THE LANGUAGE SOCIALIZATION PARADIGM FOR SLA: WHAT'S IN IT FOR YOU?

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Since C.P. Snow, in his Rede lectures of 1959 (see Snow, 1993) first described the "two cultures" of science and the humanities, the gap between them has, if anything, widened. Where Snow saw mutual isolation and ignorance, however, recent years have witnessed a number of gross misrepresentations of, and even attacks on, the scientific enterprise, from a number of intellectual quarters—self-styled "feminists", self-styled "Marxists", and, of course, postmodernists of various stripes (see, e.g., Gross & Levitt, 1994; Sagan, 1996; Sokal & Bricmont, 1998, for documentation). The field of SLA has not been spared: a look at the "applied linguistics" literature all too easily turns up misinterpretations of natural sciences and misguided attempts to apply them to SLA (Edge, 1993; Larsen-Freeman, 1997; Schumann, 1983), doubts about the value of controlling for variables (Block, 1996), reduction of empirical claims to metaphors (Lantolf, 1996; Schumann, 1983), mockery of empirical claims in SLA as "physics envy" (Lantolf, 1996), and denials of the possibility of achieving objective knowledge (Lantolf, 1996). Although the standpoints are various, one common thread unites these papers: a fundamental misunderstanding of what science, and in particular cognitive science, is about (see, e.g., Gregg et al., 1997; Gregg, 2000). One sort of critique of SLA research conducted within the framework of standard cognitive science comes from some of those concerned with social and political aspects of second language use and teaching (e.g., Firth & Wagner, 1997; Pennycook, 1990). A recent, wide-ranging, and ambitious critique of this sort comes from Karen Watson-Gegeo.

Watson-Gegeo (2001) tells us that we are "at the beginning of a paradigm shift in the human and social sciences" that is "fundamentally transforming second language acquisition (SLA) and educational theory and research" (p. 1). Watson-Gegeo is not very forthcoming as to the nature of either the old paradigm or the new one; and indeed, one of the problems with her paper is the absence of anything that could be called evidence.

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Still, based on what she sees as this emerging new paradigm, she is explicit in arguing that we need a new paradigm for SLA, what she calls the "language socialization paradigm". I propose to look at the new cognitive science as Watson-Gegeo conceives it, and at her proposals for SLA research. It will be seen that her account of the former is generally vacuous or irrelevant where it is not simply incorrect, while the latter show little promise for a productive research program for SLA.

I. OUR "NEW UNDERSTANDINGS ABOUT MIND AND LANGUAGE"

First, what is this new, emerging paradigm? Watson-Gegeo lists six specific findings, six things that "we now know about cognitive processes and the human brain" (p. 4), as well as three additional findings specifically about language. It is worth examining these one by one.

A. *"Current understandings about the brain and thinking"*

1. *"[N]euroscience research ... has demonstrated [sic] that the body-mind dualism of Western philosophical and mainstream scientific thought ... is fundamentally mistaken"* (p. 4).

In fact, neuroscience research has "demonstrated" no such thing, nor, on the other hand, has mainstream cognitive science posited such a dualism. The existence or non-existence of an immaterial mind, for one thing, is simply not the sort of thing one can demonstrate. More importantly, virtually *no one* in cognitive science is a dualist; there is near unanimity, among cognitive researchers who are otherwise at each other's throats, that dualism is a non-starter. Chomsky, for one, has long insisted that the so-called "mind-body problem" ceased to exist when Newton deprived us of any useful concept of "body" (e.g., Chomsky, 1995). What *does* divide cognitive scientists is the very different question of whether there are mental phenomena at all. A fairly small minority of scholars, including eliminativists such as the Churchlands (e.g., Churchland, 1986, which Watson-Gegeo cites in support of her claim) and the behaviorists, wish to deny the existence of such phenomena. The majority of cognitive scientists, however, while sharing the anti-dualist materialism (or physicalism) of the eliminativists and the behaviorists, nonetheless believe in the real, non-metaphorical existence of mental states

such as beliefs and desires, and of mental events such as inference, decision, and computation.

The vast majority of cognitive scientists, that is, endorse *functionalism*, which claims, in essence, that

1. the mind is certain functions of a complex system, the brain;
2. each and every particular mental state or event is some state or event of such a system [*contra* mind-body dualism—krg];
3. we have to use the language and explanatory style of psychological explanation to capture and explain mental states and events. (from Brook & Stainton, 2000, p. 95; cf. Rey, 1997)

Cognitive scientists, as functionalists, abstract away from the flesh-and-blood biology of the brain in order to better examine specific mental functions the brain carries out, such as inferencing, categorizing, or language processing. There are a number of compelling justifications for this kind of abstraction, including (a) that we know next to nothing about how the brain actually works to perform such functions, and (b) that we can, and in the best cases even do, produce good explanations at the level of the mental that we do not, and perhaps even cannot, attain at the level of the neuronal (see, e.g., Gold & Stoljar, 1999). Thus the claim that "All cognitive processes are thus embodied" (p. 4), rather than being a new finding from new-paradigm cognitive science research, is simply a *given*, an assumption that literally goes without saying.

