SLA: BREAKING THE SIEGE[1]

Michael H. Long

University of Hawai'i

After 30 years of steady growth and reasonable productivity, the field of SLA has recently come under attack from several quarters. Critics allege that, among other things, too many SLA researchers (a) focus overly narrowly on learner-internal, cognitive processes, ignoring social context; (b) inhabit an outdated modernist world, oblivious to the post-modernist "enlightenment;" and (c) believe their work has relevance for language teaching, when it has none. While few of the criticisms survive even cursory examination, the fact that they receive so much attention in the literature, including in some supposedly scholarly journals, suggests a number of structural problems in the field, which need to be addressed.

INTRODUCTION

In this paper, I address five issues. (a) What is the state of SLA? I will do a brief stock-taking. What has happened to our field institutionally in the past 30 years? What evidence is there as to whether or not it is becoming established as a credible new discipline? Why did people start doing SLA research in the first place, and why do people still do it? In other words, now that SLA is under attack from several quarters, is there anything worth defending? Next, I will summarize, and try to respond to, three broad charges made against SLA research, and certain researchers, myself included, in recent years. They are sweeping enough: (b) sociolinguistic naiveté, (c) modernism, and (d) irrelevance. Finally, (e) I will identify some structural problems which I think weaken SLA as an embryonic discipline, as shown not so much by the charges themselves as by the reception they have been given inside the field. I will make one or two suggestions as

---

[1] Plenary address to the third Pacific Second Language Research Forum (PacSLRF), Aoyama Gakuin University, Tokyo, March 26-29, 1998.

to ways in which at least some of the structural problems could be remedied. First, then, a very brief look at the history and scope of SLA. What has been achieved so far?

THE STATE OF SLA

History and Scope

Second language learning has fascinated observers for a long time, and valuable diary studies of child bilingualism were published as early as the first years of the twentieth century (for a historical listing, see Hatch, 1978). As shown by the coverage in textbooks, however, SLA as a modern field of study is generally accepted as dating from the late 1960's, meaning that the field as we know it today is still relatively young by the standards even of the social sciences. The focus of research during the first thirty years has been broad, encompassing the learning of second (third, fourth, etc.) languages, naturalistically or with the aid of formal instruction, by children and adults, as individuals or in groups, in foreign or second language settings. SLA research has embraced child bilingualism, pidginization and creolization, linguistic nativization, L2 attrition, loss and relearning, second dialect acquisition, acquisition within abnormal populations, acquisition in a variety of multicultural instructional settings, including bilingual and immersion classrooms, and acquisition in societies where the L2 is a lingua franca and very few of its users are native speakers. The broad scope, the rapidly expanding research literature, and the ever higher levels of technical expertise required of researchers in many areas, have made it increasingly difficult, if not impossible, for a single author to produce a comprehensive scholarly survey.

Although the field has expanded rapidly and diversified during the past 30 years, the principal motivations for studying SLA have remained constant. In addition to sheer intellectual curiosity, at least six broad academic and professional areas can be identified, many of whose participants have an interest in SLA, and some of whose work, equally, is of interest to SLA researchers. It is worth reminding ourselves of what those areas are. For one thing, most of us are usually preoccupied with just one or two of them, and can forget what drives other work in the field. For another, a brief stock-taking will serve us well when considering some of the accusations leveled at SLA researchers of late.

First language acquisition. Some problems in first language acquisition (L1A) are difficult or even impossible to resolve using first language data alone. One example is the relationship between language development and cognitive development, which are confounded in young children acquiring their L1, but (with some exceptions) distinct in adult L2 acquirers. Testing certain rival claims about putative sensitive periods for

language development is another. The so-called "exercise" and "maturational state" hypotheses about biological constraints on language development (Johnston & Newport, 1989) make the same prediction about L1A, i.e., that children will do better than adults, but opposing predictions about adult L2A, which therefore becomes the relevant testing ground. The exercise hypothesis claims that children and adults, having exercised their innate capacity for language learning successfully once, will do indistinguishably well with adult SLA; the maturational state hypothesis predicts child superiority there, the innate capacity supposedly having deteriorated or been lost altogether at the close of one or more sensitive periods, whether previously exercised or not. It is work in SLA that will resolve these issues, which are of interest to first and second language acquisition researchers alike.

