception that arises in particular interactional settings? Altarriba and Harris underscore the importance of the contexts of learning, where context availability becomes an intrinsic characteristic of emotion and emotion-laden words, linking the words to the contexts in which they were originally learned. Harris's research reviewed in my paper suggests that certain words trigger emotional responses even out of context. Kramsch, Viberg, and Dewaele, on the other hand, underscore the importance of contexts of interaction and illocutionary intents with which the words are uttered.

It is quite likely that both positions are correct. To give but one example, one of the greatest taboo words of the English language, the n-word, would undoubtedly elicit strong reactions and very negative ratings from participants in any experiment. And yet this term does not function exclusively as a racial slur, it may also signal brotherhood in hip-hop lyrics or in friendly banter between African-American males, where it acquires a positive valence. What matters, then, is not only the word per se but who utters it and with what intent. Similarly, as we go about our daily lives, we may perceive words like *table* or *argument* as neutral. But if we hear the same seemingly neutral words on a street of a far-away city when we have been away from our native language for weeks or months, they may elicit a very emotional response of surprise and nostalgia. This differential perception of the same words suggests that words may have more than one emotional dimension, and that the contexts in which they are uttered may shape, rather than simply trigger, the perception of their affective valence and emotional force.

Last but not least, Paradis challenges my description of the phenomena in question as uniquely visible in bilinguals and underscores the continuity between monolinguals and bi- and multilinguals. This continuity is not to be disputed and it is indeed impossible to pinpoint an exact boundary at which monolingualism ends and bilingualism begins. My main concern is that taken too far this argument may be turned on its head and used to justify continuous focus on monolingual speakers in empirical research and theoretical reflection. After all, if it is all a continuum, then findings from studies with monolinguals should easily extend to bi- and multilinguals. And yet, it is hard to imagine a monolingual moving to a new register or dialect where they would not have a single familiar word to fall back on. In fact, to echo Kramsch's comments, it is getting harder and harder to imagine a world inhabited by monolingual speakers whose languages reflect homogeneous national cultures. Perhaps, as Paradis suggests, bi-/multilingualism is worth studying because it makes certain phenomena more salient, rather than uniquely visible. And perhaps it is worth studying because soon it will be all we have left as a mode of human contact and interaction, in and outside of academia.

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