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BRIDGING THE GAP

Cognitive and Social Approaches to Research in Second Language Learning and Teaching

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For some, research in learning and teaching of a second language (L2) runs the risk of disintegrating into irreconcilable approaches to

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L2 learning and use. On the one side, we find researchers investigating linguistic-cognitive issues, often using quantitative research methods including inferential statistics; on the other side, we find researchers working on the basis of sociocultural or sociocognitive views, often using qualitative research methods including case studies and ethnography. Is there a gap in research in L2 learning and teaching? The present article developed from an invited colloquium at the 2013 meeting of the American Association for Applied Linguistics in Dallas, Texas. It comprises nine single-authored pieces, with an introduction and a conclusion by the coeditors. Our overarching goals are (a) to raise awareness of the limitations of addressing only the cognitive or only the social in research on L2 learning and teaching and (b) to explore ways of bridging and/or productively appreciating the cognitive-social gap in research. Collectively, the nine contributions advance the possibility that the approaches are not irreconcilable and that, in fact, cognitive researchers and social researchers will benefit by acknowledging insights and methods from one another.

EDITORS' INTRODUCTION

Learning a second language (L2) and developing pedagogy on the basis of what is known about learning are complex endeavors, and research into these areas has taken several different paths. One path is that taken by researchers investigating linguistic-cognitive issues, who pursue objectivity with quantitative research methods, often with the help of inferential statistics. Another path is taken by researchers who consider the social context of activity as a cardinal feature of human knowledge and thus of learning and teaching and who employ qualitative research methods such as case study and ethnography. These are only two of many paths that researchers have taken, but they are perhaps the two that can be most easily discerned and most conveniently contrasted. Do these two paths lie on either side of the stream of knowledge, or is it possible that at one or more bends in the stream there is a bridge by which a researcher may cross to the other side? Pursuing this trope leads to the following question: Is there a gap in research in L2 learning and teaching? And if there is, is the gap ontological (i.e., what *is* the phenomenon we should be studying?), epistemological (i.e., *how* should we be studying it?), or both?

The present article is an attempt by researchers whom we perceive as taking different paths to discuss the extent to which they acknowledge a gap in their own research work and the possibility of a bridge between the path they have taken and the path taken by others. It developed from a colloquium organized by Hulstijn and Young that was designed to address

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these issues at the 2013 meeting of the American Association for Applied Linguistics (AAAL) in Dallas, Texas. The colloquium focused on the question of whether researchers who have taken different paths, which we blandly call the cognitive and the social ways, may benefit from using insights and methods from one another. The premise was that, on each side, there are researchers who have become aware of the limitations of their own path and who are looking for possibilities to enrich their theories and research methods to attain more complete answers to the questions that they ask. Following on the 2013 AAAL event, the colloquium organizers invited the six colloquium presenters to write concise summaries of their individual positions, to be compiled into a larger, single piece. They also invited the colloquium discussant to join them as coeditor, and each of the three coeditors contributed his or her own individual reflections. The product is the present article, which comprises nine single-authored pieces, bracketed by this introduction and a conclusion by the coeditors.

One question, raised but not answered during the colloquium, was whether we are dealing with gaps in the field of SLA or with gaps in the broader field of applied linguistics, or perhaps in the general domain of research in language learning and teaching. We recognize that the gap is particularly strongly felt in SLA. The reason is that, in some domains of research in L2 learning and teaching (such as language teaching, bilingual education, discourse analysis, and language ideology), the proportion of researchers taking a qualitative, interpretive, or relativist stance to epistemology is larger than in SLA and other related domains, such as technology and L2 learning or L2 assessment. Nevertheless, our position is that the gap is also felt in many different domains of applied linguistics because the nature of human language itself leads inevitably to distinct ways of studying it: as a resource and commodity for individuals as writers and speakers and, at the same time, as a social vehicle for the creation and expression of culture. For some researchers in the many different domains of applied linguistics (see Chapelle, 2013, for a recent overview of domains that can be discerned in this field), the focus of their work may be on one view of language to the exclusion of the other, and they may have little sympathy for other interpretations. In the same field, however, other applied linguists may be uncomfortably aware of the gap between their knowledge of language as an individual resource and their appreciation of how it functions in society.

This article is organized into three parts, addressing the following in turn: philosophy and theory construction, data and research methods, and unsolved problems and unasked questions. In using this format, the coeditors have preserved the three-part sequence of presentations in the colloquium. In each of the three parts, two scholars present their opinions—first from a cognitive background and then from a social background—followed by a reflection by one of the editors of this article. In Part 1, Robert DeKeyser and James P. Lantolf address questions of

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philosophy and theory construction. For DeKeyser, scientific inquiry ultimately strives for generalization, but to arrive there, he encourages a descriptive-correlational-experimental loop in which various approaches and methodologies inspire one another. Lantolf argues that there is no gap between cognitive and social approaches, and thus a bridge is not needed. Jan H. Hulstijn concludes this part by reflecting on guidelines and criteria for distinguishing and deciding among competing theories.

In Part 2 of the article, Alison Mackey and Steven Talmy address issues of data and research methods. Mackey recounts a moment in her research within the interaction approach when she began to consider the social context of interaction as a factor creating conditions for successful input, output, and feedback. She calls for a balanced and collaborative approach involving research partnerships of paradigms and methods. Talmy remarks that, after the social turn in SLA, what is needed is what he calls an "interpretivist turn," in which researchers recognize that all research is the outcome of the interpretive activity of particular people working within particular disciplinary and theoretical orientations—in particular, social contexts and historical moments. In concluding this part, Richard F. Young sees the origins of the interpretivist approach in Fleck's (1935/1979) theory of the incommensurability between different thought collectives and thought styles. Young provides, however, several examples in the research literature on language use and language learning that are successful in reconciling apparently incommensurable thought styles.

In the third and final part of the article, Nick C. Ellis and Martha Bigelow address a range of remaining unsolved problems and unasked questions. Ellis views language learning and use as an accomplishment of human social and cognitive competences, all functioning in the same complex adaptive system. He argues that partitioning the field into social and cognitive theories stands in the way of creating an integrative picture of language learning and use over different levels of granularity and timescale, which requires a variety of data types analyzed with a plurality of research methods. Bigelow returns to consider the social and historical site of research into language learning, use, and teaching. She reflects on the challenge that the dichotomy between social and cognitive approaches poses for beginning researchers, who often wish to investigate language development as both a cognitive and a social process. Recognizing that we are all actors in the stories behind the studies we do, her plea is for established researchers to try to bridge the divide themselves and to support beginning researchers to take risks in their own work. Lourdes Ortega concludes Part 3 by addressing recurrent themes. Although there may be many different ways to bridge the cognitive-social gap, Ortega emphasizes the importance of conceiving the gap not as an imminent disciplinary demise but as a compelling invitation to construct and traverse bridges. Commensurability emerges, she argues, from an effort to break away from traditions

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of perceiving and experiencing cognitive and social dimensions of L2 learning and teaching as two monolithic blocks. The three editors close the article with brief concluding thoughts.

This nine-author piece is unconventional as a journal article, and, before leaving readers with it, we would be remiss if we did not acknowledge that the process of writing it has greatly challenged all of us. It has been particularly challenging as a new genre for the three of us who compiled and edited it. In preparing our own reflections on what transpired in Dallas and what it all meant (at least to each of us), we listened to and transcribed the audio recordings of the colloquium. This process reminded us of many important contributions from the audience, but it also created a dilemma as to how we should properly recognize their ideas in the final article. In the end, we have decided against verbatim transcriptions or individual attributions from the audience floor in this article. The reason is that, on the recordings, we recognize the voices of many colleagues but cannot identify several others.[1] We are deeply grateful to each member in the audience who took the floor throughout the colloquium. All their voices, anonymous or familiar, have been instrumental in shaping the dialogue and the piece we offer here.

Part 1: Philosophy and Theory Construction

THE PHILOSOPHY OF SCIENCE AND THE SOCIAL-COGNITIVE DICHOTOMY IN RESEARCH IN LANGUAGE LEARNING AND TEACHING

Robert DeKeyser
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Ever since the cognitive-versus-sociocultural debate that took place in various journals and at several conferences in the 1990s, many researchers in L2 learning and teaching are probably convinced that a wide gap between the two camps is unavoidable. Yet, both Jim Lantolf and I started off our individual contributions at the AAAL 2013 colloquium by saying that there does not need to be a gap. By the standards of the 1990s, we were off to a good start!

Why does there not need to be a wide gap? Why is it even surprising that we have a (perceived) gap? Obviously, most language learning takes place in social and cultural contexts that play a role in the learning process (and all language learning does, if you call the psycholinguistics laboratory a sociocultural context, too, albeit a highly marked one). Equally obviously, all social and cultural learning is at least in part a

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cognitive process: We acquire social and cultural knowledge, and knowledge and cognition are the same thing. Note that they are even etymologically related: The *-gn-* in cognition and the *kn-* in knowledge go back to the same Indo-European root (e.g., see Watkins, 2000, pp. 32–33).

If that is all true, then why is there a widely perceived gap? The core problem, in my opinion, is that a number of dichotomies that have little to do with one another in principle do largely coincide with the cognitive-social dichotomy in practice; this is apparent, for instance, in the way language learning research is currently practiced: Research can be quantitative versus qualitative, hypothesis testing versus hypothesis generating, and absolute versus relativist, and it can use truth versus esthetics as evaluation criteria. Equating all of these dichotomies amounts to confusing material objects (cognitive or social phenomena) with formal objects (aspects of the phenomena most amenable to certain disciplines, methodologies, approaches, etc.; e.g., see Maritain, 2005, p. 67).

In principle, the quantitative-qualitative distinction does not belong here at all. Counterexamples abound of the cognitive equals quantitative and social equals qualitative equations. Prominent examples of cognitive research that is not quantitative in nature are Piaget's work in developmental psychology, Chomsky's approach to language (which is not quantitative, and not even experimental), and much of the early work on first language (L1) acquisition by researchers such as Grégoire (1937–1947), Leopold (1939–1949), Lewis (1936), or Weir (1962), which was, again, mostly not quantitative and not experimental in nature. Conversely, we all know of research in the social and cultural domains that is heavily quantitative in nature, such as the research on motivation for L2 learning in the social psychology tradition (e.g., Gardner, 1985; Lambert, 1972; MacIntyre, 2002) or Gelfand's work (e.g., Gelfand et al., 2011) in social psychology and cultural anthropology that quantifies essential features of a culture.

More inherent is the link between the quantitative-qualitative dichotomy and the distinction between hypothesis generating and hypothesis testing. Quantitative and qualitative approaches do not simply mean presence or absence of lots of numbers. Although the two have been defined in many different ways, for me the most useful way of thinking about them is that quantitative research stresses reliability over validity and control over context, the opposite being true for qualitative research. As all four of these characteristics are desirable, both approaches have advantages. However, the observation that an emphasis on validity and context is better suited for hypothesis generating and that an emphasis on reliability and control is better for hypothesis testing is inescapable. Both are important, of course; the inductive-deductive cycle of hypothesis generating and testing is what allows for theory building and falsifying. Be that as it may, science strives for generalization (not even the biggest accumulation of facts amounts to science), and that

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requires hypothesis testing. As most sociocultural work is exclusively qualitative and, hence, does not allow for hypothesis testing, the association between the sociocultural and the qualitative makes a research endeavor that is limited to the sociocultural inadequate from the point of view of the philosophy of science. This is not to say, of course, that a case study cannot be useful. It can be very important to help generate hypotheses for hypothesis-testing research, and it can also serve to refute some hypotheses by providing a counterexample. Much of our research, however, is about how strong (or how likely) the association between some variables is, and the study of a single case cannot do much to provide evidence for the correlation of variables over cases or the strength of correlation. (Of course I am not merely referring to research involving correlation coefficients here; comparing group averages for a given variable is also about establishing a relationship between variables, one variable being group membership in this case.)

As previously pointed out, however, the association between the sociocultural approach and qualitative methodology is a mere fact, not a necessity. I see no reason why sociocultural research could not move from descriptive to explanatory to predictive, as long as the social dynamics it takes as its material object are not confounded with relativism to the point of rejecting the ideas of hypothesis testing and falsification of theory. Without such a process, there is no criterion for selecting among theories (beyond their esthetic or political appeal), no generalizability, and no science.

Meanwhile, back in the real world of language learning and teaching, addressing the content questions that really matter from a theoretical and/or practice point of view is more important than philosophizing about methodologies. How does L2 fluency develop? How does the cognitive basis of proficiency change over time? How do different kinds of practice at different stages of development change overall proficiency and specific skills? These are examples of questions to which all teachers and learners can relate, even if they do not use the same jargon.

To provide answers to such questions, we need mixed-methods research of a longitudinal nature. Mixed-methods research can avoid the poor articulation between the quantitative and the qualitative that happens when they are largely separate bodies of literature; it is much better if one can have a descriptive-correlational-experimental loop (Rosenshine & Furst, 1973) within a large study and not just in the accumulating body of literature. Longitudinal research is ideal for answering questions that address long-term development, as most of the questions in SLA do. Unfortunately, although both longitudinal research and mixed-methods research are already fairly rare in our field, the combination of the two is largely unheard of. There are practical reasons for this, of course (e.g., the finances needed and the risk of unacceptable

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levels of participant attrition). Other reasons, however, include the sociology of academia (individuals are reluctant to engage in endeavors that will not bear fruit until long after they should have received tenure or promotion) and probably also the fact that such research would not only combine qualitative and quantitative methodologies, which few academics are equally comfortable with, but also require social and cultural contextualization of the psycholinguistic findings as well as cognitive interpretation of the social and cultural dynamics, two things that even fewer academics are used to.

While we wait for a new generation of more truly interdisciplinary teams of researchers, the best we can do is to encourage the descriptive-correlational-experimental loop by letting various approaches and methodologies inspire one another, and ideally feed into one another, as the loop metaphor implies. Even if we manage to pull off a large-scale longitudinal mixed-methods study, the fact that answers to scientific questions come from bodies of literature and not from individual studies should always be in the backs of our minds. Therefore, the need to read and consult widely and to contribute to the loop (or spiral?) will be with us for a long time to come.