Theoretical linguistics. Like much L1A research, a considerable number of SLA studies are motivated by, and in some cases chiefly of interest to, theoretical linguistics. Thus, claims by Keenan and Comrie (1976, and elsewhere) about typological markedness and relative clause formation across languages have inspired a continuing line of work on the development of relative clauses in an L2 (Doughty, 1991; Eckman et al, 1988; Gass, 1982; and Hamilton, 1994, among others). Studies testing various aspects of theories of Universal Grammar (UG) in SLA (e.g., Schwartz & Gubala-Ryzak, 1992; White, 1991) are so numerous as to have spawned their own journal, *Second Language Research*, but still to figure prominently in many others. Like SLA researchers working within the 'special nativist' and other frameworks, e.g., O'Grady's 'general nativist' theory (O'Grady, 1996; Wolfe-Quintero, 1996), the primary goal of UG-ers is not a theory of language teaching. This does not make work of this type any less valuable, however, as should not be necessary to point out, but regrettably is, given the attacks to which they have been subjected by some of the critics, as described below. Nor does it mean that advances in linguistically motivated work will necessarily be less relevant to language teaching in the long run, for the same reason that research in some area of microbiology may ultimately be no less relevant to medicine than a project that is avowedly "applied" from the outset. Linguistically motivated SLA research is relevant to linguistic theory and/or to a theory of second language acquisition, and the latter, at least, should be of interest to the language teaching establishment, since bringing about the acquisition of a second language is the teacher's goal.

Neurolinguistics. There is an obvious mutual interest among neurolinguists and SLA researchers in almost any advance in our understanding of how, when, and where linguistic knowledge develops and is represented in the brain, and of any neurophysiological conditions or changes that can affect any of these processes. Theories

as different as UG and connectionism involve explicit assumptions or claims about cerebral development and linguistic knowledge, and almost all SLA theories at least implicitly recognize the potential impact of such phenomena as lateralization, localization, modularity, and (putative) biologically based sensitive periods for language development. The same is true of several related areas of inquiry, such as first language acquisition and language learning within abnormal populations. Advances in brain sciences have stimulated interesting work in SLA, but the benefits have by no means been all on one side (see, e.g., Albert & Obler, 1978; Galloway, 1981; Jacobs, 1988; Jacobs & Schumann, 1992; Schumann, 1998).

Language learning within abnormal populations. The misfortunes of abnormal populations often create "natural experiments," in which people attempt language acquisition in unusual circumstances, e.g., without sight or hearing, relatively late in life (Curtiss, 1988; Newport, 1990), or without being able to negotiate their linguistic environment, as in cases of socially isolated hearing children of deaf parents (Sachs, Bard, & Johnson, 1981) and of child neglect (but not abuse). These 'experiments' have often provided insights into the language acquisition of 'normal' humans (see, e.g., Curtiss, 1980). It is heartening when some degree of reciprocity is achieved, therefore, and (S)LA research findings with normal populations prove useful to members of disadvantaged groups, such as the hearing impaired (Berent, 1996; Strong, 1988). An outstanding example is the excellent work of Cummins (1994) on the educational testing of immigrant children, and specifically the means of differentiating learning disabilities from difficulties caused by the education (and sometimes by the testing itself) being conducted through the medium of what for the children concerned is a second language. Many a child has been consigned to the "learning disabled" category, or worse, by a teacher or tester unfamiliar with normal processes and problems in L2 development. Another example is the influence of research on the role of various kinds of input and conversation for first and second language learning on the design of a language intervention program for Down's Syndrome children (see Mahoney, 1975, for an early report). Language intervention for some mentally different populations involves many of the same questions about manner, place, and timing as does conventional language teaching to adults. Mahoney successfully circumvented the problem created by an inadequate ratio of clinicians to patients in need of language intervention by investing a few hours a week in training the parents in how to converse optimally with their children so that they could provide a linguistically nurturing environment in the home.