2. *"[M]ore than 95% of all thought is unconscious. ... Included in the cognitive unconscious is all implicit knowledge that we have learned through socialization beginning in the prenatal months [sic]"* (p. 4).

Watson-Gegeo doesn't tell us what cutting-edge research of the new paradigm produced the figure of 95%, nor does she give us any indication of how indeed one could quantify thought with such accuracy. (Nor does she explain just what sort of socialization could be going on in the womb, even between twins.) But then it's really of no consequence, as it has been non-controversial since Freud that much of our mental life is unconscious. More to the point with respect to SLA in particular, it is one of the fundamental claims of current linguistic theory, as developed under the old paradigm (see I.B. below), that essential elements of our linguistic competence are unconscious and

indeed inaccessible to consciousness, as is the process whereby we acquire that competence. In so far as it's not incoherent to attach a percentage value to the "amount of thought" that is or is not conscious, then, few if any cognitive scientists, including linguists and language acquisition researchers, would find the validity of their research programs in any way impugned by Watson-Gegeo's second finding.

3. & 4. ***"[M]ind is a better term than 'cognition' because the latter tends to focus on only parts of the mind, ..."*** **(p. 4).** ***"[O]ur earlier conception of cognition has been further expanded to incorporate many other components of a human mental life, including symbolic capacity, self, will, belief, and desire"*** **(p. 5).**

Well, to decide that one word is "better" than another is hardly a finding, even if the decision is a correct one. Whether one should use "mind" instead of "cognition" depends on what one wants to talk about: all in all, "mind" is a better term to use when talking about the mind, and "cognition" is a better term when talking about cognition. The reason we have two words, of course, is that they refer to different things. Cognition has traditionally been taken to include mental representations and their manipulation, what are often referred to as intentional states (hence, *pace* Watson-Gegeo, "our earlier conception of cognition" has *not* expanded to include symbolic capacity, belief, and desire, as these have always been at the very core of cognitive science). Other mental phenomena, such as sensations or emotions, are not intentional, whereas they do involve privately felt qualities ("qualia", in the jargon), where intentional states do not. There are compelling a priori and a posteriori grounds for categorizing mental states into intentional and non-intentional, or qualitative and non-qualitative: a priori, in that it seems intuitively plausible that, say, logical reasoning, goal-oriented planning, or sentence processing might follow different laws than, say, experiencing the taste of chocolate or an outburst of rage; a posteriori, in that in actual fact we have been able to make progress in cognitive science precisely by categorizing mental states in this way. Pre-eminently, of course, we have rather rich and sophisticated explanatory theories of linguistic competence, theories that never could have got off the ground if they had not been based on the narrower "old-paradigm" concept of cognition. (Whereas qualia, for instance, remain as much a mystery as ever.)

Watson-Gegeo tells us that "without emotional capacity, people cannot make rational judgments, including moral decisions" (p. 5), and appeals to Fischer et al. (1998, pp. 22-23), who claim that emotions are "not opposed to cognition, as is assumed in Western culture; to the contrary, [emotion] links closely with cognition to shape action, thought, and long-term development." This is hardly persuasive; after all, people cannot make rational judgments without a heart, either, and yet one seldom hears calls for a reconceptualization of cognition to include the circulation of blood. The fact that humans are humans, not zombies, is hardly a challenge to the old cognitive science paradigm. And indeed, it was none other than David Hume, a founder of the so-called representational theory of mind that is the dominant outlook in modern cognitive science, who notoriously said that "reason is and ought to be the slave of the passions." Nor, on the other hand, does the interrelation of cognition and emotion in some cases entail their interrelation in others. Loss of affect may lead to an inability to make moral decisions; it does not follow that it leads to an inability to parse a sentence.

5. ***"[N]ot only is language metaphorical, but because of the kind of neural networks we build in our brains, thought itself is metaphorical and made possible through categorization that is typically conceptualized as prototypes"*** **(p. 5).**

This takes a bit of unpacking, and even then it's not easy to derive anything coherent from this set of claims. First of all, Watson-Gegeo doesn't bother to make clear what she means by saying that language is metaphorical. If it's the simple commonplace that we use metaphor all the time in using language, once again we have a truism that could not differentiate between possible paradigms in cognitive science. On the other hand, if she means that *all* of language is metaphor, so that there are no literal, non-metaphorical sentences in a language, then it would behoove her to provide a little evidence, for what is prima facie a gross falsehood.

But we'll get to the new cognitive science's discoveries about language below. Putting language aside for the moment, how does one reconcile the claim that thought is metaphorical with the claim that thought is 95% unconscious? What evidence is there—what evidence could there possibly be in principle—that our unconscious thoughts are metaphorical? Indeed, what could it possibly mean? Let us take an everyday example of thinking. Let us imagine that I want my colleague, Michael, to teach my Thursday

afternoon class because I have an important committee meeting that will take all day Thursday. My thoughts are likely enough, on most accounts, to include: (a) the thought that I want Michael to teach, etc.; (b) the thought that if he has classes of his own on Thursday he won't be able to accede to my request; (c) the thought that in order to get him to take my class I'll need to ask him to do so; (d) the thought that in order to ask him I'll need to know his phone numbers and when he's scheduled to be in his office or at home; and so on. Try as I might, I cannot find anything metaphorical in any of these thoughts.