Ultimately, the ability of individual studies to contribute to this cumulative body of work will be best served if we all strive for context and control, for reliability and validity, realizing that sometimes a trade-off is unavoidable. There is no such trade-off between the social and the cognitive; after all, Vygotsky himself emphasized the social nature of cognition. We must all strive for generalizability of our findings, though, whether we are primarily cognitively or socioculturally oriented, and whether we try to improve generalizability by building more complex designs (e.g., aptitude-treatment interaction studies instead of mere method comparisons) or by designing simpler studies while keeping in mind their place in the descriptive-correlational-experimental loop. There is much value to many things beyond science (visual arts, film, literature, literary criticism, journalism, etc.), but without a relentless pursuit of the generalizability of knowledge, there is no (social) science.

A BRIDGE NOT NEEDED

The Sociocultural Perspective

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In inviting contributions to the 2013 AAAL colloquium, the organizers asked two key questions: Is there a gap between cognitive-linguistic and

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social approaches to SLA, and, if so, is it due to epistemological or ontological differences (or both)? According to the organizers, the answers to the questions matter because research in learning and teaching of a L2 risks "disintegrating into two irreconcilable approaches," as the colloquium abstract read. Given the comments from the audience during the colloquium, it seems that the fault line is indeed due to epistemological differences between those who engage in quantitative research—presumably cognitivists—and those who carry out qualitative research—presumably socialists (pun intended). As DeKeyser forcefully argued during the colloquium and in his contribution to the present article, however, the problem is not about the isomorphism between cognitive research and quantification on the one hand and social research and qualitative analysis on the other. Science does not rely exclusively on quantification to define itself. Final confirmation of Einstein's general theory of relativity, for example, came from the observation of a single solar eclipse. Certainly, Vygotsky and his colleagues had no aversion to quantification and made extensive use of inferential statistics in their research. (However, following a different research tradition, they reported on the outcome of their analysis as significant or not and avoided cluttering their manuscripts with dense and, at times, impenetrable statistical manipulations.) In DeKeyser's words, science is the "relentless pursuit of the generalizability of knowledge," a stance that clearly captures Vygotsky's goal in proposing sociocultural theory as a new, unified theory of psychology. In what follows, I present a brief account of Vygotsky's proposal for overcoming what he characterized as the "abyss" (Vygotsky, 1997, p. 311) between idealist and materialist approaches to psychology to explain why a sociocultural approach to language learning and teaching does not have to concern itself with ways of bridging gaps, be they cognitive-social, teaching-testing, or research-practice in nature. Vygotsky did not bridge them; he eliminated them.

Vygotsky (1997, p. 311) perceived the crisis in psychology as emanating from the Cartesian mind-body dualism that separated idealist from materialist approaches to the study of mind. The differences emerged from divergence in epistemological and ontological assumptions. The idealist side of the abyss privileged first-person accounts of inner mental experiences (e.g., the subjective experience of consciousness based on Husserl's phenomenological philosophy and Freud's psychoanalysis of the unconscious), whereas the materialist branch comprised reflexologists (e.g., Pavlov) and behaviorists (e.g., Thorndike, Watson) who focused on bodily reactions to external stimuli (e.g., salivation in animals, button pushing in humans). He sought to overcome the crisis by constructing a unified methodological orientation that would lead to reliable and generalizable knowledge grounded in a materialist ontological perspective—a perspective that is explained in the discussion that follows.

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Although Vygotsky realized that the goal was to build a single science of psychology—a single epistemology—he recognized that this would be an impossibility if the discipline continued to accept "*two* categories of being which are fundamentally, qualitatively heterogeneous and irreducible to each other" (Vygotsky, 1997, p. 314). Essentially, the creation of a single epistemology depended on the answer to the following ontological question: What is the nature of human cognition? Young, in his contribution to the present article, proposes that understanding what cognition is is an epistemological and not an ontological problem. However, ontology is concerned not just with what is but also with the nature of what is (Hofweber, 2013). Vygotsky recognized that the problem could not be resolved by building bridges between idealism and materialism, because no matter how much traffic crosses the bridges, the abyss is still there. Widdowson (2012), in discussing the theory-practice gap, took a similar position, reminding readers that bridges "are a way of crossing from one side to another. The gap is still there and the difference remains" (p. 3).

The proposal Vygotsky offered was grounded in dialectical materialism (DM) and, more specifically, within the historical dialectic materialism (henceforth, HDM) of Marxist socioeconomic theory. To give a brief description, DM is predicated on the fundamental assumption that "nature alone, based on matter in motion, has a self-sufficient existence; everything in human life is derived from and dependent upon the objective world" (Novack, 1978, p. 119). Although everything is comprised of matter, it manifests itself in "an endless multitude of material objects and systems," including human society, which may be different from nature but is at the same time "inseparably linked with it and has a material basis of existence" (Yurkovets, 1984, p. 50). Thus, everything we encounter in the world is "matter in the process of development" (Yurkovets, 1984, p. 51). Dialectical materialism is a specific manifestation of materialist theory, which holds that matter constantly undergoes motion and transformation as a result of the interpenetration of opposing forms of materiality. It postulates the universality of "the process of internal contradictions," which operates in inorganic and organic nature as well as in society and human thinking, all of which have a material foundation (Novack, 1978, p. 401). Among the dialectical processes are the "struggle and unity of opposites," "the mutual transition of qualitative and quantitative transformations," and "negation of the negation" (Yurkovets, 1984, p. 33).

Although DM postulates that dialectical processes are universal and therefore operate in the four material domains previously mentioned, each domain requires domain-specific intermediate theories to explain the phenomena unique to its segment of reality (e.g., planetary motion in astrophysics, photosynthesis in biology, monetary systems in economics, cognition in psychology). Vygotsky's task was to build an intermediate

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theory for psychology that simultaneously took account of general dialectical processes as well as domain-specific processes, concepts, and principles. To achieve his goal, among other things, he borrowed two very important notions from HDM: (a) the concept of the *cell*, which for Marx was commodity and served as the unit of analysis for Marx's study of economic formations, and (b) history and the process through which humans create their own life conditions. As the cell, or unit of analysis of his theory, Vygotsky proposed, not without controversy (see Wertsch, 1985), linguistic meaning, and, as its investigative approach, he put forward the *genetic method* (which is explained in the next few paragraphs).

Vygotsky argued that the fundamental dialectical process that gives rise to human thinking (i.e., consciousness) is the interpenetration of two different forms of matter: the human brain, which is subject to the laws of biological evolution, and human social activity, including social relationships shaped by institutions such as family, politics, economy, education, religion, leisure time, and so forth as well as the artifacts that humans create as they participate in the various institutional activities at the core of human life. He argued that consciousness is the consequence of social activity reflected in the human brain. To use an analogy discussed by Vygotsky (1997, pp. 327–328), consciousness is the equivalent of an image reflected in a mirror. The image is determined by the properties of the mirror, the object reflected, and the light waves bouncing off the mirror—all of which are material. The image itself, however, is not.

Vygotsky recognized that the brain has a specific structure shaped over the course of biological evolution, and, as a consequence, it is endowed with certain mental capacities, which humans share with other primates (e.g., memory, perception, attention). What makes human thinking different from primates, however, is its voluntary, mediated nature, the source of which is found not in the brain but in the world of human sociocultural activity. Indeed, as Leontiev (2004) pointed out, "thousands of years of social history have produced more, in this respect, than millions of years of biological evolution" (p. 85).[2]

Although the mirror analogy is useful in pointing to the nonmaterial quality of consciousness, the analogy has its limitations. For one thing, an image emerges from an object—the mirror—that cannot act on and change the world in the way consciousness operating through the human body can, and, for another, human bodies are themselves embedded in social formations that enhance the power of single individuals to shape the world in ways that have a reflexive effect on thinking. Thus, the interpenetration of world and brain-body results in bidirectional processes whereby each influences and potentially changes the other.

Vygotsky argued, therefore, that, given the ontological difference between the object of study of the physical sciences (i.e., the natural

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world) and the object of study of materialist psychology (i.e., the dialectical coupling of biology and sociocultural life), "new methods of investigation and analysis" were required (Vygotsky, 1978, p. 58). The general methodological framework proposed by Vygotsky is usually referred to as *genetic analysis* (see Wertsch, 1985). It is a methodology based on the fundamental notion that it is only in "movement that a body shows what it is" (Vygotsky, 1978, p. 65). By "movement," Vygotsky, relying on Marx's analytic approach to the study of social and economic forces, meant the study of the history of a process as it emerges and changes over time. Vygotsky introduced the genetic method into four domains in which thinking could be studied: phylogenesis, which focused on comparisons of human and primate psychology; sociogenesis, which concerned itself with the development of human social formations (e.g., political, economic, educational, and religious institutions) and artifacts (e.g., language, numbers, art, music, computers, etc.) and their impact on thinking; ontogenesis, which highlighted the development of individuals throughout their lives as they came into contact with different social formations and artifacts; and microgenesis, a term not used by Vygotsky but coined by Wertsch (1985, p. 54) to capture experimental research designed to provoke development over the course of relatively short time periods. Vygotsky (1978, p. 61) referred to this type of research as experimental-developmental, in which the goal is to create cognitive development under laboratory conditions to understand how people appropriate and internalize different forms of mediation as they engage in practical tasks (for a fuller discussion, see Lantolf & Poehner, 2014).

For Vygotsky, explanation is achieved not through prediction but through reconstruction (sociogenesis) and construction (ontogenesis and experimental-developmental research). This is not to say that prediction and hypothesis testing do not enter into his methodological approach, but these are predicated on a genetic epistemology. For instance, the theory predicts that changes in sociocultural circumstances are likely to effect changes in psychological processing. This was documented in Luria's (1976) research among Uzbek peasant populations in the 1930s, whose thinking changed from a highly functional and pragmatic mode to a more abstract and taxonomic mode as a consequence of schooling (see Tulviste, 1991, for a replication of this study, carried out among the rural population of Kyrgyzstan). It has also been documented in the research of contemporary cognitive psychologists and cognitive anthropologists. Boivin (2008), for example, reported on a study that demonstrated the differential effects of subsistence culture on the psychology of time. The Muria Gonds, a rice-farming community of central India, have a highly structured and precise sense of time, which is conceptualized as a scarce resource. They pay close attention to the calendar, arranging social events years in advance and often refusing requests with the expression "I haven't got time" (Boivin, 2008, p. 56).

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In contrast, members of the Umeda community of New Guinea, whose subsistence depends largely on gathering sago fruit by roaming through the forest at irregular intervals, have a vague concept of time. Members of the community do not know how many months constitute a year and instead operate with two seasons: wet and dry (Boivin, 2008, p. 57).

Another study implicating the social environment in cognition is a study by Evans and Schamberg (2009) on the effect of childhood poverty on working memory. Their research presented evidence that the chronic stress experienced by children raised in poverty resulted in the overproduction of the stress hormones epinephrine and cortisol, which in turn weakens the body's immune system, with a deleterious effect on neural functioning, including working memory. The researchers argued that it should be possible to overcome the problem through specific training programs aimed at improving working memory. A study by Holmes, Gathercole, and Dunning (2009), for instance, showed that it was indeed possible to improve the working memory of children and young adults who experience temporary emotional stress (e.g., the death of a loved one) through training. Casasanto's (2011) research focused on the effect of bodily differences on thinking, an area that, as far as I know, Vygotsky and his colleagues did not explore in any detail but that certainly meshes with the tenets of the theory. Casasanto uncovered cognitive difference in the thinking of right- versus left-handers. For example, when presented with action verbs such as *grasp*, right-handers process its meaning in the left hemisphere, which controls right-hand movements, whereas left-handers process these verbs in the right hemisphere, which controls their left-hand movements. Casasanto (2011) suggested that "people tend to understand verbs as referring to actions they would perform with their particular bodies . . . in this sense, people with different bodies understand the same verbs to mean something different" (p. 379).

Casasanto also reported that the meanings of cospeech gestures are different depending on handedness. For instance, right-handers tend to use left-hand gestures when verbally expressing negative emotions and tend to use right-hand gestures when using positive emotional language. Left-handers reverse things. Moreover, when forced to change handedness due to brain injury or when artificially handicapped in an experimental situation (e.g., wearing a bulky ski glove on the dominant hand during extended manual motor tasks) right-handers—the only ones participating in their study—manifested a shift to the left-handed bias of left-is-good. From this, Casasanto concluded that "motor experience plays a causal role in shaping abstract thoughts" (2011, p. 381).

Andy Clark (1998, 1999), following a line of reasoning that parallels Vygotsky's (1978, p. 60), argued that the human brain evolved in such a way as to take advantage of the affordances provided by the environment to carry out cognitive processes. Using the parlance of contemporary

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technology, A. Clark (1999, p. 349) refers to the bodily and environmental components of thinking as "wideware." The idea of wideware is that aspects of the social environment are necessary components of cognitive processing in a way that parallels how other species integrate aspects of the physical environment into their behavior. For instance, as A. Clark (1999) pointed out, given their physical attributes, bluefin tuna should not be able to execute the sudden directional shifts and rapid acceleration in swimming speeds they are noted for. They are able to carry out these behaviors not because of their anatomical structure but because they have learned to exploit the vortices and changing currents of the ocean, and this enables them to perform swimming behaviors that their bodies alone are incapable of. Hence, the bluefin is not just a fish, but a "fish-as-embedded-in" its environment (A. Clark, 1999, p. 345). Similarly, humans exploit their environment as "agents-operating-with-mediational-means" (Wertsch, 1998, p. 24). For instance, to multiply large numbers (e.g., 3,454 × 9,759), most of us reach for pen and paper. As sociocultural artifacts, these tools allow us to reduce what would otherwise be a complex multiplication process to a series of simpler calculations (A. Clark, 1998, p. 349). If the artifacts are withheld, most of us are at a loss to solve the problem (unless, of course, we have access to another human artifact, a calculator). The paper and pencil, therefore, are the wideware elements that allow humans to carry out a process that their brains, acting alone, would find difficult, if not impossible, to complete.

With regard to language learning and teaching, we would expect that, under different social circumstances, language development would vary. On this view, learning a new language in educational settings would be a different cognitive process from learning the same language in immersion settings. In the former environment, properly organized instruction provokes rather than follows development and, as such, can be expected to result in different developmental routes than what has been documented among those who acquire language exclusively by immersion, in which they do not often receive systematic or well-organized mediation. We are currently assessing this hypothesis through comparison of the developmental processes of learners in instructed settings with the findings of research conducted under the umbrella of processability theory (Pienemann, 1998) and its related teachability hypothesis (Pienemann, 1984, 1987). Specifically, X. Zhang (2014) has demonstrated that a pedagogical intervention designed according to sociocultural theory educational principles does alter the developmental path proposed by processability theory for topicalization in L2 Chinese (see Y. Zhang, 2001).