Language teaching. The most obvious and widespread use of SLA theory and research is to improve language teaching. SLA, after all, is the process language teaching

is designed to facilitate. Numerous conclusions have been drawn over the years about how best to teach, and about how best not to, conclusions influenced strongly, although (rightfully so) far from exclusively, by work in our field (see, e.g., Brown, 1994; Doughty & Williams, 1998). Several books have appeared, one of whose principal purposes has been to survey SLA theory and research findings for language teachers (see, e.g., R. Ellis, 1997a; Gass, 1997; Lightbown & Spada, 1993). Views differ sharply as to the usefulness to date of SLA research in this regard, as well as to its potential for helping language teachers—issues to which we will return below—but the impact on ESL and foreign language teaching has been pervasive and undeniable.

Language in education. Just as important, and potentially affecting the educational life chances of hundreds of millions of people around the world, are the implications of SLA theory and research for the design, implementation, and evaluation of educational programs, often whole education systems, delivered through the *medium* of a second language. Whatever one's views as to the merit of particular proposals, work by Cummins, Genesee, Krashen, Lightbown, Swain, and others has had a major impact on bilingual, sheltered-subject-matter, and immersion education in both foreign and second language settings (see, e.g., California State Department of Education, 1984; Skutnabb-Kangas & Cummins, 1988). In my view, the field also has rich, thus far largely untapped, potential for education in second dialect situations, too, as evidenced by the recent furor over Ebonics in the U.S.A. (for some initial proposals, see Long, 1997a; Malcolm, 1995; Malcolm & Koscielecki, 1997; Sato, 1989; Siegel, 1992, 1996; and issues of the Pidgins and Creoles in Education (PACE) Newsletter[2]), and for educational language planning in general. In numerous countries on all five continents, children and adults, if they have access to education and training programs at all, must currently often receive the instruction through what is a second language for them, and sometimes both for them and their teachers (see, e.g., Watson-Gegeo & Gegeo, 1995). Most of the L2 literature related to such situations to date, while important, has treated macro-issues of politics, economics, education, and sociolinguistics, rather than work at the micro level of syllabus and materials design, methodology, and classroom processes. Both are needed. The situation is slowly changing, however, with SLA research beginning to influence individual educational decision-makers in a few of these societies—a trend that can be expected to grow in the coming years.

---

[2] The PACE Newsletter is obtainable free of charge by writing to Dr. Jeff Siegel, Department of Linguistics, University of New England, Armidale, NSW 2351, Australia, or by email at *jsiegel@metz.une.edu.au*

Intellectual curiosity. SLA presents numerous fascinating puzzles, many of the basic ones being discussed by Bley-Vroman (1989) in his classic paper on the "logical problem of foreign language learning." Why is it, for example, that almost all child L1A is successful, and on the face of it, effortlessly so, whereas SLA by almost all (some would say all) adults ends in partial failure? Why do so many failures occur even when adult SLA is attempted by people of high intelligence, clear motivation, ample opportunity to acquire, and also with what would seem to be the distinct advantage of having learned at least one language successfully already? This and other mysteries attract some SLA scholars, much like mountains attract some climbers, "because they are there." Unlike the 'fascinating puzzles' to be found in newspapers and airport book kiosks, however, those in SLA will, if solved, tell us something about the human mind.

Institutional Development

As is evident from the examples cited, SLA is an important area in cognitive science, and one with considerable social significance. This is reflected professionally and institutionally in many ways. The past three decades, and especially the 1990's, have witnessed impressive growth in almost every aspect of the field, except research funding.[3] For example, there are now at least four regular international conferences devoted exclusively to data-based SLA research: the Second Language Research Forum (SLRF), first held in 1977; the European Second Language Research Association (EUROSLA) conference, first held in 1991; the Pacific Second Language Research Forum (PacSLRF), first held in 1992; and the Generative Approaches to Second Language Acquisition (GASLA) conference, first held in 1987. Work in SLA also figures prominently at many other, more general, conferences on language acquisition, language teaching, linguistics, education, and applied linguistics. Three refereed international journals now specialize in