Nor, try as I might, can I find any *sequitur* connecting the claim that we build neural networks in our brains (or, to be a bit less metaphorical, that neural networks form in our brains) with the claim that our thoughts are metaphorical, let alone that they are metaphorical of necessity. Nor, for that matter, can I see any way to connect the premise that we build neural networks to the conclusion that our thoughts are *not* metaphorical. It is definitely the case that neural networks form in our brains; and it is definitely the case, for me anyway, that we have thoughts. And that's about it so far as the state of cognitive science goes; we do not have a clue as to what the connection is between our neural networks and our having thoughts. I want to stress that; not a clue. But if we are so totally clueless as to how our thoughts are instantiated in our brains, then, even in the unlikely case that our thoughts *could be*, let alone are, metaphorical, there is not the slightest reason in the world to say that they are so because of the specific way our neural networks are formed.

There is still more confusion to be sorted out, though. "Thought itself" is "made possible through categorization that is typically conceptualized as prototypes" (p. 5). Watson-Gegeo doesn't make clear who is doing the conceptualizating of categorization, but I'm going to guess that it is not us who categorize but rather the new-paradigm scholars such as those she cites, especially those who base themselves on Rosch's important research, who view categorization in terms of prototypes. We still have a category error to dispose of: on anyone's account, concepts are individuals, and categories are sets of individuals, whereas categorization is a process. Thus, it cannot be that categorization is conceptualized as prototypes; so Watson-Gegeo presumably means either that categorization is the process of creating prototypes or else that it is the process of assigning an individual to more or less prototypical membership in a category. But it

can't be that categories themselves are conceptualized as prototypes; this would be another category error. On the standard prototype account of categories, category *membership* consists in possession of various properties; prototypes are members of a category that have more, or the more central among, such properties. This may seem pedantic, but it's important to be as clear as possible what one is claiming in this area, even at the risk of pedantry.

Well, then, does the new paradigm treat concepts as prototypes? And is it right to do so? The answer to the first question is probably a qualified yes, at least in so far as one is justified in talking about paradigms in the first place. The classical idea that categories are defined by necessary and sufficient conditions on membership does seem to have been pretty generally abandoned, and talk of concepts does seem to center pretty generally around the idea of prototypes, so that category membership is no longer seen as a question of entailment (where, say, X is a bachelor if and only if X is male and unmarried) but rather as a question of probability (so that a 25-year-old unmarried bank teller is a better example of a bachelor than a 15-year-old high school student or a 25-year-old priest). And this idea has been applied to linguistics and language acquisition (cf., e.g., Ellis, 2002; Shirai & Andersen, 1995; Taylor, 1989).

But the fact that most cognitive scientists have given up on the idea of necessary and sufficient conditions for category membership does not mean that categories are "typically conceptualized as prototypes". Rosch herself, the pioneer of prototype research, stresses that "Prototypes do not constitute a theory of representation of categories" (Rosch, 1978, p. 40). Lakoff, another of Watson-Gegeo's sources, dismisses prototype effects as "superficial. They show nothing *direct* about the nature of categorization" (Lakoff, 1987, p. 63; cf. Eubank & Gregg, 2002, for similar criticism of Ellis). Fodor (e.g., 1998a) offers compelling reasons for thinking that concepts or categories could not possibly be prototypes, centering on the self-evident compositionality of concepts and the equally self-evident non-compositionality of prototypes. Margolis (1994), like Fodor recognizing that the field has shifted from the classical theory of concepts to the prototype theory, argues that this shift is "unwarranted"; "prototype theory offers no advantage over the classical theory" (p. 88), since it inherits all the defects of classical theory. Thus, although there is widespread agreement as to the ubiquity of prototype effects, there is anything but consensus as to

whether or not concepts are themselves prototypes. There is even less of a consensus as to how prototypes are constructed; Watson-Gegeo tells us that many of them, "probably the majority, are socioculturally constructed", but offers no evidence in support of that claim.

6. *"Research demonstrates [sic] that 'both the content and process of thinking ... are distributed as much among individuals as they are packed within them' (Cole & Engestrom, 1993:1)." "...[C]ognition is socially constructed through collaboration..."* (p. 6).

Again, Watson-Gegeo is rather casual in her use of the word "demonstrate"; not only has there been no demonstration that thought is distributed among individuals, it has not been made clear what this could even mean. Certainly the kind of research Cole and Engestrom refer to tells us nothing about how knowledge is instantiated in an individual or a group. (Indeed, the editor of the very volume in which Cole & Engestrom's essay appears expresses strong reservations about the idea (Salomon, 1993b; see also Nickerson, 1993).) Nor has anyone yet given a coherent account of what it could mean for cognition to be socially constructed (cf., e.g., Hacking, 1999). It is the most banal of commonplaces that we function, generally, in social groups, and that we learn while, and sometimes from, interacting with others. It hardly follows from that truism that when I ponder, say, what to order for dinner, my decision is actually made jointly by my waiter and me, any more than that we jointly find my fish overdone; nor does it follow that my knowledge of French is not in my head simply because similar knowledge is also in other people's heads, or because I learned French in a classroom.