To echo and extend to sociocultural theory–SLA the sentiments expressed by Nick Ellis in his contribution to the present article: There is no gap. Building a bridge, therefore, is an unneeded act that would result in a bridge to nowhere.

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EPISTEMOLOGICAL REMARKS ON A SOCIAL-COGNITIVE GAP IN THE STUDY OF SECOND LANGUAGE LEARNING AND TEACHING

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As an observer of the discussions in the 2013 AAAL colloquium, it seemed to me that there appeared to be agreement on two points. First, there does not exist a gap between social and cognitive approaches to L2 learning and teaching insofar as language learning is inherently social and, at the same time, all learning is cognitive by definition. There was, however, no agreement on the weight to be attributed to the social aspect of language learning. For instance, for Lantolf, who adopts a Vygotskian approach, the psyche is inherently social, and there is therefore no opposition between nature and nurture. Culture creates special forms of behavior, it modifies the activity of mental functions, and it constructs new superstructures in the developing system of human behavior. For DeKeyser, however, this claim needs to be tested empirically (it must be falsified or supported). Thus, whereas Lantolf emphasized the ontological status of Vygotsky's theory, DeKeyser raised issues of an epistemological nature: How can we obtain supporting or conflicting evidence for Vygotsky's theory, or indeed any theory? I return to this point later in this piece.

Second, speakers appeared to agree on the fact that scientific knowledge and the interpretation of the findings of empirical research is collective in the sense that interpretations are shared by scholars united in what might be called a theory or a school—or what Fleck, in an important but relatively unknown book, called a thought collective, and which, some 30 years later, Kuhn called a paradigm (Fleck, 1935/1979; Kuhn, 1962/1996). Whereas Young, in his contribution to the present article, elaborates on the social-cultural context of scientific knowledge in schools (or thought collectives), here I limit myself to proposing some hermeneutic questions with which one may try to diagnose the nature of the gap, real or apparent, between scholars defending different theories. Do their theories belong to different, incommensurable schools, and, if so, what is the nature of the incommensurability? There may be several gaps in our field, of which some might be bridged, whereas others cannot. An unbridgeable gap clearly exists between relativists and critical rationalists. As DeKeyser put it, does an acknowledgement of the social-cultural context of scientific knowledge stand in the way of hypothesis testing, seeking generalizable knowledge, and evaluating theories? DeKeyser pointed out that there is a difference between acknowledging that all scientific knowledge is embedded in a social-cultural context and claiming that all scientific knowledge is relative; a scholar can agree with the first

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statement but assume at the same time that there is a best truth that we can find at this point and that it is the scholar's task to uncover that truth.

Although I place myself in the camp of Popperian critical rationalism (Hulstijn, 2013; Jordan, 2004), the AAAL colloquium left me in a state of confusion with respect to the question of how to conceptualize differences among applied linguists and SLA researchers in terms of their theories and their epistemological stances. To help myself, and hopefully to help others too, I have listed in the subsequent sections a number of considerations and evaluation criteria instrumental in making sense of differences in theories and their epistemological stances. Though some may consider these criteria biased in favor of critical rationalism, I offer them in the spirit of promoting better understanding across epistemological lines.

GUIDELINES FOR DIAGNOSING DIFFERENCES BETWEEN THEORIES

In trying to make sense of what appear as different theories, approaches, frameworks, or models espoused by individual scholars, it might be useful to ask the following questions.

Epistemology

Are we dealing with differences with respect to epistemology? Is one scholar perhaps a relativist, and does the other perhaps subscribe to Popper's (1959) critical rationalism? In this case, the gap cannot be bridged. These epistemological stances are incommensurable. Note that this epistemological divide does not necessarily run along the borders between Vygotskian and non-Vygotskian approaches, between social and cognitive theories, or between qualitative and quantitative methods in applied linguistics.

Theoretical Constructs

Are theoretical constructs proposed as requiring bottom-up empirical evidence for their existence ("research-then-theory"), or are they proposed as not in need of direct empirical support ("theory-then-testing"; Jordan, 2004, p. 46)? To give an example from linguistics, whereas most pre-Chomskyan structuralists adopted the bottom-up stance, linguists working in the generative school work from the top down. The abstract

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constructs in a structuralist theory rest inductively on empirical evidence. A good example is the phoneme construct. A structuralist theory offers empirical means to determine whether or not a sound-like phenomenon is assigned the status of phoneme. By contrast, a theory, viable in the generative school of linguistics, may consist of many unobservable abstract constructs, as long as hypotheses can be derived from a theory that can be empirically tested (supported or falsified). In the top-down approach, testing theory-driven hypotheses is the way to accept or reject a construct in the theory's attempt to explain a phenomenon.

Object of Inquiry

Do the two theories try to solve the same problem? Do they have the same object of inquiry? For instance, a theory that attempts to understand the behavior of students and teachers in L2 classrooms cannot be directly compared with a theory that attempts to address the so-called learnability question in the field of SLA (briefly: Why do L2 learners produce grammatical utterances that they have never heard or seen?). Note that two scholars can, but need not, differ both in the objects of their theories and in their epistemological stances.

Assumptions

Is it possible to distinguish, in each scholar's theory, between (a) the fundamental part that is not proposed as testable (the theory's assumptions or postulates) and (b) the part that is proposed as testable—that is, the part from which testable hypotheses can be derived? Although differences in (b) can, in principle, be settled by empirical research, differences in (a) cannot be settled on rational grounds, and, in this case, we can speak of an unbridgeable gap. Broadly speaking, most (perhaps all?) theories consist of two parts. The first part is their conceptual basis, comprising a number of assumptions that in and of themselves are not proposed as being falsifiable. Built on this conceptual basis, the theory's second part consists of an explanation that is proposed as testable. For instance, researchers working in the generative framework assume that both L1 learners and L2 learners acquire abstract properties of the target language that are underdetermined by the input. Thus, the input is insufficient for the acquisition of such properties. This assumption forms the basis for explaining the acquisition of, for example, *wh*-movement by L1 and L2 learners of English (White, 2007). The assumption of underdetermination (known as *poverty of the stimulus*) cannot be falsified within

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generative linguistics itself. If one does not want to accept this assumption, one has to construct another theory with other assumptions. For instance, emergentist theories of language acquisition do not assume that L1 and L2 acquisition are constrained by abstract linguistic knowledge. Instead, they assume that the input does suffice for both L1 and L2 acquisition by postulating general learning principles not specific to language learning.

Having made this distinction between a theory's assumptions and claims derived from it, I should perhaps add that it is possible for a theory to include no claims (or hardly any) proposed for empirical investigation. Such a theory thus mainly consists of a set of views, and, for critical rationalists, this would hardly qualify as a theory because, for them, falsification of testable claims is the hallmark of inquiry. If I understand Lantolf's position correctly, the materialist approach to psychology in Vygotsky's theory is proposed as a postulate that cannot be tested within the theory. However, Lantolf also formulates a claim derived from the theory—namely, that changes in sociocultural circumstances are likely to bring about changes in psychological processing. He cites a study in progress to test alternative claims with respect to processes of second language development, emanating from sociocultural theory and processability theory (Pienemann, 1998).

Note, furthermore, that two theories that rule each other out in terms of assumptions or claims cannot both be correct. Thus even a relativist could not say that, whereas the generativists are right in postulating that L1 and L2 learning are constrained by highly abstract linguistic preknowledge, emergentists are right in solely postulating general learning principles not specific for language learning.

At this point, I should note that, in the previous paragraphs, I have used the term *school* (paradigm) in two ways, which is potentially confusing. First, I placed theories adopting different stances to epistemology (e.g., bottom-up vs. top-down views on theoretical constructs, or relativism vs. postpositivist rationalism) in different schools. But I also placed in different schools theories differing in terms of the nontestable assumptions with respect to the phenomena to be explained or understood. In the case of structuralist and generative linguistics, I believe we are dealing with schools that differ both in epistemology and in terms of assumptions. That is, the structuralist and generative schools hold, respectively, bottom-up and top-down views on the status of theoretical constructs, and, respectively, they deny and accept the necessity of testing theory-driven hypotheses. In terms of their respective assumptions, they do not (structuralist) or do (generative) hold assumptions concerning the so-called black box of the human mind. But in the case of generative linguistics and emergentism, I believe we are dealing with two schools adopting the same epistemological stance toward theoretical constructs and the necessity

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of theory testing, but we are taking different positions with respect to some fundamental ontological assumptions concerning language acquisition. That is, whereas generative linguistics and emergentism both allow abstract constructs not based on observable phenomena as long as testable hypotheses can be derived from the theory, the generative school explains language acquisition with the assumed existence of a language faculty, and the emergentist school assumes that language acquisition can be explained solely with a general cognitive learning device.

ADDITIONAL CRITERIA

Given two theories that appear to address the same question and after having answered the four questions just discussed, one might subsequently profit from the following criteria, proposed by Kuhn (1977), who thereby demonstrated that he was not an extreme relativist, as some of his critics have argued (see Jordan, 2004, p. 51). I render the criteria in my own words:

- *Coherence.* How coherent are the theories? Are their constructs clearly defined, and is their formulation consistent?
- *Testability.* To what extent are the theories testable? What is the empirical evidence for the theories?
- *Scope.* What is the scope of the theories? That is, which and how many phenomena can they account for (beyond the question that they both address)?
- *Fruitfulness.* How (potentially) fruitful are the theories? Do they open avenues for new insights and new research?
- *Simplicity.* How simple are the theories? If the two theories score equally well on the other four criteria, can we then, for reasons of economy, select the one with fewer constructs?

I acknowledge that, for extreme relativists (including postmodernists and constructivists; see Jordan, 2004, Chapter 3), these five criteria for the evaluation of competing theories are unacceptable because of their intellectual origins in Popperian critical rationalism. In their view, critical rationalism should not be applied—at least not wholesale—to applied linguistics in general or to sociocultural issues in L2 learning and teaching and language education in particular.

CONCLUDING REMARK

This brings me back to what I see as an almost impossible job of playing the role of a commentator in a more neutral way, with the intention to seek bridges, while, at the same time, embracing Popper's (1959)

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philosophy of critical rationalism. When I compare the debate and personal communications during and after the colloquium with the relativist-rationalist debate in the 1990s (e.g., with Block, 1996, in one camp, and Gregg, Long, Jordan, & Beretta, 1997, in the other—for more references to this debate, see Jordan, 2004, pp. 1–3), it occurs to me that there are two differences between then and now that give reason for optimism. First, as I tried to argue with my diagnostic questions, there is no single social-cognitive gap in L2 learning and teaching research: Differences in stance may be a matter of epistemology (extreme relativists vs. extreme rationalists), but they may alternatively constitute a matter of ontology (the part of the theory that is proposed as untestable). Second, there appears to be a willingness to listen to voices from the other side and to enter into a dialogue not characterized by acrimony.

Part 2: Data and Research Methods

EXPLORING QUESTIONS OF BALANCE IN INTERACTION RESEARCH

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In the 2013 AAAL colloquium and in my contribution to this article, I was asked to address the following questions: What are the methods of inquiry that you prefer in your own work, what do you see as their weaknesses or limitations, and what attracts you in other methodologies? I have chosen to address these issues by discussing work within the interaction approach to SLA. In particular, I focus on how that approach is currently evolving to include a social dimension and how typical methods of inquiry associated with it are expanding in parallel. I have titled my contribution "Exploring Questions of Balance" for reasons I hope will soon become obvious.

In the initial version of the interaction hypothesis, meaningful interaction, corrective feedback, and associated interactional features were thought to be associated with attention, in a process involving intake and driving L2 learning. However, in the original formulation of the hypothesis and until recently, there were relatively few concrete statements or claims in relation to the social factors that underlie interaction. In the last few years, a reenvisioned and current version of the interaction approach has begun to take off (see, e.g., Mackey, 2012). In general, interaction researchers are now taking a more cyclical view, as DeKeyser has noted in his contribution to this article. Qualitative research often provides insights into social factors such as those mentioned by Young in his

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contribution to this article, and it is then sometimes followed up by quantitative research. This is essentially the same process that occurs when controlled and balanced laboratory research uncovers constructs that can then be explored in classrooms and possibly applied (with caution, as Lightbown, 2000, has reminded us) in authentic instructed settings. Keeping this cycle in mind is one of the issues of balance. We need to carefully weigh the cognitive, the social, the laboratory, and the classroom, bearing in mind the goals of the research. It is also important to consider pace, because research needs to progress at a rate at which the expansion of inquiry evolves at a comfortable speed. In other words, we need to recognize the value of different approaches, both in their own right and also for what they can provide. It is not practical and should not be necessary for those interactionists beginning to consider social variables to completely shift their methodological tools and paradigms. Although progress is inevitable as we work to continue to address interesting questions and to elicit valid data, these developments should build on a strong foundation, and we must recognize that the *process* of evolution is important.

The progression to include social variables can be seen in other work related to interaction research. For example, in relation to Robinson's (2001) cognition hypothesis and task complexity, recent work reported by Révész and Gilabert (2013) described methodological advances in task research illustrating the same trend that we are also privy to in the interaction and feedback area—namely, a primarily cognitive orientation beginning to be influenced by social concerns. Révész and Gilabert, for example, looked at learners' perceptions of difficulty and mental effort, supplementing external theories and claims about difficulty by triangulating data from the learners' perspectives. It is a short next step for research like this to examine a range of factors that may impact how learners respond to these sorts of questions, and it would seem to be a logical next step from looking at what learners believe about difficulty to asking what contributes to those beliefs.

The realization that considering social and contextual factors provides interesting primary and supplementary data can also be seen in work that looks outside the head of the individual and unpacks what a "task" means to an individual learner or pair. For example, Young's (2009) work recognizes that what he calls discursive practices—essentially communicative tasks—have a history, and he observes that we need to understand what learners do in any one task by also looking at the various aspects of their history with that task and what it means for them.