---

[3] Funding for SLA research has remained unchanged, i.e., virtually zero, in the U.S.A. for the past ten years, and declined almost everywhere else as part of a worldwide trend towards defunding public services in general, and public education in particular. The U.S. case is especially unfortunate, given that the need for SLA research there is acute and growing, and that it is in the U.S. that so many active SLA researchers live. The two principal U.S. federal agencies from which funding should emanate, but rarely does, are the National Science Foundation (NSF) and the U.S. Department of Education (USDOE). The former has no designated section for SLA, so the few researchers who still bother to apply at all usually do so to the Linguistics section. Many excellent proposals pass the first round of external reviews, but are then rejected by the inside panel of seven theoretical linguists. The USDOE is mostly interested in training, and when funding research even vaguely relevant to our field at all, supports studies of a heavily "applied" nature, such as descriptive studies of multicultural classrooms or evaluations of ESL and bilingual education programs. In contrast, funding for SLA research, both basic and applied, however scarce and decreasing in absolute terms, has remained *relatively* easy to obtain in Australia, Canada, and Europe during the same period.

SLA: *Language Learning, Studies in Second Language Acquisition,* and *Second Language Research* (founded in 1951, 1978 and 1985, respectively), and several more either specialize in studies of the acquisition of specific L2s or include SLA studies along with other material. They include: *Language Acquisition, ITL Review of Applied Linguistics, Applied Linguistics, Applied Psycholinguistics, Applied Language Learning, System, TESOL Quarterly, Spanish Applied Linguistics, The Modern Language Journal, Acquisition of Japanese as a Second Language, Zeitschrift fuer Fremdsprachenforschung, Linguistische Berichte, Die Neueren Sprachen, The Language Teacher,* and *Language Teaching Research.* Commercial publishers, not professional associations, control most of these journals, a point to which we will return. They also produce large numbers of books (of varying quality) about SLA, and the editors of recent encyclopedias of language, linguistics, applied linguistics, language teaching, education, and psychology have devoted substantial numbers of entries to SLA topics.

Growing numbers of universities in many countries offer advanced degree programs that include coursework and possible specializations in SLA (e.g., many of some 200 masters and 30 doctoral programs in ESL, SLA, language teaching and applied linguistics in the U.S.A. and Canada alone, listed in the 1995-97 TESOL and 1998 American Association for Applied Linguistics Graduate Program Directories). Moreover, it is now common to find faculty and graduate students doing coursework and SLA research in departments of psychology, linguistics, education, and foreign languages, among others. In some countries, it is departments in related disciplines, notably linguistics and psychology, at some universities (e.g., Bangor, Essex, Durham, Birkbeck, and Thames Valley) that are leading the way in research productivity in this area. The number of applicants for places in graduate programs specializing in SLA exceeds capacity in many cases.[4]

Perhaps not unrelated to the demand for graduate places, the job market for SLA scholars at the tertiary level is also reasonably healthy and expanding. In most countries, in an era of "restructuring" (a euphemism for massive cuts) in tertiary education as a whole, SLA, and applied linguistics in general, is, with a few notable exceptions, emerging *relatively* unscathed. In fact, demand for properly trained SLA faculty currently outstrips supply in the U.S.A. and several other countries. For over a decade now, there have typically been 40 to 50 tenure-track openings for applied linguists at universities in the U.S.A. and Canada each year, more and more of which list SLA as one of the

---

[4] For example, while large, with an average of 80 full-time students, the University of Hawai'i's M.A. in ESL program has typically accepted fewer than 50% of applicants, often fewer than 33%, for many years, and since its inception in 1989, admissions to the Ph.D. Program in SLA have run at around one in 20.