Watson-Gegeo appeals to "situated cognition" and "situated learning", which she seems to think have replaced, or are replacing, "older cognitivist theories [which] viewed knowledge as a collection of real entities, located in heads" (p. 16). But her conception of situated cognition hardly transcends the sort of banality of such claims as that "every cognitive act must be viewed as a specific response to a specific set of circumstances" (Resnick, 1991, p. 4, quoted by Watson-Gegeo, p. 16), claims that fail utterly to distinguish between various possible accounts of cognition. And her conception of situated learning is no more precise: "everything that happens in the human world is in a context with specifiable characteristics", a position that is hard to refute, certainly, and

equally hard to apply productively to actual cases. Nor can I see how one is to combine the various findings of the new cognitive science, so that my thoughts are at once situated (not, contra the old cognitive science, in my head), unconscious, and metaphorical. At the present moment, for instance, I am entertaining the thought that it is time to check the dryer to see if my shirts are dry; what does the new situated cognition tell me about that thought, or about how it has just now led me to go to the dryer and open it up?

I might point out that within standard cognitive science there actually is a school of thought, or a movement, sometimes called "situated cognition", and that it has a good deal more empirical content than the sorts of generalities Watson-Gegeo cites from the social sciences (see, e.g., Thelen & Smith, 1994). Research in this framework has indeed come up with concrete results (unlike the "situated cognition" of Watson-Gegeo's paper), although not in the realm of language. However, Watson-Gegeo can only with difficulty appeal to this real, contentful form of situated cognition, since workers within this framework deny the very existence of representations, whereas Watson-Gegeo (see above) is committed to a prototype version of representationalism. Whether in fact real situated cognitionists are correct is another question, which I'm certainly not prepared to speculate on here (see, e.g., Clark, 1997, for sympathetic but critical discussion); the point is that, unlike Watson-Gegeo, they have at least made precise claims which can be tested.

B. *"What [we have] discovered about language from cognitive science research"*

1. The first discovery is actually several discoveries:

(a) *"[R]esearch has discovered no structure in the brain that corresponds to a Language Acquisition Device as argued by Chomsky and others"* (p. 6).

(b) *"Language is not a human genetic innovation, because its central aspects arise via evolutionary processes from neural systems that are present in so-called 'lower animals'"* (pp. 6-7).

(c) *"Linguistic concepts, like all other cognitive processes [sic], arise from the embodied nature of human existence, and through experience"* (p. 7).

(d) *"Language develops through the same general processes as other cognitive skills ..."* (p. 7).

(e) *"[G]rammar is a matter of highly structured neural connections"* (p. 7).

(a) It is definitely the case that no one has discovered a brain structure corresponding to a Language Acquisition Device, but then again, no one has been looking for one. (Of course, on the other hand, it's wholly uncontroversial that the brain *is* a language acquisition device.) Certainly Chomsky has never argued for such a physical structure, nor has his research (or generative linguistic research in general) been conducted with a search for such a structure in mind. Hence pointing to the non-discovery of a physically distinct LAD is simply neither here nor there.

(b) I don't know what to make of the claim—hardly a discovery, in any case—that language is not a human genetic innovation. I assume that it is "innovation", not "human" or "genetic", that is within the scope of the denial; after all, language is a human characteristic, and I can't imagine that Watson-Gegeo wants to deny the genetic basis of the language faculty. So presumably, the claim is that other primates, at least, have the same neural systems that humans do, but that for some reason those systems have evolved in humans to the point where we have language. This sounds like a human genetic innovation to me, I must confess. But in any case, since we do not yet know what the neural foundations are for language in humans—certainly Watson-Gegeo doesn't give us any hints—there is simply no force to her "discovery".

(c) It seems odd to treat a centuries-old claim about the etiology of concepts as an empirical discovery of cutting-edge research. (It also seems odd to refer to concepts as processes, but that's by the way.) Actually, as phrased by Watson-Gegeo, it's not clear

whether (c) is even making a claim worth debating: aside from the odd Platonist, no one in cognitive science or linguistics would deny that our linguistic concepts are embodied, and no one would deny that the acquisition and use of a language require experience. Presumably, however, what Watson-Gegeo is arguing is something of more substance, viz. that our linguistic and other concepts are *learned*. This is, of course, a claim with many defenders; it also has many opponents, and, as a matter of brute fact, the issue is simply not settled. There is a large and important literature in philosophy of mind and in cognitive science on this subject, and, more to the point, there are powerful arguments for the innateness of many concepts (Fodor, 1975, 1998a). Perhaps even more to the point here, and *pace* Watson-Gegeo, these arguments have nowhere been refuted (Cowie, 1998).

(d) Like (c), this is an old and perfectly respectable claim; it simply isn't a discovery. One reason that it isn't a discovery is that we still do not know what the putative general cognitive processes are by which skills develop. Further, it is perfectly possible that language skills develop, however they develop, in the same way that other skills develop, however they develop, while language *knowledge* is fundamentally different from other kinds of knowledge. It's perfectly possible, that is, that our ability to use active and passive forms of sentences improves according to the power law of practice (Ellis, 2002), while our possession of the concept SUBJECT is innate.