In other words, as the interaction hypothesis and task-based approaches to learning continue to evolve, it seems logical to me that those taking a more cognitive approach and those taking a more social approach will benefit from working together. This is another one of the questions of balance I want to raise. We need to ask one another, and ourselves, how

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we can benefit from combining our perspectives. It is clear how fruitful cognitive research and social research are on their own, as other contributions to this article demonstrate. I suggest that we need to ask questions like the following: What can we gain? How can cognitively oriented research most productively include socially oriented methodologies and vice versa? What are the challenges in doing this? How do we go about creating research questions that do not all suppose a priori constructs? And how do we adopt new methods, adapt current methods, and combine methods? I suggest that, to do this most effectively, collaboration is an essential requirement.

In a study I carried out with Jenefer Philp, published in 2010 in a book edited by Rob Batstone, we saw how relationships among learners impacted what they were willing and able to listen to and attend to in interaction and how this impacted what they produced. Because input and output were clearly equated with learning for us as interaction researchers, we realized that we needed to recognize what creates conditions for what we think of as successful input and output. This led us to think about methodologies for investigating social concerns.

What the Philp and Mackey (2010) study led me to believe is that my own perspective has evolved and now falls somewhere between quantitative and qualitative and between a primarily cognitive and a primarily social approach. This led me to ask how researchers from each perspective do what the other party is trained to do when (a) we do not have the skill set in methodology and (b) we are not fully confident in the framework. These issues are taken up much more eloquently by Martha Bigelow in her contribution to this article. As an example of the need for caution when examining questions of balance, our field does sometimes see criticisms of mixed methods as well as objections when someone like me, who has traditionally taken a primarily cognitive approach, takes a mixed-methods approach. It is obviously a legitimate concern to suggest that tacking a minor case study of one or two learners onto the end of a primarily cognitive, quantitative study is not sufficient to allow us to think of our approach as authentically socially informed. I do not intend this to be an argument against mixed-methods research, although I do suggest that it might be short sighted for social researchers to call for cognitive researchers to examine social factors without acknowledging that there may be benefits to their own research of taking cognitive factors into perspective. The problem is, once again, one of training, of time, and of expertise. Again, I believe one way to address this is through collaboration and teamwork.

Simply associating cognitive with quantitative and social with qualitative is overly simplistic (a point also made in a much clearer way by Talmy in his contribution to this article). Some modes of inquiry are associated with both and some with only one. Some of the constructs

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that we may consider socially informed, or at least useful for broadening the horizons of traditionally cognitive work—for example, motivation, willingness to communicate, or introspective measures—are psychological constructs and methods that have been included in research on language learning and teaching since the inception of the field. We need to recognize that, just as we read in research methods books, there is no clear dichotomy between quantitative and qualitative, and our familiarity with paradigms similarly operates on a continuum.

I also believe that being open to new methodologies for inquiry, as I optimistically think we have traditionally tended to be in L2 learning and teaching research, may guard us from entrenchment in theoretical approaches and from oppositional positions. Indeed, data obtained through stimulated recall protocols—elicitation techniques that were originally employed in interaction work simply to shed light on learners' cognitive processes (Gass & Mackey, 2000)—were what eventually convinced me that social factors are an important force in cognitive-interaction research.

In summary, then, it is my belief that we need, as Rod Ellis (2010) has put it, to go beyond the psycholinguistic rationale in the interaction approach. However—and this is again related to balance—let's not throw the baby out with the bathwater. I vote for augmenting and developing the interaction hypothesis and working together with those who take a primarily social approach. Together, I think we can achieve the best outcome. By this, I mean research partnerships of paradigms and methods. Social factors underlie the nature of learners' participation in interaction and, therefore, will logically impact learning opportunities through interaction.

TOWARD AN INTERPRETIVIST TURN IN L2 STUDIES

Reflexivity, the Cognitive-Social Divide, and Beyond

Steven Talmy
University of British Columbia

In my contribution to the 2013 AAAL colloquium and to this article,[3] I make a brief case for an interpretivist turn in L2 studies, predicated on the belief that if the field has made a social turn—or, for that matter, a discursive turn, a narrative turn, a multilingual turn, a critical turn, and so on (Pennycook, 2010)—then the frameworks of inquiry that we deploy need a similar turn to help us account for them. A good candidate for such an accounting is interpretivism (e.g., see Howe, 2003, 2004). I begin by quoting from the abstract to the 2013 AAAL colloquium:

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> The field of applied linguistics runs the risk of disintegrating into two irreconcilable approaches to language learning and use. On the one side, we find researchers investigating linguistic-cognitive issues, often using quantitative research methods including inferential statistics; on the other side we find researchers working on the basis of sociocultural or socio-cognitive views, often using qualitative research methods including case studies, ethnography, and often taking a critical stance. This colloquium focuses on the question whether both sides might benefit from using insights and methods from one another. The colloquium's premise is that, on each side, there are researchers who have become aware of the limitations of their own approach and who are looking for possibilities to enrich their theories and research methods in order to attain more complete answers to the questions that they seek to answer.
>
> The colloquium brings together researchers from both sides who share the concerns expressed above and who are willing to enter into dialogue with the other side.

There are several important assumptions in this abstract that raise many important questions, only a few of which I have space to consider here. What follows is not meant as criticism, for surely the assumptions I address are prevalent throughout L2 studies. Its purpose is, instead, to provide a context for the discussion that I undertake in the second half of my contribution.

QUESTION 1. ARE WE BRIDGING DIVISIONS OR (RE)CREATING THEM?

This question concerns the premise of the colloquium itself, specifically the binarisms that it denotes and connotes—cognitive-social, quantitative-qualitative, experimental-naturalistic . . . and why stop there: neutral-political, conservative-progressive, right-wrong. It is hardly an original insight to note that binarisms like these are reductive and fail to capture the kinds of complex inquiries often undertaken in their name. It is worth asking if we might be reifying and reproducing the very divisions the colloquium is predicated on bridging. Moreover, are the distinctions between them as stark as we make them out to be (Allwood, 2012)? Who profits from them? Do we need to *bridge* divides or *dissolve* them by interrogating (how we participate in) their construction and by committing to metatheoretical dialogue with colleagues on other coastlines?

I would like to extend this question to the participation structure that was adopted for the colloquium in Dallas: that of debate. Debates cannot be construed as entering into dialogue. Rather, debates yield the sorts of *displacive discourse* that Julian Edge (2004) has written of (also see Tompkins, 1988), the stuff of the so-called paradigm wars of the 1970s

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and 1980s, and, closer to home, in L2 studies, of the 1990s. Have those zero-sum, scorched-earth disputes (of many examples, see Beretta, Crookes, Gregg, & Long's, 1994, "response" to van Lier, 1994) taught us nothing?

QUESTION 2. QUANTITATIVE : COGNITIVE :: QUALITATIVE : SOCIAL?

Relatedly, in arguing for an enrichment of research that engages with diverse theoretical orientations, we could also ask if we are working from a premise in which cognitive is mapped to quantitative and social to qualitative. Obviously, there is substantial quantitative research that adopts an explicitly social orientation, just as there is qualitative research that examines cognition. Why not historicize and contextualize our discussion here in those diverse and robust literatures?

QUESTION 3. SAME TERMS, DIFFERENT MEANINGS?

And this brings to mind another question: Do we share similar conceptual vocabularies? Though the terms may be the same—*cognition*, *social*, *research*, *learning*, *bilingual*—even the putative learning object, the L2, can mean very different things to those working from, say, positions informed by cognitive psychology or linguistic anthropology, sociology or social psychology, and discursive psychology or linguistics. This is as it should be for a field as increasingly transdisciplinary as L2 studies; nonetheless, it is worth asking if we are playing the same *language-game* (Wittgenstein, 1953/2001) and, if we can acknowledge that we are not, then asking if there is some way we might contend with that.

QUESTION 4. METHODOLOGY OR METHODOLATRY?

Finally, I was asked to focus my discussion on research methods, specifically, "the methods of inquiry that [I] prefer in [my] own work, . . . their . . . limitations, and what attracts [me to] other methodologies." But I cannot do this absent an actual study and research purpose. I have conducted critical discourse analyses, critical ethnography, and interview studies, but, in doing so, I have used methods not necessarily associated with these traditions, not because I preferred these methods in the abstract but because they were best suited for the particular research project and research questions I had formulated. So I wonder, is it in our interest to discuss method in the abstract? Might a preference

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for quantitative methods, or qualitative methods, or, more likely for the purposes of "bridging gaps," mixed-methods research be putting the cart before the horse? What if we turn our attention to the particularities of our research projects, research questions, and theories and then, in the context of such particularity, formulate our methodology and, in so doing, occupy ourselves not with "methodolatry" (Janesick, 1994) but with epistemology and ontology (Pascale, 2011)? Because it is in the areas of epistemology and ontology—not method—that I believe our attention ought to be. And this leads me to the main part of this contribution.

AN INTERPRETIVIST TURN IN L2 INQUIRY

The engagement with ontology and epistemology that I am talking about in terms of method might be called—in its strong version, at least—an *interpretivist turn* and—in a less strong version, perhaps—a *reflexive turn*. Regardless of label, such a turn would be suitably rigorous, reflexive, and responsive to diverse theoretical positions and conceptions of language and learning, cognition, the social, and so on (see Sfard, 1998). It would embrace methodological and paradigmatic pluralism and hybridity, diverse "styles of inquiry" (Pascale, 2011, p. 141), similar to Howe's (2003, 2004, 2012) notion of *mixed-methods interpretivism*, a metaparadigmatic move that, when taken, immediately recognizes that paradigms are themselves interpretive frameworks and, thus, that sees all social science research—quantitative, qualitative, and mixed-methods research—for what it is: the outcome of the interpretive activity of particular people working from particular disciplinary and theoretical orientations in particular social contexts and historical moments (Giddens, 1976).

From such a perspective, differing paradigmatic positions and theoretical orientations come to offer *different* rather than *competing* perspectives on an object of study—different rather than competing conceptions of what that object might be. Those differences can then be engaged at metaparadigmatic, metamethodological, and metatheoretical levels in ways that reflexively reveal the contours and boundaries of knowledge production and that demonstrate how our conceptions about and our study of a particular phenomenon are inevitably perspectival, as we ourselves as researchers are inevitably implicated in how we come to know and speak of what we study. In short, an interpretivist turn would compel us to avoid the "god trick" that Haraway (1988) has described. It would oblige us to acknowledge that research is the outcome of our participation in particular social practices, practices that are as diverse as they are generically identifiable, disciplinarily and theoretically informed, historically specific, and paradigmatically sited.

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Of the potential ways forward in such an interpretivist turn, one may be with Howe's mixed-methods interpretivism (mentioned previously). Another could be Alvesson and Skoldberg's (2009) reflexive methodology, which I briefly sketch next.

REFLEXIVE METHODOLOGY FOR AN INTERPRETIVIST TURN

Reflexive methodology is characterized by careful, rigorous interpretation and a sustained, thoroughgoing reflexivity. Interpretation "implies that all references . . . to empirical data are the results of interpretation. Thus the idea that measurements, observations, the statements of interview subjects . . . the study of . . . statistics or archival data [in some way reflect or mirror reality] is rejected on principle" (Alvesson & Skoldberg, 2009, p. 9). Methods are selected not in the abstract but always in relation to specific research problems and research objects (p. 8). This is not to say that inquiry proceeds without criteria for high quality: Rigor counts, certainly, but there is a shift from searching for "truth" to developing well-warranted, defensible claims based on empirical data, which at the same time are acknowledged to be the outcome of the researcher's analytic activity.

The possibilities of, and dimensions for, high-quality research are further enhanced by *reflexivity*. But this form of reflexivity does not remain at the level of method; it is "the launching of a critical self-exploration of one's own interpretations of empirical material" (Alvesson & Skoldberg, 2009, p. 9). It is a "problematizing practice" (Pennycook, 2001, pp. 41–45) that includes consideration of the inherent value-laden-ness of research and of representational politics (Giroux, 1992). These interpretations of the interpretation can then be considered for their epistemological and ontological assumptions, for what they offer, for what they do not offer, and for points of contact and commonalities as well as divergences and disjunctures between alternative interpretive frames. With such reflexivity, "the center of gravity is shifted from the handling of empirical material towards . . . a consideration of the perceptual, cognitive, theoretical, linguistic, (inter)textual, political and cultural circumstances that form the backdrop to—as well as [infuse]—the interpretations" (Alvesson & Skoldberg, 2009, p. 9; also see Giddens, 1976; Pascale, 2011).

CONCLUSION

An interpretivist turn essentially engages Giddens's (1976) notion of the double hermeneutic. As Giddens observed, social scientific research stands in a "subject-subject relation" to its object of inquiry (contra a

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"subject-object" relation in the natural sciences); that is, it is embedded in its subject matter: human society (pp. 146–147). Thus, "the creation and reproduction of meaning-frames [i.e., paradigms] is a very condition of that which it seeks to analyse" (Giddens, 1976, p. 158). An interpretivist turn compels a discursive space in which the explicit mediation of such "meaning-frames" can occur. This provides opportunities to engage with issues of ontology and epistemology that are sidestepped at the level of method. It turns theory and method "back on itself" to consider how researchers' "location carries privileges and secures particular forms of authority . . . [regarding] who speaks, under what conditions, and for whom" (Giroux, 1992, p. 222). This applies to epistemologies and paradigms that are well represented in research in language learning and teaching as well as to ones that, unfortunately, are not (e.g., indigenous, feminist, critical, Queer, postcolonial, postmodern, Green, and more), thus offering additionally intriguing possibilities for the sort of interpretive turn that I am suggesting. From such a position, it is acknowledged that there are multiple—not just two—ways of seeing, knowing, and understanding the world and of conceptualizing, researching, and representing phenomena. From such a position, rather than arguing ad nauseam over who is right and who is wrong (i.e., "the ding-dong of point-scoring that sometimes accompanies, and sometimes masquerades as, academic debate" [Edge, 2004, p. 718]), we see what one perspective offers in terms of another, what that other offers in terms of the former, how they diverge, how they may converge, and so on, and we engage *that*.

In sum, the sort of interpretivist turn that I am suggesting is predicated on reflexivity, a reflexivity that extends beyond method to paradigm. In this view, one's paradigmatic commitments, whatever they are, can be maintained, but they are made explicit and are addressed, discussed, compared, and contrasted in a language game that all L2 researchers play, one that centrally concerns epistemology and ontology. An interpretivist turn grounded in such a thoroughgoing reflexivity is one way I believe that a generative, shared understanding across paradigms in L2 studies might be attempted. Contributors to this article (especially Mackey and Bigelow) offer examples of precisely the sorts of reflexivity that I am proposing and the advantages that can derive from it.