principal areas of specialization sought in applicants, often the main area. Colleagues frequently report difficulty in finding suitably qualified candidates. In a comparatively recent development, in addition to the types of programs that have traditionally employed and trained such people, increasing numbers of foreign language departments are seeking at least one specialist in SLA and language teaching to supplement their previous pure diet of linguists and literature specialists. This trend in the U.S.A. seems strongest so far in programs in Spanish and Japanese, with German, Korean, and other European and Asian language faculties beginning to follow suit. If there is a dark cloud on the horizon, it is that SLA, like applied linguistics in general, is coming of age as a field at the very time when public education in general, and public higher education in particular, is under assault almost everywhere, and when politicians and university presidents are ever seeking new ways to "streamline" programs in the relentless drive to make their institutions "leaner and meaner," emulating the corporate model so many of them admire. This is not a good time for a relatively new discipline to be seeking institutional recognition, e.g., departmental status. It remains to be seen, in fact, whether existing units and programs will remain autonomous or, as seems more likely in some cases, "merged with" (i.e., disappear into) older, politically more powerful departments— typically Linguistics, Education, or even English. Many are in such departments already and currently seem likely to stay there. Students wishing to study SLA and applied linguistics often make up a large proportion of their students, but typically are served by only a small proportion of their faculty.

As might be expected with such rapid expansion and from its broad scope, SLA continues as it started, an interdisciplinary endeavor. Practitioners can still be found whose primary training is in one or more of the fields of linguistics, ESL, psychology, applied linguistics, anthropology, education, and foreign languages. Serious degree programs in SLA typically provide obligatory coursework not just in L2 acquisition, but in L2 analysis, education, and use, as well as in research methods. Publications in the field reflect this diversity, most obviously in preferred research methodologies, with use of everything from ethnography to experiment, and from grammaticality judgments to "free speech" (for review, see, e.g., Doughty & Long, to appear). The tolerance and respect for difference is no doubt healthy in the long run, but it sometimes risks creating unhelpful fragmentation in the short term. It is also probably one of several reasons why so much air-time of late has been provided at conferences and in the SLA literature for some ideas that would in a more mature discipline quickly be shown the door.

THE CHARGES

Against a background of three decades of steady growth and reasonable success, the field has recently been loudly criticized from a number of quarters, and the criticisms entertained in supposedly scholarly journals. Among several charges that have been leveled, mostly by individuals outside the field, at work in SLA in general, and at named individuals, in particular, three of the most frequent and inflammatory are: (a) sociolinguistic naiveté, (b) modernism, and (c) irrelevance, at least, irrelevance for language teaching. The three charges are sometimes made separately, but are also, in the first two cases, especially, often confused and conflated. I will attempt to summarize two representative critics' arguments in each case as fairly as possible, letting their own words speak for them, and then to show that most of the accusations are themselves naïve and irrelevant, but potentially very damaging, nonetheless, meaning that they need to be addressed. It is important at the outset to grasp the scope of the charges: even when specific individuals or studies are targeted, it is often large segments of the field or SLA as a whole that are under siege.

Sociolinguistic Naiveté

A number of writers, most notably Firth and Wagner (1997, henceforth, F & W), have criticized SLA research in the most sweeping terms for "ignoring social context" (see, also, Firth, 1996; Tarone, 1997; Wagner, 1996, and, especially, the important and more constructive work of Rampton, 1987, 1995, 1997a, b, c, and elsewhere). By this, F & W mean such things as the following: (a) that SLA researchers should take account of the fact that learners, including participants in SLA studies, have social identities in life, e.g., 'father,' 'friend,' or 'business partner,' in addition to those of 'NS,' 'NNS,' and 'learner'—categories about which F & W, like others (e.g., Davies, 1991), are, in general, highly skeptical; (b) that acquisition often takes place in contexts in which little or no access to NSs is available[5]; (c) that conversational norms vary, so that there is no standard way of speaking that is modified for learners, casting doubt on strong claims for the role in acquisition of certain kinds of experiences with the target language; and, (d) (surely uncontroversially) that it can be unwarranted to generalize findings from experimental to natural settings. F & W's criticisms are ostensibly aimed at "discourse" studies in SLA, but as suggested by the title of their article in a special issue of the *The Modern Language Journal* in 1997—'On discourse, communication, and (some) fundamental concepts in

---

[5] 'NS,' 'NNS' and 'learner' are categories which F & W alternately condemn and utilize themselves.