(e) As with many other of Watson-Gegeo's discoveries, this one is either a truism accepted by all parties, or else is a highly controversial and far from settled claim. No one denies that grammar, like any other aspect of competence in any mental area, is instantiated in the brain, and that the brain comprises highly structured neural connections. (Well, no one besides Watson-Gegeo's situated cognitionists, who would seem to locate grammar in the community.) On the other hand, very few cognitive scientists would accept the claim that grammar just *is* neural connections; the whole point of the functionalist enterprise, after all, is precisely to abstract away from specific neurons to cognitive capacities.

2. ***Although "innateness is usually equated with language universals," "what we take to be universal typically involves universals of common human experience starting after birth" (p. 7).***

Actually, *pace* Watson-Gegeo, innateness in language is not usually, or primarily, equated with language universals; universality is neither a necessary nor a sufficient condition on innateness. No one argues that the prostate gland is not innate, even though it's hardly universal; nor, on the other hand, does anyone argue that having a lexical item denoting water is an innate characteristic of languages because it's a universal characteristic of languages. Once again, it is the merest of commonplaces that some linguistic phenomena—some, mind you—reflect cultural or historic phenomena; honorifics, say, or gender-specific lexical items or speech styles in some cultures. This says exactly nothing about—in particular, it advances us not one inch toward refuting—the claim that linguistic competence is in a fundamental way innate. Large, unsupported claims that "gender, ethnicity, social class, and sociohistorical, sociopolitical processes" affect "perceptions, assumptions, language(s), and other understandings of the world," or that seeming universals of language "typically involve universals of common human experience starting after birth" (p. 7), are simply so much handwaving. They tell us nothing, for instance, about why all languages are structure-dependent, or why they show the same set of subject-object asymmetries, or why structural variation across natural languages is so limited.

The real argument for innateness in language, in fact, is not the existence of universals, but rather the argument from the poverty of the stimulus (Chomsky, 1988; Laurence & Margolis, 2001). The claim is that (in the case of language) "The data that would be needed for choosing among [various possible] sets of principles [necessary for successful acquisition] are in many cases not the sort of data that are available to an empiricist learner," where "empiricist learner" means a learner lacking innate, language-specific knowledge (Laurence & Margolis, 2001, p. 221). Since children are in fact successful language learners, it follows that there must be innate, language-specific knowledge. The argument has, needless to say, been attacked, both on conceptual grounds (e.g., Cowie, 1999; Goodman, 1969; Putnam, 1967) and on empirical grounds (e.g., Elman et al., 1996). The attacks have so far been signal failures (see, e.g., Fodor, 1998b, 2001; Laurence & Margolis, 2001; Marcus, 2001). In particular, attempts to

create connectionist models to serve as existence proofs for the possibility of empiricist learning—perfectly legitimate and praiseworthy attempts—have failed to eliminate the need for innate ideas (Marcus, 2001). The issue is hardly settled, of course; but the claim, which Watson-Gegeo seems to wish to deny, that language acquisition depends on domain-specific innate knowledge is still very much alive and kicking. Indeed, it is the only plausible explanation we currently have for which there is ample evidence. The burden is on the anti-"innatists" to show how the environment is rich enough to provide universally successful language instruction to first language learners.

3. ***"[L]anguage structure, language use, and language acquisition are inseparable because experience shapes all our neuronal networks. These processes are therefore [sic] shaped by sociohistorical, sociocultural, and sociopolitical processes"*** **(pp. 7-8).**

There are a number of problems with this "discovery". To start with, while it's one thing to say that language structure, use, and acquisition are interconnected or interrelated, it's quite another to say they're inseparable. The former is truistic; the latter is false, at least on some plausible interpretations of "inseparable". Certainly it's possible to separate the three for purposes of analysis and research; linguistic theory, after all, has developed by systematically ignoring questions of use and acquisition. And studies of use—research on politeness expressions, for instance, or on turn-taking in conversation—normally don't trouble themselves with questions of syntactic or morphological structure. Indeed, it's not even clear what a linguistic theory would be that tried to account for the facts of structure, use, and acquisition within one single explanatory framework. Nor is it clear what connection is intended between the putative inseparability of language structure, use, and acquisition on the one hand, and the effect of experience on our neural networks on the other. If it's a fact that experience shapes all our networks, and if it's this fact that causes the inseparability of structure, use, etc., then we should be equally justified in claiming, say, "Language structure, language use, and digestion are inseparable because experience shapes all our neuronal networks."

But is it a fact that experience shapes all our networks? The claim could be false in at least two different ways: it may be the case that some neural networks are not affected by experience, and it may also be the case that not every experience affects every network. Both counterclaims seem eminently plausible, at least (as always, Watson-Gegeo

provides no evidence one way or the other). I know of no neuroscientist who claims that the entire neocortex, let alone the entire brain, is plastic, hence experience-dependent. Nor do I know of any neuroscientist who claims that *every* neural connection is altered by *any* given environmental stimulus. But if there are neural networks that are not altered by experience and if a given environmental stimulus affects only certain networks, then there is no reason whatever to accept Watson-Gegeo's claim that language structure, use, and acquisition are all "therefore shaped by sociohistorical, sociocultural, and sociopolitical processes". All in all, I think that's a good thing, because all in all, it seems pretty hard to conceive of what sort of effect processes like decolonization, the sexual revolution, or globalization could have on the Binding Principles, or subject-object asymmetries, or *wanna*-contraction.