Finally, the colloquium has made me aware that if there is any "gap" in L2 studies, it concerns a general lack of consistent consideration among L2 scholars of matters in the philosophy of science, the anthropology of science, and the sociology of scientific knowledge. We would do well to create in our studies, our professional journals, our graduate courses, our graduate programs, and our conferences (!) the space for these sorts of discussions to occur (no mean feat, I know, given ever-tightening page restrictions, journals contending with voluminous numbers of submissions, and courses with already overstuffed syllabi). The study

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of learning, teaching, and the use of L2s has surely reached a point at which sustained and substantive attention to such issues is overdue.

METHODS AND DATA

Tangled Up in Blue

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RESEARCH IN L2 LEARNING AND TEACHING AS A COLLECTIVE ACTIVITY

In conceiving the 2013 AAAL colloquium, Hulstijn and I were inspired by an idea, originally proposed by Ludwik Fleck (see Sady, 2012), that what members of a research community know about their field is, in fact, a collective social activity. If we extend Fleck's (1935/1979) ideas to L2 studies, it becomes clear that, when individuals do research in language learning and teaching, when instructors teach SLA, and when teachers relate research to the classroom, they are part of a community of persons mutually exchanging ideas and maintaining intellectual interaction, a community Fleck called a *thought collective*. Our understandings of the diverse phenomena of L2 learning and teaching is a product of that collective activity—a *thought style*—within the thought collective, or a "picture, which is visible only to anybody who takes part in this social activity, or a thought which is also clear to members of the collective only" (Cohen & Schnelle, 1985, p. 77).

In fearing that research in L2 learning and teaching runs the risk of disintegrating into two irreconcilable approaches to language learning and language use, Hulstijn and I were echoing another of Fleck's (1935/1979) notions: the incommensurability of thought styles, a notion that underlies the far better known work of Kuhn (1962/1996). The incommensurability that we perceived between researchers investigating linguistic-cognitive issues and those working on the basis of sociocultural or sociocognitive views of research in L2 learning and teaching is recognized by Fleck as three differences in thought style. First, echoing Wittgenstein's (1953/2001) concept of the *language-game*, Fleck recognized that the language used by members of a thought collective to describe what appears to them as reality differs from one thought collective to another. For example, the term *L2 learning* has different meanings according to whether researchers consider learning as the incremental accumulation of concepts that are gradually refined and combined with other concepts to form cognitive structures or, alternatively, whether

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they consider learning as movement along trajectories of changing engagement in discursive practices. Second, the questions that members of a thought collective ask arise from within the thought collective; in other words, the phenomena to which members of a thought collective attend are constructs of their thought style. For example, in analyzing a conversation between two people, Mackey has written in her contribution to this article that researchers working with the initial version of the interaction approach focused their attention on meaningful interaction, corrective feedback, and associated interactional features. Working within a different thought collective, although researchers may recognize the importance of interactional modifications, they may choose instead to attend to very different features, including the gender, ethnicity, and age of the participants as well as how power is exercised and resisted in interaction. The third characteristic of incommensurable thought styles, according to Fleck, is a difference in perceptions: What researchers within one thought style see, people working within a different thought style do not see, and thus they think differently about phenomena. As Fleck (1935/1979) wrote:

> The principles of [a different thought collective] are, if noticed at all, felt to be arbitrary and their possible legitimacy as begging the question. The alien way of thought seems like mysticism. The questions it rejects will often be regarded as the most important ones, its explanations as proving nothing or as missing the point, its problems as often unimportant or meaningless trivialities. (p. 109)

In L2 learning and teaching research, though researchers in different thought collectives study the same phenomenon—what Cook and Bassetti (2011) term *bilingual cognition*—some see it as neural activation at the interface between the cortex and the limbic system, whereas others see the genesis of cognition in cultural artifacts, social interaction, and the socioeconomic environment of individuals. Within these different thought collectives, different thought styles envisage the nature of bilingual cognition in their own and very different ways.

METHODS AND METHODOLATRY

A similar understanding of the history of ideas has been put forward by Talmy. In his contribution to this article, Talmy questions whether, in recognizing differences and even incommensurabilities between thought styles, we are not creating discursive space through a rhetoric of difference. Some of that difference may be attributed to the different methods researchers in L2 learning and teaching employ, methods that become ingrained within one thought collective but are considered esoteric

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and misunderstood by researchers in a different thought collective. A noticeable difference between thought styles was presented by Talmy, who recognized that, within a cognitive thought community, quantitative research is the predominant thought style, whereas researchers in a social thought community adopt a predominantly qualitative thought style. Indeed, after many years of teaching a graduate course in research methods, I have found that qualitative research methods including case study, ethnography, grounded theory, and narrative inquiry appeal to students with undergraduate majors in the humanities, whereas numerical methods involving statistical analyses of data from surveys, experiments, and language corpora appeal to students with a background in sociology or psychology. So, yes, in these student preferences I recognize a rhetoric of difference between qualitative and quantitative research methods, what Janesick (2011) called different *habits of mind*, grounded in the personal preferences of researchers and the early training they received.

Do such differences of method lead to irreconcilable differences in understanding the phenomena of language learning and use? There are two cases showing that differences may in fact be reconcilable, and the choice of one method may lead to insights that can then be best studied by a different method. In one case, insights from qualitative research may lead to investigations best undertaken through quantitative comparisons. An example of this comes from the results of conversation analysis applied to crosslinguistic comparisons. Conversation analysts, as Schegloff (1993) argued, have eschewed quantitative analysis in the belief that when analysts count, they are not counting instances of what to the participants are necessarily the same thing. Fox et al. (2009), while accepting that, in same-turn self-repair, speakers are not always doing the same thing, chose to investigate the site where speakers initiate repair within the same turn. They then compared the sites at which speakers preferred to initiate same-turn self-repair across seven different languages. By means of quantitative comparisons across the seven languages, Fox and colleagues found that the sites favoring self-repair initiation within the same turn in the different languages depend to a large extent on the syllabic and morphemic properties of words in the language.

A second example of reconciling separate thought styles illustrates how a quantitative analysis may reveal broad connections between variables while, at the same time, identifying outliers whose atypical performance merits further investigation. An example of just such a quantitative study is Abrahamsson and Hyltenstam (2008)'s study of the relationship between language learning aptitude in adults and children and their eventual nativelikeness. Abrahamsson and Hyltenstam confirmed a correlation between quantitative measures of aptitude and nativelikeness (stronger in adults but weaker in children), but their

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analysis also revealed that the rare nativelike adults all turned out to be exceptionally talented language learners whose unusual ability is worth investigating further by qualitative means.

We can perhaps conclude, then, that although cognitive research is predominantly quantitative, whereas qualitative methods prevail in social research, neither quantitative nor qualitative research methods should be taken as unquestioned thought styles within their respective thought collectives. Both quantitative cognitive research and qualitative social research may produce results best investigated with a different methodology. In other words, as Talmy has argued, we should be careful that our own training and formative experiences as researchers do not lead to a habitual attachment to a single habit of mind resulting in methodolatry.

HOW DO WE KNOW WHAT WE KNOW?

If the association between thought style and thought collective is strong, then we can ask if one way to bridge the gap between social and cognitive approaches is for researchers in L2 learning and teaching in each thought collective to adopt different research methods (i.e., different thought styles). If the question of methodology is essentially "How do we know what we know?" then if we change *how* we know, does that alter *what* we know? In Lantolf's contribution to this article, he has proposed that the knowing-how question (a question of epistemology) is not much of a question; instead he asks us first to settle what he calls the "ontological problem"—the question of what human cognition is. Strictly speaking, this is not a question of whether human cognition exists, which *is* an ontological problem, but rather a question of what we understand by human cognition—a problem of definition that is, in effect, closely related to the epistemological problem. In Lantolf's terms, do we understand human cognition as occurring isolated within the brain of an individual or do we instead consider it as a dialectical relationship between the individual brain and the social world in which we live, a dialectic mediated by language, culture, and cultural artifacts? This is, then, a question of our understanding of cognition, and surely, therefore, how we arrive at that understanding is greatly influenced by the means with which we pursue it.

In the cultural-historical school of psychology that Lantolf advances, the genetic method of researching cognition (i.e., higher mental functions) is quite different from other schools of psychology. The genetic method emerged as Vygotsky's response to the dualism of mind and body put forward by Descartes and others, and it involves studying the processes through which human cognition develops (rather than the outcome of those processes). In the genetic method, the dynamic of cognitive

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development can be studied in several aspects: the evolutionary development of the species (phylogenesis), the historical development of culture and tools, the development of an individual person (ontogenesis), and the continuous formative activity underlying cognition (microgenesis). Genetic investigations that aim to discover the origin, trend, rate, direction, and pattern of cognitive development describe a process very different from studying a given cognitive phenomenon or a particular behavior for its own sake. Genetic investigations are much more extensive than other investigations because they recognize that human cognition arises from interaction between biological and cultural inheritances.

To summarize, the concept of cognition espoused by Lantolf and other advocates of cognitive-historical psychology is indeed very different from the concept of cognition taken as the basis of research by psychologists who do not attend to the cultural and historical development of cognitive phenomena. The difference between the two is, I have argued, a difference of ways of knowing, of method. In other words, the difference between the two approaches is a question of epistemology, not ontology.

THOUGHT STYLES

Methods of inquiry that L2 researchers prefer differ from one thought collective to another. Because learning and using a L2 is a vast and vastly complex phenomenon, any way of investigating the phenomenon inevitably involves attending to some aspects and disattending from others. Selective attention to different phenomena is the principal way in which thought collectives differ, and it is an important part of Fleck's theory of thought style, which he defined as

> *directed perception, with corresponding mental and objective assimilation of what has been so perceived*. It is characterized by common features in the problems of interest to a thought collective, by the judgment which the thought collective considers evident, and by the methods which it applies as a means of cognition. (1935/1979, p. 99)

The way that different researchers attend to different aspects of L2 learning and even how the same researcher comes to attend to different aspects have been described by researchers who introspect on their own research process. In her contribution to this article, Mackey, for instance, has recounted a shift in her own attention to the nature of interaction involving language learners. From a research posture of initial disattention from the social factors underlying interaction, Mackey (e.g., in Philp and Mackey, 2010) refocused her attention to consider the relationships among learners and how they impacted what learners were

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willing and able to listen and attend to, and thus what they produced. Mackey's attentional change parallels the recognition by a number of authors of the different roles for the human subject in language learning experiments, depending on whether researchers focus narrowly on the phenomenon of interest or whether they expand the focus of their attention to include their subjects' social and political roles with other subjects, with the researcher, and with the society beyond the walls of the laboratory. For instance, in her early studies of the cognitive process of reading in a foreign language, Cavalcanti (1983) found that her subjects not only reported their thoughts about reading but also framed them in the social context of their roles as subjects in a research study. In more recent applied linguistic studies of metaphor, which earlier studies had interpreted narrowly as an index of cognition alone, Zanotto, Cameron, and Cavalcanti (2008) argued that metaphor must be understood as social and situated, not merely a reflex of thought. Reflection by these investigators on interaction, reading, and metaphor reveals the importance of attending to the social contexts of human subjects in studies of L2 learning and teaching because they index how subjects position themselves with respect to their social and political roles with other subjects, with the researcher, and with the society beyond the walls of the classroom or laboratory. As Preston (1989) pointed out, even in studies of classroom interaction, researchers should attend to their subjects' membership in different communities inside and outside the classroom.

The habits of mind—the phenomena to which researchers attend and how they analyze and interpret them—characterize a thought style. But neither thought styles nor thought collectives are immutable, and the examples of perspectival change reported by Mackey (this article), Cavalcanti (1983), and Zanotto et al. (2008) show how changes in the thought styles of individuals index historical development within a particular thought collective. What this implies is that, instead of looking at the differences between social approaches and cognitive approaches to research in L2 learning and teaching (the thought collectives), it is more fruitful to consider the thought styles of individual investigators, their activities, their habits of mind, and how they position themselves within the social process of investigation and publication. This is the perspective taken by Talmy in proposing an interpretivist turn in L2 inquiry, in which explaining anything says as much about the explainer as about the thing being explained.

AN INTERPRETIVIST TURN IN L2 INQUIRY

In his contribution to this article, Talmy has argued that all research in the social sciences is the outcome of the interpretive activity of particular

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people working from particular disciplinary and theoretical orientations in particular social contexts and historical moments. In the previous section, I have tried to address disciplinary and theoretical orientations of particular researchers in L2 learning and teaching, but let me now turn to two other issues within an interpretive perspective that have to do with the world outside the confines of the discipline: How is the work of researchers in L2 learning and teaching disseminated, and how is their work interpreted in the social world?

Nobody was born an applied linguist, and there are very few undergraduate degrees in applied linguistics, so most of us come from backgrounds in psychology, linguistics, literature, or language teaching—a diversity captured in Bob Dylan's words, "some are mathematicians, some are carpenters' wives" (Dylan, 1975, track 1). We read literature in fields that interest us and disseminate our research in journals that we read, and, as a result, the thought collective reproduces itself. In our field, one journal defines itself as "a scientific journal dedicated to the understanding of language learning broadly defined," whereas another journal broadcasts its mission as publishing "research and discussion about the learning and teaching of foreign and second languages." The first journal mentions science, and the second mentions teaching; and, perhaps, one journal appeals more to mathematicians, the other to carpenters' wives. Whatever the readership, however, the economics of publishing may restrict the length of articles that any journal can print, and both journals announce on their Web sites that the length of articles they publish is generally less than 10,000 words. Such a length requirement may restrict publication vehicles for authors using interpretive methods of ethnography, case study, narrative inquiry, and even meta-analyses, while it may impose few constraints on reports of experiments, archival analyses, or other methods in which numerical representation of L2 phenomena restricts consideration of the multiple contexts in which languages are learned and limits attention to differences among learners.

A second issue raised by the interpretive approach is how research, once published and disseminated, influences the social world of those human subjects who participate in it and those who read it. As Giddens (1984) first observed, a difference between the social sciences and the natural sciences is that, because the work of social scientists is embedded in its subject matter—human society—their work (intentionally or unavoidably) enters constitutively into the social phenomena they describe. In other words, whereas the natural scientist maintains a subject-object relation with phenomena, for the social scientist, the relation is a reflexive one of subject-subject. Some researchers take an active stance in the relationship with their subjects and work to uncover ways in which the context of language learning creates, reproduces, and provides opportunities for resistance to dominant power and ideology.