SLA research'—their target is much broader. Any ambiguity as to what F & W really have in mind is dispelled in the very first lines of their abstract:

> This article argues for a reconceptualization of SLA research that would enlarge the ontological and empirical parameters of the field. We claim that methodologies, theories and foci within SLA reflect an imbalance between cognitive and mentalistic orientations, and social and contextual orientations to language, the former orientation being unquestionably in the ascendancy (1997, p. 285)

and in an early footnote:

> ... our critique of SLA is based on the view that the field has core interests, theoretical predilections, methodologies, and basic assumptions. It is this SLA "core" that has the focus of our attention here. (1997, p. 296, fn.1)

F & W, it transpires, believe SLA researchers should give up their preoccupation with what goes on in the learner's mind, with what Rampton (1987, p. 49) refers to as the space between a speaker and his or her interlanguage grammar. The focus, they assert, should shift from such linguistic and "mentalistic" matters as input, innate knowledge, cognitive factors, linguistic processing, mental representations of L2 grammars, and mechanisms and processes in interlanguage (IL) change, where it is now, to a "more balanced" treatment of cognitive and (largely unspecified) social factors, one that will correct the "general methodological bias and theoretical imbalance in SLA studies that investigate acquisition through interactive discourse" (1997, p. 288). Note, incidentally, that a major source of confusion in F & W's reasoning is their tendency (e.g., 1997, pp. 287-288) to equate all "cognitivist" work in SLA with the narrower meaning of 'cognitive' in studies conducted within a UG framework, of which they are highly critical. This is unfortunate and highly misleading. UG-ers may have the single most coherent research program (some would say the only one) and some of the best minds in the field, but most cognitively-oriented SLA-ers are not UG-ers. They include general nativists, for example, (I believe) all the cognitive/social-interactionists whom F & W attack in their article, and literally hundreds of other "cognitively oriented" researchers who are not mentioned by F & W, presumably because they work predominantly with interlanguage, rather than conversational, data. F & W's misrepresentation continues, despite the error having been pointed out explicitly (Long, 1997b, p. 322, fn. 2). In an attempted response to their critics, they write:

> In our view, SLA seems to be dominated by Chomskian thinking to such a degree that others' frames of reference for the understanding of language and cognition have become inconceivable. (F & W, 1998, p. 92)

They may have become 'inconceivable' to F & W, but clearly not to SLA researchers. The 1998 PacSLRF in Tokyo, for instance, was fairly typical of the larger SLA conferences in that it included a small minority of (roughly 10) UG-motivated papers amidst numerous others (over 100) from various cognitive perspectives, including, not least, a day-long symposium, 'Cognition and SLA,' which consisted of nine papers by prominent figures in the field on such topics as attention, sentence-processing, automaticity, aptitude, focus on form, and incidental and intentional learning, not one of them operating within a generative framework.

It is clear that F & W really have in mind much more than simply a "more balanced" view, given such statements as the following:

> As part of this examination, we discuss the status of some fundamental concepts in SLA, principally nonnative speaker (NNS), learner, and interlanguage. These concepts prefigure as monolithic elements in SLA, their status venerated and seemingly assured within the field. We claim that, for the most part, they are applied and understood in an oversimplified manner, leading, among other things, to an analytic mindset that elevates an idealized "native" speaker above a stereotypicalized "nonnative," while viewing the latter as a defective communicator, limited by an underdeveloped communicative competence. (1997, p. 285)

> The predominant view in discourse and communication within SLA, F & W assert, is: individualistic and mechanistic, and ... fails to account in a satisfactory way for interactional and sociolinguistic dimensions of language. As such, it is flawed, and obviates insight into *the nature of language, most centrally the language use* of second or foreign language (S/FL) speakers. (1997, p. 285, emphasis added)