II. THE TWO "PARADIGMS"

In short, the new cognitive science that Watson-Gegeo thinks is replacing the old one has nothing to tell us that isn't either vacuous or wrong or both. Not to put too fine a point on it, this new cognitive science does not, in fact, exist. This is not, however, to say that Watson-Gegeo's claims are empty of content; although she does not in fact provide any empirical evidence whatever—and I mean that literally; none—she definitely has complaints about cognitive science in general and SLA research in particular as they are currently carried out, and the burden of her paper is to outline how SLA research should be carried out.

Standard cognitive science has a number of characteristics which Watson-Gegeo seems to find problematic, at best, and which seem to lie at the heart of her quarrel with the "old paradigm". Four of these are worth noting, as Watson-Gegeo's "new paradigm"—in effect, her wished-for approach to the study of the mind—is in direct contrast with standard cognitive science on these four parameters.

1. Cognitive science is, for want of a better term, ***modularist***. The goal of cognitive science is, to use the term common in the philosophy of mind, to "naturalize the mind": to treat the mind, or such parts of it as are amenable to such treatment, as the object of empirical scientific inquiry. One consequence of such a stance, though, is that the researcher, like any other scientist, seeks to "carve nature at its joints," to categorize the

domain, to the extent feasible, in terms of natural categories. Further, those categories are defined, not in commonsense everyday terms, but by the theory as it is developed. All this is to say that "mind" for a cognitive scientist may turn out to exclude, say, the passions, just as "language" for the linguist may turn out to exclude implicature. And conversely, cognitive science may turn out to find the distinction between conscious and unconscious of no interest when dealing with beliefs, just as linguistic theory may find the distinction between signed and oral languages, or written and spoken languages, of little interest.

2. Cognitive science is ***individualist***; it locates cognition within the individual mind. In the same way, linguistic theory locates linguistic competence within the individual mind (in Chomsky's terms, linguistic theory is concerned with I-language not E-language). This certainly does not mean that one cannot abstract away from the individual to study larger units like families or cultures, just as one can abstract away from individual linguistic competence to study E-languages (English, Swahili, whatever) or the language practices of groups, as in sociolinguistics. The cognitive scientist, however, takes as a working assumption that the appropriate level of abstraction for analysis of cognitive capacities is the individual mind. This assumption follows equally from the functionalist perspective taken by most cognitive scientists and from the minority eliminativist position; in either case, cognitive capacities are embodied in an individual brain. With a different species, for example the social insects, one might prefer a different perspective (e.g., Queller & Strassmann, 2002); but there seem to be good reasons for thinking that the individual human's mind/brain is the appropriate place to look for causal processes to explain individual cognitive capacities.

3. Cognitive science is ***universalist***. This follows from the naturalistic stance: in looking for natural categories, one tries to set aside individual differences that might accidentally differentiate members of the same category. Thus biologists ignore commonsense markers of "race", such as skin color or hair type, and work on the (empirically correct) assumption that humans constitute a single species. This position does not, of course, preclude subcategories of possible interest; there may, for example, be cognitive differences between the two sexes (Halpern, 2000; Kimura, 1999). But then sex defines a pair of natural categories; there would be a lot less likelihood of discovering scientifically interesting cognitive differences between, say, Greeks and Norwegians, or

between socialists and conservatives. And of course, modern linguistic theories abstract away from surface differences between languages to concentrate on the universal principles underlying all human language.

4. Cognitive science is ***nativist***. This, too, follows from the commitment to naturalizing the mind: if our cognitive capacities are instantiated in the brain, then they are ultimately part of our biological makeup, and that makeup is largely a function of our genetic characteristics. Of course, these characteristics are expressed as a result of interaction with the environment, and when it comes down to cases the role of the environment may well be decisive. This in no way weakens the essentially nativist commitment of cognitive science, although it certainly does make the job of teasing apart genetic and environmental contributions to cognition a good deal harder.

Against this standard kind of cognitive science—essentially, natural science as applied to the mind—Watson-Gegeo opposes her putative new paradigm:

1. Where normal cognitive science is modularist, Watson-Gegeo calls for ***holism***. As we have seen, she repeatedly insists on the inseparability of emotion from rationality, of syntax from semantics, of language structure, use, and acquisition. "A situated learning perspective rejects the notion that there can ever be decontextualized knowledge..." (p. 17); "The learning of language, cultural meanings, and social behavior is experienced by the learner as a single, continuous (though not linear) process" (p. 20). As I've already noted, such claims of inseparability amount to little more than vacuous talk about how everything's connected. But more importantly, insisting on a holistic approach to cognition leads inescapably to "Fodor's First Law of the Nonexistence of Cognitive Science": "the more global ... a cognitive process is, the less anybody understands it" (Fodor, 1983, p. 107). Fodor, of course, is betting that there are non-global, modular cognitive processes as well as the global ones, because if there aren't any, then cognitive science may well be impossible. Of course, one can only find modular processes if one goes looking for them, and one looks for them by refusing to be cowed by the interconnectedness of things.