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In some cases, a researcher's values may be fundamentally pessimistic. For example, some critical researchers identify sources of oppression and ground their analyses in the belief that we live in a fundamentally unjust world. For others, critical research is founded on the view that knowledge of the connections between local educational practices and societal ideologies may further an individual learner's resistance and struggle against the preponderant influence of harmful public policy. Taking a less critical view but nonetheless doing research to better language teaching and learning practices is the object of action research, a form of self-reflective inquiry often undertaken by teachers to improve the effectiveness of their teaching, their understanding of their teaching practice, and the relation between their own teaching and the institutional context in which they teach.

One argument against conceiving of research in this way as an essentially pragmatic undertaking to improve the lives of language learners or the effectiveness of teachers is that such limited goals of inquiry may lead to only a partial understanding of the phenomenon. Because critical inquiry and action research are essentially normative, researchers working within these domains take both a moral and a pragmatic stance with respect to language learning. The moral stance is that certain aspects of people's use and attitudes toward languages and their speakers are unjust, inequitable, unfair, or just plain wrong, and the pragmatic stance concerns what to do about it. Normative judgments about social situations of language use are, however, located within the political and cultural context of the critic, such that criticizing anything says as much about the critic as about the phenomenon being criticized. A second argument against research as a pragmatic undertaking is that by focusing on the solution of a specific problem, researchers may be unaware of the context of the problem and the unintended consequences of their intervention. The safest goal for critical researchers is perhaps, as suggested by Habermas (1968/1971), not to control social processes or even to influence the decisions that teachers, learners, and institutional authorities make in any determinate sort of way but rather to initiate public processes of self-reflection.

CONCLUSION

I have argued that the methods and data that characterize L2 learning research from a cognitive or a social point of view are differences in thought style prevalent within different thought collectives. I have tried to recast those differences as indexes of historical development within research in language learning and teaching. Because individuals from different thought collectives chose to attend the 2013 AAAL colloquium in Dallas, where they eloquently addressed the issues that apparently separate them, their

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discussions revealed that understanding one another includes misunderstanding one another in a different sense. Misunderstandings are inevitable because habits of mind—observation habits, habits of analysis, habits of interpretation, and habits of dissemination—differ from one thought collective to another. However, as Fleck (1935/1979) put it, communication "never occurs without a transformation, and indeed always involves a stylized remodeling, which intracollectively achieves corroboration and which intercollectively yields fundamental alteration" (p. 111). Maybe Bob Dylan put it better: "We always did feel the same / We just saw it from a different point of view / Tangled up in blue" (Dylan, 1975, track 1).

Part 3: Unsolved Problems and Unasked Questions

COGNITIVE *AND* SOCIAL LANGUAGE USAGE

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Language is essentially human. It is the crowning accomplishment of our social and cognitive competences. Language bridges society and cognition. It is a distributed, emergent phenomenon. People and language create each other, grow from each other, and act and change under the influence of each other. Language and cognition are mutually inextricable; they determine each other. Language has come to represent the world as we know it; it is grounded in our perceptual experience. Language is used to organize, process, and convey information from one person to another, from one embodied mind to another. Learning language involves determining structure from usage, and this, like learning about all other aspects of the world, involves the full scope of cognition: the remembering of utterances and episodes; the categorization of experience; the determination of patterns among and between stimuli; the generalization of conceptual schema and prototypes from exemplars; and the use of cognitive models, metaphors, analogies, and images in thinking. Language is used to focus the listener's attention to the world; it can foreground different elements in the theatre of consciousness to potentially relate many different stories and perspectives about the same scene. What is attended is the focus of learning, and so attention controls the acquisition of language itself. The functions of language in discourse determine its usage and learning. Language structure, language acquisition, language processing and usage, and language change are similarly inseparable: They are facets of the same complex, adaptive system.

So each of us, whatever our theoretical background, shares a fascination with language, and we try to understand it. We ask our questions,

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so many of them, and so interesting. We do our research. Our diverse questions require different methods. Language learning diaries, functional magnetic resonance imaging (fMRI) scanners, analyses of service interactions using conversational analysis (CA), introspection, visual world eye-tracking, classroom interaction recordings, computer simulations, artificial grammar learning experiments, billion-word corpora of usage, questionnaires, dynamic assessment, event-related potentials (ERPs), think-alouds, feedback manipulations, *n*-back tasks, psychometric batteries, error analysis, longitudinal corpora, laboratory experiments, classroom field experiments, ethnographic research, agent-based modeling, contrastive analysis, brain connectivity analysis, dynamic systems analysis, behavioral genetics, idiographic and nomothetic approaches, thick and thin descriptions, emic and etic approaches . . . are all usefully applicable techniques, but they are useful for different things. Distrust introspection as a valid index of the working of the prefrontal cortex and anterior cingulum in bilingual code switching. There is a reason we have neuroscience. Distrust armchair analyses of language change. There is a reason we have corpus linguistics. Distrust parental recollections of their child's language 20 years prior. There is a reason we make dense longitudinal recordings of language interaction. Distrust current computer models of emotional interaction. There is a reason we prefer people to chatbots. Distrust overzealous current technology: Do not expect rich cultural competence from computerized language instruction or solitary infants to learn language when sat in front of a TV. We are social beings. Different research methods suit different purposes. Personally, I have learned much about language from my children, from laboratory experiments, from connectionist modeling, from learning a L2, from analyzing the British National Corpus, and from behavioral genetic research, but differently so.

As we research all of our questions, we will incrementally develop a better understanding of language. But if we do it simply in the way that I have listed pieces of the enterprise here, it will be piecemeal theory. We need an additional understanding of how the pieces fit together, interacting in space and time over many different levels of granularity and timescale. Distrust any theory that claims that you can comprehensively study a component in isolation: syntax separate from lexis or semantics, form from function, representation from processing, diachronic from synchronic, knowledge from experience, behavior from brain, competence from usage, and so on (N. C. Ellis & Larsen-Freeman, 2006). Especially pertinent here is the social-cognitive gap. Such partitioning leads to theoretical, ontological, and social isolation; self-aggrandizement; and autistic hostility. Diversity is powerfully creative if there is chance of interaction (Darwin, 1859/1928; Holland, 1998; Page, 2008, 2010). I am encouraged by the multiple perspectives currently represented within usage-based approaches to language (Behrens, 2009; Bybee,

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2010; N. C. Ellis, O'Donnell, & Römer, 2013; Robinson & Ellis, 2008; Tomasello, 2003; Trousdale & Hoffmann, 2013). These hold that we learn language while engaging in communication—the "interpersonal communicative and cognitive processes that everywhere and always shape language" (Slobin, 1997, p. 267). Some of the basic tenets, many of them explicitly addressed by de Saussure (1916/1983), include the following:

1. Language is intrinsically symbolic, constituted by a structured inventory of constructions as conventionalized form-meaning pairings used for communicative purposes.
2. Language is intrinsically linked to human cognition and processes of perception, attention, learning, categorization, schematization, and memory.
3. Adult language knowledge consists of a continuum of linguistic constructions of different levels of complexity and abstraction. Constructions can comprise concrete and particular items (as in words and idioms), more abstract classes of items (as in word classes and abstract constructions), or complex combinations of concrete and abstract pieces of language (as mixed constructions). No rigid separation exists between lexis and grammar.
4. Constructions may be simultaneously represented and stored in multiple forms, at various levels of abstraction (e.g., concrete item: *table* + *s* = *tables* and [Noun] + [morpheme *-s*] = plural things).
5. Constructions can thus be meaningful linguistic symbols in their own right, existing independently of particular lexical items. Nevertheless, constructions and the particular lexical tokens that occupy them attract each other, and grammar and lexis are inseparable.
6. Language structure emerges ontogenetically from usage in particular contexts. Development is slow and gradual, moving from an initial reliance on concrete items to more abstract linguistic schemata. This process is dependent on the type and token frequencies with which particular constructions appear in the input. Storage of wholes depends on token frequency; the development of abstract linguistic schema depends on type frequency.

From analyses of large usage corpora, we can analyze the latent structures of language and their roles in the associative and cognitive learning of language (N. C. Ellis et al., 2013). This is the stuff of cognitive psychology, associative learning theory, and corpus linguistics. But in addition to construction forms and their frequencies, there are their meanings—embodied, attended, consciously formed in dialogue and dialectic, and situated and encultured in social and educational interaction. Usage-based theories hold that an individual's creative linguistic competence emerges from the collaboration of the memories of all the meaningful interactions in his or her entire history of language usage.

Cognitive linguistics (Croft & Cruise, 2004; Langacker, 2000; Robinson & Ellis, 2008) provides detailed analyses of how language is grounded in our experience and our physical embodiment, which represents the world in particular ways. The meaning of words in languages and how

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they can be used in combination depends on the perception and categorization of the real world around us. Because we constantly observe and play an active role in this world, we know a great deal about the entities of which it consists. This familiarity is reflected in the nature of language. Ultimately, everything we know is organized and related to our other knowledge in some meaningful way, and everything we perceive is affected by our perceptual apparatus and our high-level motor control and motor apparatus: Spatial language is grounded in the visual processing system as it relates to motor action, the multiple constraints relating to object knowledge, dynamic-kinematic routines, and functional geometric analyses (Coventry & Garrod, 2004). Action elements relating to hand posture, joint motions, force, aspect, and goals are all relevant to linguistic distinctions (Bergen & Chang, 2012). Meanings are embodied and dynamic (Spivey, 2006); they are flexibly constructed online. Here we have the embodied, dynamic mind of modern cognitive science (A. Clark, 1998; Rosch, Varela, & Thompson, 1991).

Shared attention, shared cooperative activity, and shared cognition are key to meaningful language usage. In their first 2 years, infants develop their capabilities of attention detection (gaze following), attention manipulation (directive pointing), intention understanding (the realization that others are goal directed), and social coordination with shared intentionality (engaging in joint activities with shared interest, negotiating meanings), and these processes are central in child language acquisition (Tomasello, 1999, 2008).

The nature of language follows from its role in social interaction. Social interactions are typically characterized by what philosophers of action call shared cooperative activity (Bratman, 1992) or joint actions (H. H. Clark, 1996). Joint actions are dependent on what might be broadly called shared cognition, a human being's recognition that she can share beliefs and intentions with other humans. Thus usage-based approaches emphasize how language is learned from the participatory experience of processing language during embodied interaction in social and cultural contexts in which individually desired outcomes are goals to be achieved by communicating intentions, concepts, and meaning with others. Conversation partners scaffold and coconstruct meanings. Socially scaffolded noticing (Schmidt, 1990) solves Quine's (1969) problem of referential indeterminacy and builds so much more. The dynamics of language learning are inextricably linked to the dynamics of consciousness, in neural activity and in the social world as well (U. Frith & Frith, 2010). Input to the associative network is gated by consciousness, and consciousness is coconstructed in social interaction (N. C. Ellis, 2005; C. Frith, 2010). In these ways, the input to the associative network is socially gated (Kuhl, 2007).

Language—and language learning—is ever thus. It takes place in social usage, involving action, reaction, collaborative interaction, intersubjectivity, and mutually assisted performance (Lantolf & Thorne, 2006).

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Speech, speakers, identity, and social relationships are inseparable (Lee, Mikesell, Joaquin, Mates, & Schumann, 2009; Norton, 1997; Tarone, 2007). Activity theory emphasizes how individual learning is an emergent, holistic property of a dynamic system comprising many influences: social, individual, and contextual. Action provides a context within which the individual and society, mental functioning, and sociocultural context can be understood as interrelated moments (Wertsch, 1998). The associative learning network is culturally gated. Tomasello's constructionist approach to language unites with his research in comparative primate cognition, the unique place of social cooperation in humans, and the Vygotskian intelligence hypothesis, whereby regular participation in cooperative, cultural interactions during ontogeny leads children to construct uniquely powerful forms of perspectival cognitive representation, including language itself (Moll & Tomasello, 2007). The last 40 years have seen huge progress in research into social cognition, and within social cognitive neuroscience there is now a rich understanding of the role of implicit and explicit knowledge in social cognition (C. D. Frith & Frith, 2008), of the role of consciousness and metacognition in social interaction (C. Frith, 2010), and of the brain mechanisms involved in these processes (U. Frith & Frith, 2010). From the very name of the field, you can tell that there is no social-cognitive divide within contemporary social cognitive neuroscience.

How should we research this usage? If language learning is in the social cognitive linguistic moment, we need to capture all these moments so that we can objectively study them. We need large, dense, longitudinal corpora of language use with audio, video, transcriptions, and multiple layers of annotation for data sharing in open archives. We need these in a sufficiently dense mass that we can chart learners' usage history and their development (Tomasello & Stahl, 2004). We need them in sufficient detail that we can get down to the fine detail of CA analyses of the moment (Kasper & Wagner, 2011). Brian MacWhinney has long been working toward these ends, first with the Child Language Data Exchange System (CHILDES; MacWhinney, 1991) and then with Talkbank (MacWhinney, 2007). These projects have developed various computerized language analysis (CLAN) tools for computer analyses of large bodies of data, right down to, in collaboration with Johannes Wagner, tools for a fine-grained CA bank (http://talkbank.org/CABank). With these types of data, we can study the cognitive and the social. This way the future lies. These are huge contributions to language acquisition research, though, at the moment, the data are relatively sparse. We need much more, especially for studies of L2 learning and teaching. We need big, dense, longitudinal data sets that we can all observe; it is up to the field to contribute to these open archives.

We need theoretical integration, too. Cognition; consciousness; experience; embodiment; brain; self; and human interaction, society,

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culture, and history are all inextricably intertwined in rich, complex, and dynamic ways in language. We require additional perspectives on dynamic interactions between levels, perspectives provided by approaches such as complex adaptive systems (N. C. Ellis & Larsen-Freeman, 2009), dynamic systems theory (de Bot, Lowie, & Verspoor, 2007; N. C. Ellis, 2008; Spivey, 2006), and emergentism (N. C. Ellis, 1998; N. C. Ellis & Larsen-Freeman, 2006; MacWhinney, 1999).

At the AAAL 2013 colloquium in Dallas, I was asked to talk from a cognitive perspective along with Martha Bigelow, who was presenting for the social side. I knew how lucky I was in this pairing: *Language Learning* had previously published her excellent monograph *Mogadishu on the Mississippi: Language, Racialized Identity, and Education in a New Land* (Bigelow, 2010). At the end of her presentation, I was asked if I wanted to respond. Postperformance zonked and worried by the adequacy of my own contribution and the large, varied, and potentially awesomely critical audience (you, maybe?), I was not prepared for this. But our joint reaction was easy. In the moment, we faced each other, checked the trust in each other's eyes, smiled, shook hands, and embraced. It emerged in that cognitive-social moment: There was no cognitive-social divide. For my part, it was easy to embrace the position of someone who blends together approaches from focus groups, sociocultural analysis, storytelling, psycholinguistics, and educational evaluation, as they are appropriate in dealing with very real educational and social issues of immigration. As Ortega noted in her discussion of our presentations at the colloquium, we can go beyond simple dichotomies if we choose to imagine better, we can strategically try to undo these outdated notions, and we can socially coconstruct something superior. There is no social-cognitive gap.

BLENDING SOCIAL AND COGNITIVE RESEARCH TRADITIONS IN LANGUAGE LEARNING AND TEACHING

A Matter of Mentoring and Modeling

Martha Bigelow
University of Minnesota

There is a story behind every study, and it seems as though most studies are a series of compromises, with hindsight being 20-20. We always miss something or discover limitations, and I hope this piece offers encouragement for researchers to try doing research in new ways, with different populations, in different settings, and using new, hybrid, or mixed methodologies. There are many ways of knowing, and we should push our use of current methodologies to make our analyses better and

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to explore possibilities for blending research traditions in ways that give us more insight into phenomena studied in the fields of language learning and teaching.

I would like to take the colloquium and this article as opportunities to discuss some of the ways the epistemological polarization of our field between cognitive and social approaches to inquiry limits the sorts of questions we allow ourselves, and our students, to ask. I also describe the damaging impact this polarization of research can have on our students. Our students are in the process of making sense of the wide range of research they read and making decisions about who they want to be as researchers. They are in the process of choosing their research contexts, their questions, and their methodology. Unfortunately, this process often results in students leaving behind their teacher selves or their selves as cultural beings.

Personally, I am not loyal to any research method, but I have not done studies that fully combine approaches in a balanced way. In other words, I have done work in the cognitive camp and brought in minor data sources analyzed in a qualitative way, particularly in my initial years as a researcher. Or, more increasingly in recent years, I have done intensely ethnographic work that also attempts to identify certain data points that have implications for language learning and teaching. Being epistemologically open does not necessarily mean a researcher is able to merge the two, and that is what I wish we were able to do more of and do better. The main reason for bridging methodological approaches is to make our research stronger and our findings more grounded and, hopefully, more useful to others. The training and the investment needed to do this sort of research, however, are different than what the academy typically offers.

I began to realize this through my work in our Somali communities of Minnesota with Elaine Tarone and Kit Hansen (Tarone, Bigelow, & Hansen, 2009). The first study we did was cognitive to the core (Bigelow, delMas, Hansen, & Tarone, 2006). But in gaining access to the research participants, I had to do a lot of work in the Somali community. I had to become known, build trust, and develop relationships to gain access. As I did the work it took to get my data, I changed as a researcher. I began to learn about the perspectives of Somali transnationals in an urban setting and the political and policy contexts of language learning and education. My cognitive study of language learning started to get complicated because of my attachments to the community, my new ties with community activists, community concerns, and individuals who were asking me to help them learn to read. These experiences led to new research questions and methods that allowed me to leverage all of my teaching and intercultural skills—skills that were minimally tapped in our study. In this qualitative and ethnographic work (e.g., Bigelow, 2007, 2010, 2011; Bigelow & King, in press), I found that I was challenged

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by four teenage Somalis to understand issues of schooling, literacy, culture, and religion, among other things, while, at the same time, trying to systematically track language learning. There was no way to link their performance to any intervention. The only thing I kept noticing about the girls was how prior schooling and engagement with print literacy mattered time and time again. The thing I noticed about myself was that I was becoming an engaged researcher, and, for me, my research was beginning to take an advocacy turn. Even after this transformation, the issue remains that, although I am not loyal to any one research paradigm, I embody both, but I do not bring them together well. The challenge I see is the common desire to contribute to generalizable research and theory building in language learning and teaching, while also using everything the field knows about the importance of identity, ideology, and context.

I have seen many of our doctoral students, who are our next generation of researchers in language learning and teaching, try to combine approaches. I would like to describe some of their challenges; I think they will resonate with many readers. It is common for students in our doctoral program in the Department of Curriculum and Instruction at the University of Minnesota to be deeply interested in research on language learning and teaching. Then, through their coursework, they read into fields such as cultural studies, anthropology, and immigrant education through lenses of social justice. We have many classes that give them formal training to do this, and our faculty members support any research design that seems to match the students' research questions, including research that uses quantitative, qualitative, or mixed methods as well as ethnographic, hermeneutic, and phenomenological approaches. But what occurs is that, as our students design these studies, they struggle to create designs that would produce research that can dialogue with established cognitive research in language learning and teaching, which is what they often, but not always, hope to do. For example, their doctoral dissertation research, in an effort to be situated, culturally sensitive, and aware of learners as individuals (not just as a L1 group or a nationality), may not have enough participants to allow for inferential statistics and theory building in the cognitivist realm. This tendency results in students choosing designs that are not quantitative, yet hoping to produce knowledge that contributes to ongoing cognitivist dialogues, particularly dialogues about language development. I will give two examples of students who are doing research in language learning and teaching but are not using quantitative research designs. Both are presently in the middle of the process of planning their doctoral dissertation studies.

One of our students became very interested in using tasks to promote more linguistic complexity among learners. Her research questions emerged from issues of practice, and she wants her study to have implications for practice. But she found it hard to reconcile the practical nature of language teaching and the task-based learning research that

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often isolates task features with psycholinguistic rationale for task complexity, accuracy, fluency, and cognitive demand (e.g., Robinson & Gilabert, 2007; Skehan & Foster, 2007). This student wished to account for what happens when learners do their tasks collaboratively—what happens from a sociocultural standpoint. In other words, she was interested in what happens among learners, how they perceive one another, and how they jointly influence the way the task unfolds. She wanted to know what sociocultural factors and task features contribute to language development. If one thinks about all a researcher needs to know to do this sort of study, it is daunting (although see Storch, 2013, for a review of worthy attempts). She needs to know about task design and analysis, language analysis, activity theory, and how to gather high-quality field notes and interviews. This student took a class in sociocultural theory in our department but had to read deeply into the application of activity theory to L2 learning and teaching mainly on her own. She had multiple classes in SLA and applied linguistics but needed to figure out how to apply this body of knowledge to her tasks and analysis. It was challenging to figure out a manageable classroom research design for this classroom language learning research: How many dyads did she need to be able to track language learning? Once given access to an intact class, how many tasks could she logistically carry out without interfering too much with the curriculum?

These challenges resulted in explorations of a time series design to track language learning, which was discounted after consultation with a statistician. Presently, she is analyzing the language-related episodes and language complexity of two dyads, mostly qualitatively. The student worries that her work may not be taken seriously by those who study task-based learning, that she can only talk about language learning through the particular cases, that her qualitative results will not be generalizable, and that it is very challenging to link the interview and observational data to language production without a high level of speculation and interpretation. As a field, we have a problem when a student and her committee are making decisions on study design that largely reifies the ideological divide in our field, instead of allowing the student to leverage her dual interest in language learning as both a developmental and a social process. Sometimes the feasibility of a study carried out in a classroom affords fewer opportunities to gather longitudinal L2 acquisition data, which may lead the researcher to carry out more microanalyses on dyadic interaction or more out-of-class interviews on the tasks carried out in the class.

Another example is a student who knew she wanted to study L2 writing by international students in higher education. She wanted to systematically track language development alongside international students' experiences in their undergraduate classes. She was drawn to both language socialization as well as traditional research frameworks

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in language learning and teaching. She gathered data in students' undergraduate writing class. She attended every class period three times a week for 15 weeks and now has all their writing assignments and about 45 hours of classroom video. She also has multiple interviews with each of her four focal participants and recordings from all of their writing conferences. This is an enormous amount of data, and this student would tell you that she sometimes feels like she has five dissertations going on. She told me that she felt that there are not a lot of studies that do what she is trying to do or that perhaps she is trying to do something that is overwhelming. Language socialization (e.g., Duff & Talmy, 2011) has allowed her to see that one of the main purposes of the class is to socialize students into critical thinking and developing arguments that critically present multiple viewpoints. But her dilemma is pinpointing how this instructional goal is reflected in students' language use. Students are learning macro writing and speaking skills related to critical thinking, which is built on countless linguistic skills used in myriad ways depending on genre and topic, among other things (e.g., Cumming, 2013). She is wondering what to quantify without losing sight of qualitative aspects of socialization into critical thinking. This is much more than tracking discrete linguistic features. Language socialization is also taking new and multiple directions in studies of L2 learning, and this student, as a novice researcher, is tentatively making analysis decisions that may be uncommon in the emerging applications of language socialization. This student wisely conceded that if you do one or the other—look at context or look at language learning—you can ignore a lot. If you are doing both, you have to look at it all, and this is hard to do as a beginning researcher. The data gathered for this dissertation will include multiple examples of how students engage in critical thinking, or attempt to offer evidence of their critical thinking. These instances can be analyzed in depth within and across a small number of participants, but it would be difficult to go back and gather longitudinal L2 acquisition data from more participants. For this reason, it is crucial to decide early on whether a certain number of participants are necessary to carry out an analysis to answer a predetermined SLA question.

I am sure that both of these students will produce excellent dissertations, but the ideological divide they are navigating causes them uncertainty in terms of who the audience is for their work. They both worry whether they will be convincing to anyone as they try to reconcile the issue of who their primary audience is. They worry about fitting incongruous theories, methodologies, and research contexts together in ways that produce useful knowledge for our field.

The methodological and epistemological challenges our students face are the result of the traditions or heritage in our field. These traditions often support a damaging dichotomy of ideologies as well as a lack

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of role models who do the work our students hope to do. What can we do in this situation? I believe the best we can do is to trust our students' new reading of the field and defend their choices to do innovative work that bridges cognitive and social approaches. What I see us doing is something different. Students work hard to acquire new habits of mind as they learn new discourses and cultures in academia. Yet we project methodological fear in them, which limits their willingness to take risks or to listen to their own (often vast) experience with language teaching and learning. I often wonder what would happen if we were to allow ourselves and our students to stay true to what we care about and what we want to research and to use all of our good sense and research tools to do this, regardless of the ideological divides in the field. One could say that an apprentice to research in L2 learning and teaching must submit to a socialization process that leads him or her to more confidence as a researcher. However, we must remember that socialization goes both ways. Our students can and should change us as we do our own research and seek wisdom as we guide theirs.

We are all actors in the stories behind the studies we do, and my plea is for established researchers to try to bridge the divide themselves and to support new researchers to take risks in their own work. I trust that the field will move forward in productive ways with more attempts to bridge the cognitive and social gap in research on language learning and teaching.

THE COGNITIVE-SOCIAL GAP

Multiple Understandings, Hopeful Commensurabilities

Lourdes Ortega
Georgetown University

In their contributions to this article, Nick Ellis and Martha Bigelow have chosen to highlight bridge building and bridge traveling, respectively. With these choices, each offers hopeful understandings of the cognitive-social gap. Yet, each is also cautious. Ellis proposes complementarity protected by a certain epistemic independence, which is to be achieved by a disciplinary division of research labor. Bigelow encourages bridge traveling through epistemic blending, but she also painfully details the vulnerabilities and risks involved in the enterprise.[4] There is much agreement in their hopeful yet cautious framings of the issues. All the same, there are also differences in their understandings of the gap and in the solutions they offer. In this reflection, I align myself with a similar spirit of cautious optimism. I want to emphasize the multiplicity of understandings of the alleged cognitive-social gap and affirm the

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search for hopeful moments of commensurability across cognitive and social epistemic lines.

RESPONDING POSITIVELY TO THE GAP: BRIDGE BUILDING AND BRIDGE TRAVELING

For Ellis, language and language learning and use are at once cognitive and social. Therefore, their study must be tackled from both cognitive and social perspectives. In other words, it is the very nature of the objects of study that creates the justification for bridging cognitive and social inquiry; the need for bridge building is thus rationalized as an ontological imperative. Ellis stipulates, however, a disciplinary division of labor: Multiple social and cognitive dimensions of language learning and use dictate different granularities and timescales for research and thus require different research questions and demand different methods. This being so, cognitive and social dimensions must be addressed by different communities—or in Fleck's (1935/1979) sense, by different thought collectives and their resulting thought styles. Thus there is epistemic independence in the complementarity Ellis proposes. He also sees a need for theoretical integration and finds it in usage-based approaches. This family of theories stipulates that the cognitive and the social, the abstract and the physical, and even the empirical and the meditative create layers of phenomena that, at the metatheoretical level, are interdependent and must be integrated into explanations of human cognition (MacWhinney, 1999; Varela, Thompson, & Rosch, 1991). When complementarity in epistemic independence is looked at from this integrative metatheoretical perspective, a bridge is built that invites the traveler to cease viewing the cognitive-social gap as a threat.

For Bigelow, too, the relevant phenomena to be explained include social and cognitive dimensions. She feels, however, accountable to bridging the social and the cognitive by blending the two dimensions in her own research praxis—that is, within single studies and within the same research program. Thus, her account bears witness to researchers who respond to the gap by trying to close it through bridge traveling. Bigelow shares her arresting awareness of the vulnerabilities and risks involved in these efforts. For senior researchers like her, and even more so for junior researchers and students, bridge traveling via epistemic blending, she tells readers, can be a tremendous challenge. One could argue that the motivation for epistemic blending of such proportions goes beyond ontological understandings of the object of study and is fueled by a bottom-up, problem-oriented enticement (or felt necessity) to transcend, in practice, whatever cognitive-social dichotomies appear to be in opposition. In Bigelow's case, studying the underserved population of Somali adolescents in urban Minnesota challenged what she and

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her colleagues thought they knew about language learning and use (Bigelow & Tarone, 2004) and what they decided they needed to know, and it commanded a broaching of social and cognitive dimensions of the problem at once. Her bottom-up justification for the need to travel cognitive-social gaps resonates with views of applied linguists who argue that research should begin with an analysis of both the given problem that needs to be understood and transformed and the anticipated consequences of the given research, with the choice of methods and theories subordinated to that analysis. Far from being monolithic, these views are epistemologically diverse and range from critical pragmatism to critical realism, from critical pedagogy to critical theory, and even to poststructuralism (see discussions in Bygate, 2005; Corson, 1997; Crookes, 2005; McNamara, 2012; Pennycook, 2010).