Watson-Gegeo, of course, disapproves of SLA research's "experimental modes of inquiry that cannot incorporate cultural and sociopolitical context into its models" (pp. 2-3), but it is precisely that inability, or rather refusal, to take everything into account that enables SLA research, or any scientific research, to get any results at all. As Glymour

puts it, "while faithfulness to the complexity of natural settings produces endless research (because natural settings are endlessly varied) and sometimes produces useful technology or policy, it has never, in any science, produced an understanding of fundamental mechanisms. In natural settings, the fundamental mechanisms, whether physical or psychological, are endlessly confounded with one another and cannot be sufficiently separated to reveal their several structures, effects, and interactions" (Glymour, 2000, p. 193).

2. Where normal cognitive science is individualistic, Watson-Gegeo's hoped-for paradigm is ***collectivistic***. Cognition is "socially constructed" (p. 6). Indeed, "epistemological agents are communities rather than individuals" (p. 11, quoting Gegeo & Watson-Gegeo, 2001, p. 58). It takes a village, as it were, to know a language. The problem, of course, is that it is hard to give this sort of concept any empirical content. It is hard to go from the fact, for instance, that children are raised in families within cultures and acquire their language in that context, to a causal explanation connecting this fact with the fact that four-year-olds obey the constraints on agentive compounding in English or object-drop in Japanese.

3. Where normal cognitive science is universalist, Watson-Gegeo believes that the new paradigm "has already seriously eroded the universalist assumptions that have until now determined mainstream theory and method, and that are anchored in Anglo-Euro-American cultural ontology and epistemology" (p. 2). We might call this approach ***particularist***, in that it both assumes and looks for crosscultural differences in cognitive capacities, states, and processes (except, it would seem, in England, Europe, and America). What these cognitive differences are, Watson-Gegeo doesn't specify, nor do her sources. It would seem that given the self-evident differences across cultures in behavior and belief systems, and given the collectivist idea of the community as the locus of epistemological states, the culture-relativity of cognition is simply supposed to follow; it doesn't.

4. Where normal cognitive science is nativist, Watson-Gegeo is a ***constructivist***. Here, the central claim is that "knowledge is constructed and validated by a cultural group" (p. 13); "all knowledge is subjective, positioned (i.e., from a standpoint, not objective in a final sense), historically variable, and specific" (p. 12); "cognition originates in social interaction and is shaped by cultural and sociopolitical processes" (p.

3). The new paradigm, in fact, is actually a relativist variation of traditional empiricism: the mind is essentially empty at birth, and the environment—in this case, pre-eminently the social environment—fills it. The problems with constructivism should be too well-known by now for me to have to spend much time on them (Fodor, 1975; Giere, 1999; Hacking, 1999; Laudan, 1996). Suffice it to say that just as nativism depends on the poverty of the stimulus argument for its justification, constructivism has to show how the environment is rich enough to produce cognitive effects about the existence of which there is little or no disagreement. Watson-Gegeo is hardly alone in failing to meet this requirement (Gregg, 2001).

III. THE "LANGUAGE SOCIALIZATION PARADIGM"

What we have, in short, is not a new paradigm, but an old agenda. Where normal cognitive science sees itself as part of the natural sciences, and takes the mind to be an object of empirical research, Watson-Gegeo wishes, not to naturalize the mind, but to socialize it. Thus the "Language Socialization Paradigm" that she calls for is, in effect, an attempt to locate the field of SLA within the study of social organization and behavior. The five premises of what Watson-Gegeo calls "language socialization (LS) theory" are as follows:

1. *"[L]inguistic and cultural knowledge are* constructed *through each other"* (p. 20; emphasis in original). *"Learners construct 'a set of (linguistic and behavioral) practices that enable' them to communicate with and live among others in a given cultural setting' (Schieffelin, 1990: 15)"* (p. 21).

2. *"[A]ll activities in which learners regularly interact with others ... are not only* by definition *socially organized and embedded in cultural meaning systems, but are inherently political"* (p. 21; emphasis added).

3. *"'[C]ontext refers to the whole set of relationships in which a phenomenon is situated' (Watson-Gegeo, 1992: 51), incorporating macro-levels of institutional, social, political and cultural aspects, and micro-levels involving the immediate context of situation"* (p. 22).

4. *"[C]hildren and adults learn culture largely through participating in linguistically marked events, the structure, integrity, and characteristics of which they come to understand through primarily verbal cues to such meanings"* (pp. 22-23).

5. *"[C]ognition is built from experience and is situated in sociohistorical, sociopolitical contexts..."* (p. 23). *"Capacities and skills are therefore built by active participation in a variety of different roles associated with a given activity over a period of time, from peripheral to full participant"* (p. 24).

These premises are vague enough that one could easily quibble with any of them, or even, if one wished to be uncharitable, dismiss them all as either manifestly incorrect or else unhelpfully truistic. What sort of morphosyntactic or phonological knowledge is constructed through cultural knowledge, for instance? What is inherently political about going to the grocery store? Who would deny that learners live and act in different kinds of cultures and political systems? Who would deny that, as a rule, learners proceed from less to more adept members of a language community? But to raise questions like these, reasonable as they may be, is to miss a more important problem about the Language Socialization Paradigm for SLA. The problem is twofold: on the one hand, the foundational premises of LS theory address only the sociopolitical, as opposed to the cognitive, aspects of L2 learning; while on the other hand, Watson-Gegeo clearly intends her paradigm to replace, not complement, the methods and principles of empirical cognitive science. It's hard, frankly, to see how the "language socialization paradigm for SLA" would be of any use to researchers interested in the very real problems of language and socialization; its premises are so vague that even were they correct they would hardly delineate a research program. But in any case, it has nothing whatever to say about SLA.

Watson-Gegeo tells us that her proposed new paradigm "would transform SLA research" (p. 26), and indeed it would; it would put an end to it. Here, for instance, are some research results taken at random from recent issues of *Second Language Research* and *Studies in Second Language Acquisition*, two SLA journals from well within the old paradigm: Inagaki finds L1 influence on the acquisition of L2 argument structure when the L2 is in a subset/superset relation to the L1; Hirakawa reports that L1 English-speaking learners of L2 Japanese make the target language's distinction between unaccusatives and unergatives correctly, in the absence of explicit instruction; Flege and Liu report that listening proficiency varies by length of residence among students, but not

among non-students; Herschensohn claims that most cases of nonfinite verbs in L2 French are examples of defective inflection rather than root infinitives. Putting aside the question of whether these findings are accurate or whether the research was well-conducted, how would one conduct research under the New Dispensation that could lead to results such as these? Why, indeed, would one even come up with the relevant research questions?

It's perfectly possible, of course, that results such as the above examples are not of interest to many researchers; different strokes, after all, for different folks. But at least these results *relate to the acquisition of an L2*; the claims made are claims about what happens in a learner's mind. Old paradigm or new, cutting edge or old hat, this is cognitive science. It tells us nothing, granted, about the behavior of communities or of learners in communities, or about power relations between them; but it does tell us something, perhaps even something correct, about the mind. What is wrong with that? And conversely, if one is interested in, say, how colonialist attitudes among educational authorities affect the way in which L2 education is conducted in third-world societies, what is there in this "old-style" cognitive research to prevent one from investigating that? Certainly there is absolutely nothing in the cognitive science enterprise that denies the importance of the sorts of concerns that seem closest to Watson-Gegeo's heart, or the validity of investigating them. And in fact such research is being conducted, as Watson-Gegeo herself so ably illustrates, without any interference or disparagement from the cognitivists among L2 researchers.

The idea that SLA research is a part of cognitive science, and as such is an empirical science, is sometimes dismissed, usually by self-styled humanists, as either arrogance or misplaced "science envy"; this is a serious mistake. As William James once said (James, 1892, p. 467), the project of naturalizing the mind is actually an expression of modesty:

> When, then, we talk of "psychology as a natural science," we must not assume that that means a sort of psychology that stands at last on solid ground. It means just the reverse; it means a psychology particularly fragile, and into which the waters of metaphysical criticism leak at every joint, a psychology all of whose elementary assumptions and data must be reconsidered in wider connections and translated into other terms. It is, in short, a phrase of diffidence, and not of arrogance.

SLA-as-cognitive-science is an attempt to identify those aspects of the mental processes involved in L2 learning *that are amenable to investigation within a natural-science framework*, in an attempt to *explain* what goes on in the mind when an L2 is acquired. SLA research as cognitive science is, by virtue of its commitment to the enterprise of naturalizing the mind, prepared to acknowledge the existence of domains of interest that are simply not amenable to investigation with the tools of the natural sciences. This is not to dismiss such domains as uninteresting; it is simply to recognize one's limitations.

The problem, in fact, lies with the take-no-prisoners call for a new paradigm itself. A paradigm, after all, is a *dominant* outlook or approach within a scientific discipline, a set of values to which most or all members of the discipline hold allegiance. Now, it's one thing for a given discipline to achieve a paradigm, ideally through the development and progress of a theory and the research it supports. It's quite another thing, and a very odd thing indeed, to simply *proclaim* one, to urge that the members of the discipline just change their minds and toe a new line.

The real arrogance, in other words, is to be found in the disciplinary imperialism that tells all of us to restrict our interests to one area of the L2 phenomena, and in inflated claims about the scope and consequence of a given proposal. We are actually told, for instance, that Watson-Gegeo's proposals are "a new kind of science" (p. 29) calling into question "all that we hold dear, all that we have assumed, the theories close to our heart, the methods we have believed in, the goals we have set for our careers" (pp. 28-29). In a similar vein we are told that "applied linguistics is much more fundamental philosophically than mainstream linguistics, since its concerns go beyond theories of knowledge and theories of meaning, and reach into questions of 'being' itself" (Corson, 1997, p. 168). All of this, while at the same time we are not offered one single humble fact about how SLA takes place. If all we are to gain from the new paradigm is emptiness of empirical content combined with delusions of grandeur, we have every reason to continue with business as usual.

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