NOT TWO BUT MULTIPLE UNDERSTANDINGS OF GAP BRIDGING

Ontological bridges might be built to look quite different. Ellis's is one example. Lantolf, in his contribution to this article, offers a quite different example, choosing a different theory that helps build another ontological bridge: Vygotskian sociocultural theory. His choice of theory helps bridge the gap organically and naturally, to the point that no gap is felt, and no bridge is needed. This is done not through complementarity, however, but by recognizing that the dialectic between social activity and mental activity is mediated by language and cultural artifacts. The aspiration to undo ontological dualisms and dichotomies is shared by Vygotskian sociocultural psychology and by usage-based approaches (see N. C. Ellis & Larsen-Freeman, 2006). The dualisms, however, are overcome by different means, either at the intratheoretical or at the metatheoretical level.

Bridge traveling, or the integration of the cognitive and the social within a single study or within the same research program, can also occur in different ways. Bigelow's account is one example. Mackey, in her contribution to this article, offers a different case when she recounts how her research practices and overall research program changed to better balance social and cognitive factors. In Mackey's case, the changes seem to have been prompted by theoretical pressures brought about by the social turn in the field of SLA (Block, 2003). Mackey's process is perhaps best described by Fleck's (1935/1979) notion of stylized remodeling (discussed by Young in this article). No researcher or research community can forever be insulated from surrounding intellectual developments. Eventually, he or she will be changed by varying degrees of familiarity with those developments. Theoretical pressures to bridge the gap such as those underlying Mackey's case are very different from the bottom-up, real-world pressures described by Bigelow.

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QUESTIONS OF (IN)COMMENSURABILITY, ALWAYS PENDING

I have created two metaphors, bridge building and bridge traveling, to characterize the positions taken by Ellis and Bigelow in this article. This rhetorical move allows me to suggest that it is possible for individual researchers to learn from felt cognitive-social tensions and to act on them positively across different epistemic lines along the shores of the gap. I have also suggested that there are not one or two but several ways to respond positively to the gap, as in the parallels and differences between Ellis and Lantolf in their contributions, on the one hand, and between Bigelow and Mackey in theirs, on the other. Yet, with these two rhetorical moves, I would not want to minimize important questions and tensions that run through the contributions in this article under the theme of (in) commensurability. Just how possible are mutual understanding and successful communication across different research communities that stand at different sides of the cognitive-social gap? In thinking about this question, it is important to remember that views regarding (in)commensurability are greatly diverse across relevant fields, such as the philosophy of science; history of ideas; or science, technology, and social studies. I choose to discuss here three positions toward the problem of (in)commensurability that have been distinguished by Holbrook (2013).

A well-known model of interdisciplinary communication is offered by Habermas (1981/1984), who argued that commensurability, or at least reciprocal understanding, is a tall order, though possible through the human capacity to engage in rational, action-oriented deliberation. The best-known position overall, however, is Kuhn (1962/1996), who was inclined toward incommensurability. Communication attempts that build on the shaky grounds of translation (always less satisfying than access to the original), argued Kuhn, will result in nonunderstanding or, worse, in overestimation of understanding and thus misunderstanding. The only solution would be for researchers from different communities to learn one another's discourses, though developing such interdiscursive competence may prove extremely difficult, if not impossible.

As an alternative to the Habermasian and Kuhnian renditions of the dilemma of (in)commensurability, Holbrook (2013) offers a third option, the reflective invention model, crafted from his reading of Bataille (1957/1993) and Lyotard (1983/1988). This model predicts that attempts at establishing understanding across epistemic boundaries will eventually lead to communicative breakdown precisely because each interlocutor, by virtue of being a member of a different thought collective, holds different assumptions and plays by different rules of the game of knowledge construction. (This is the perspective emphasized in the contributions to this article by DeKeyser, Hulstijn, Talmy, and Young.) The breakdown point marks the recognition of belonging to different epistemic discourses.

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And it is at the point of recognition of difference that the response becomes a critical choice:

> We can certainly answer by reasserting our (disciplinary) identities. In doing so, however, we forego the possibility of strong (interdisciplinary) communication. Such communication is possible only between individuals who risk their disciplinary identities and sacrifice them to the possibility of co-creating a new, shared genre of discourse. (Holbrook, 2013, p. 1878)

I find the Bataille-Lyotard reflective invention model poignant and useful. Through this lens, (in)commensurability can be seen as a local, emergent achievement whose centripetal impulse is located in critical moments afforded by a recognition of difference, which are then resolved either by reaffirming one's own epistemological identity and intellectual affiliations and thereby retreating from the possibility of communication (i.e., choosing incommensurability) or by transcending those differences in some way and keeping up the hard work of achieving at least some degree of success in communication (i.e., choosing commensurability).

Let us assume, then, that moments of recognition of difference and imminent communication breakdown are opportunities either to lock into incommensurability or to choose to work toward commensurability. What might be conditions that can sway individual researchers to choose to work toward commensurability? I would like to put forth two tentative answers. One draws from the rationalist traditions that predominate in the philosophy of science and that are likely to be familiar to many readers. The other looks for inspiration into poststructuralist traditions in postcolonial studies and feminist studies that may be less familiar to many readers. I include both as epistemological translations of each other, fully cognizant of their distinct potential to reach and persuade different readers.

RATIONALIST AND POSTSTRUCTURALIST INROADS INTO COMMENSURABILITY

From the rationalist tradition of philosophy of science, it is relevant to search for intellectual predispositions that may support commensurability. Understanding seems a necessary but not sufficient condition to achieve commensurability. Once the difference is recognized and understood, one can always opt out of communication efforts. Nor is knowledge an antidote for incommensurability because the deeper one's knowledge of someone else's thought collective becomes, the more acutely one recognizes the limits of superficial translation across thought styles. This may, in itself, lead to hopelessness and communicative disengagement at the edges of the gap. Respect for epistemological and methodological difference is also likely to be insufficient. For

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example, there is respect in Ellis's bridge-building proposal when he exhorts researchers to interact across disciplines and to abhor intellectual isolationism. But the question remains: Can researchers truly communicate when they are members of complementary yet independent research communities?

Tolerance of ambiguity, trust, and self-reflexivity may be among the needed ingredients, according to a panel of natural and social scientists who were charged by the European Science Foundation to evaluate felicitous conditions for interdisciplinary collaborations (ESF Standing Committee for the Social Sciences, 2013). Tolerance of ambiguity is required in communication with colleagues "whose research might, for example, look dangerously reductive or trivially broad from the point of view of their own epistemic community" (ESF Standing Committee for the Social Sciences, 2013, p. 21). In turn, such ambiguities are likely more tolerable, the panel concluded, when trust among individual researchers is strong. Being able and willing to trust is often associated with self-reflexivity, as "researchers frequently seek out trustworthy others, and also become willing to trust, only after critical reflection on the qualities of, and gaps within, their own disciplines and methods" (ESF Standing Committee for the Social Sciences, 2013, p. 21). This element of self-reflexivity is important in the interpretive turn called for by Talmy in his contribution to this article.

Support for commensurability can also be found in poststructuralist thinking. From this perspective, feelings of inner tension and threats to researcher identity can be uniquely illuminated by a willingness to confront questions of moral values. As social scientists like Kirschner (2013) put it, this requires going well beyond cold rationalism and drawing from nontraditional, visceral explorations of ideologies and ethical consequences of knowledge. The key condition supporting commensurability from the poststructuralist perspective is a willingness to transcend dichotomous epistemic boundaries. Two good candidates for doing this are the construct of an *interstitial* view offered by postcolonial literary theorist Bhabha (1994/2004) and the notion of *"world"-traveling* proposed by feminist philosopher Lugones (2003).

Bhabha (1994/2004) proposed that human beings have the capacity to achieve an interstitial view when, in the unfolding of an experience of marginality, they manage to displace the dichotomous assumptions implicit in a difference they recognize and suffer; by doing so they are able to reach beyond the other-imposed and self-imposed dichotomy and assert their right to difference in equality. Bhabha explained: "It is in the emergence of the interstices—the overlap and displacement of domains of difference—that the intersubjective and collective experiences of *nationness*, community interest, or cultural value are negotiated" (p. 2). Lugones (2003) proposed that human beings have the capacity to travel symbolically to someone else's world when we "understand *what it is to*

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be them and what it is to be ourselves in their eyes" (p. 97). This "world"-traveling in turn facilitates empathy and the recognition of the other as a valuable, legitimate interlocutor because "only when we have traveled to each other's 'worlds' are we fully subjects to each other" (Lugones, 2003, p. 97); by cultivating "world"-traveling, resistance to oppression and coalitions across different kinds of marginality can be achieved. In other words, *complex communication* is achieved (Lugones, 2006).

HOPEFUL COMMENSURABILITIES

Ultimately, the two answers to the challenge of cultivating commensurability I have offered are epistemological translations of each other. Readers will have to judge how superficial or successful the translation work is. As in all translations, my purpose in considering both has been one of reaching across linguistic and epistemological boundaries.

Rationalist answers to the conundrum of (in)commensurability, such as those offered by the ESF Standing Committee for the Social Sciences (2013), are helpful. Understanding, knowledge, respect, tolerance of ambiguity, trust, and self-reflexivity may all be important resources that individual researchers should cultivate if they are interested in closing the cognitive-social gap in the study of language learning and use. But these rationalist explanations are also somewhat limited: These virtues do not explain what ignites some researchers' desire to achieve mutual understanding and communication, particularly when the ideals of bridge building and bridge traveling may risk disciplinary identities, as Holbrook (2013) puts it and as Bigelow suggests on the basis of her own experience. How, when, and why, for example, would one even know that the trouble is worthwhile? More visceral analyses such as those offered by Bhabha (1994/2004) or Lugones (2003) offer helpful ways of deepening our appraisal of (in)commensurability as an emergent achievement of the Bataille-Lyotard kind (Holbrook, 2013). It is through liminal, performative processes such as the interstitial view and "world"-traveling that the recognition of difference during communication across epistemic communities may end up being seized as a momentary opportunity for working at commensurability. In critical rationalist epistemologies, these emotional-affective, relational-empathic, and moral-political accounts of commensurability fall outside the proper domain of knowledge and science. However, in a wide range of constructivist, structuralist, and poststructuralist epistemologies, knowledge is viewed as value laden and ideological. There is, thus, more willingness for researchers to feel they are accountable to axiological dimensions of knowledge and to acknowledge that they are influenced by them in their research praxis.

In the end, however, it should be clear that commensurability cannot be thought of as a constant virtue, only as an emergent achievement.

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Complete, permanent commensurability is not possible, nor is it desirable. For one, ways of thinking and knowing are not monolithic but encompass considerable internal heterogeneity, both across sides of any perceived gap and within the same thought collective (Bronson & Watson-Gegeo, 2008; Philips, 2004). Moreover, the preservation and coexistence of a degree of incommensurability within the field, or the cultivation of epistemological pluralism, is a good thing (Ortega, 2011). It may be wiser, therefore, to think of commensurabilities, in plural, as strategically chosen acts whose value is contingent on local purposes and dynamic conditions, acts that are always unfinished and ongoing, in a precarious but hopeful temporary balance.

CONCLUSION

Bridge building and bridge traveling in research in L2 learning and teaching are possible and worthwhile endeavors. Many researchers may feel conflicted by the perception of a cognitive-social gap. But many have also built bridges that allow multiple formats for traveling back and forth, for those who so desire, and bridge travelers willing to venture on them are likely to be on the rise. I hope that within reach in the future of our disciplines is also an even more radical reinterpretation of the felt cognitive-social gap. The bridges are not one or two but many: They spread along a variety of disciplinary topographies and, thus, are differently located alongside the gap, and they are of many different shapes and sizes. The traffic back and forth over these bridges is unpredictable, bidirectional, and ever flowing. Whatever cognitive-social tensions we feel, it will be good for researchers to learn to imagine the gap not as an imminent disciplinary demise but as a compelling invitation to build and travel many bridges. The cognitive-social gap can be an opportunity to open up new paths to knowledge creation.

EDITORS' CLOSING THOUGHTS

Looking back at the contributions to the original 2013 AAAL colloquium, the lively and positive audience interaction they inspired, and the resulting formulations gathered in this article, we believe we can legitimately draw the following two conclusions. First, contributors agree that there is no single, monolithic social-cognitive gap in L2 learning and teaching research, or at least there need not or ought not to be a gap. The contributions gathered in this article succeed in disentangling various independent contrasts in ontology, epistemology, and methodology, distinguishing a range of views rather than a single dichotomy. Furthermore, whereas it is true that, as one member of the colloquium audience remarked, when

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a gap is bridged there is still a gap, we note that genuine attempts to build bridges constitute attempts to understand views on the other side, which cannot but lead one to reflect on one's own stance. We trust that the contributions to the colloquium and to this article have shown many areas for dialogue and collaboration.

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NOTES

1. Hulstijn, Young, and Ortega recognize by name the contributions of the following colleagues, whose identity is known to the editors from audio recordings of the colloquium and who offered important thoughts that we have incorporated into our discussion: Dwight Atkinson, Andrew Cohen, Rod Ellis, Agnes He, Brian MacWhinney, Paul Matsuda, John Norris, Bonny Norton, Elaine Tarone, Steven Thorne, Albert Valdman, and Henry Widdowson. They are particularly grateful to Heidi Byrnes for introducing the ideas of Ludwik Fleck, whose work they subsequently read and have cited in this article.

2. Translated from the Spanish by Lantolf.

3. Talmy is grateful to Jan Hulstijn and Richard Young for inviting him to participate in this colloquium and to the many audience participants who helped transform the debate into discussion. He particularly thanks Keith Richards, Patsy Duff, Ryan Deschambault, and Meike Wernicke-Heinrichs for their comments on earlier drafts. All remaining errors are his own.

4. By "epistemic" Ortega means (in her own definition): reflective of any kind of theoretical knowledge gleaned from systematic evidence collected through the practice of science and scholarly inquiry. She finds this adjective is a useful alternative to *scientific* or *epistemological*, as it is more explicitly inclusive of any knowledge generated in the natural and physical sciences, in the social sciences, and in the humanities.

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