The field is said to privilege settings in which the L2 is widely spoken or taught by NSs and to ignore those in which few, if any, participants are natives. This means, for example, that SLA should be studied, or should have been studied (more often), in societies like India, Nigeria, or Singapore, where English and other languages may serve as lingua francas, or in foreign language settings, when, e.g., English is used as a means of communication by a Dane and a German discussing a business deal on the telephone. By (allegedly) ignoring such settings, and "while the field in general perpetuates the theoretical imbalances and skewed perspectives on discourse and communication" (1997, p. 296), F & W assert, SLA will not realize its "potential to make significant contributions to a wide range of research issues conventionally seen to reside outside its boundaries" (1997, p. 296). They recognize "the growing number of SLA studies, mainly of an ethnographic nature, that are socially and contextually oriented" (1997, p. 286), but contend that

most tend to take the formal learning environment (i.e., the S/FL classroom) as their point of departure. Thus, although S/FL interactions occurring in non-instructional settings are everyday occurrences (e.g., in the workplace), they have not, as yet, attracted the attention of SLA researchers. (1997, p. 286)

F & W and others are quite correct in pointing out that few published studies to date have focused on acquisition in what we will here loosely call 'lingua franca' environments, or situations in which "non-standard" or "nativized" varieties have been involved (but see, e.g., Fraser Gupta, 1994; Warner, 1996; Williams, 1988). Rather than being the result of theoretical myopia or methodological imbalance, however, I suggest that this is simply a reflection of the situations in which, and the constraints under which, most SLA researchers work (see, e.g., fn.[3], concerning research funding). Also, the force of F & W's criticism could only increase if they were to suggest the kind of new insights about SLA they envisaged from studies in such environments. This is not to say the research could not be motivated, simply that F & W have not done so. With respect to the alleged paucity of research in naturalistic settings, on the other hand, one can only assume F & W are either unfamiliar with, or dissatisfied with, the publications of work in naturalistic (non-instructional) settings by the likes of Clyne (1977); Huebner (1983), Meisel, Clahsen, and Pienemann (1981), Sato (1990), Schmidt (1983), Terrell (1990), and Watson-Gegeo (1992), among many others, not to mention some of the research motivated by interests in the six broad issues outlined earlier other than language teaching. Note that a common criticism of SLA research from F & W and other members of the "discourse community" is that too many studies are conducted in classrooms, not enough in naturalistic settings. As we shall see, the "irrelevance community" argue that SLA research has little or nothing to say to language teachers because researchers conduct too much of their work in naturalistic settings, and not enough in classrooms.

Research in sociolinguistics, F & W note, has "irrefutably established and documented [a] reflexive relationship between *language use and social context*" (1997, p. 293, emphasis added). And who would deny it? They continue:

Language is not only a cognitive phenomenon, the product of the individual's brain; it is also *fundamentally a social phenomenon*, acquired and used interactively in a variety of contexts and for myriad practical purposes (1997, p. 296, emphasis added).

Some *would* deny that. But assertion and counter-assertion will get us nowhere. What F & W need to show for their criticisms to have substance is *which* social dimensions of language use and social context allegedly ignored by SLA researchers play an important enough role in SLA to justify reorienting the field to a "more balanced"

consideration of "both cognitive and social factors." Indeed, perhaps to whet our appetite, it would be nice if F & W could provide data showing how even *one* of the social dimensions to which they allude has any influence on acquisition (as opposed to use) at all. This they signally fail to do. Instead, what they offered (F & W, 1997) were lengthy re-analyses of brief excerpts from conversational data treated in "mainstream" discourse analysis studies, the results of which (if one accepts the validity of the re-analyses, which the original researchers generally do not, judging from the responses to F & W's article by those criticized, published in the same issue of *The Modern Language Journal*, and by Gass, 1998) is that the L2 speakers might occasionally have meant something different in one or more of their utterances, and that this might have been appreciated if the original researchers had taken more contextual information into account. Even *if* the re-analyses are accepted as valid, nowhere do F & W say what the import of any of this even might be for *acquisition*. It is impossible to falsify an existence claim, i.e., in this case, for SLA researchers to prove the *irrelevance* of context, however defined, to acquisition; it is up to F & W to show that context *is* relevant. Nowhere do they provide evidence to that effect. This did not, however, give them pause before bringing a sweeping indictment of the field. Nor did it preclude their article being published in a major journal.

Quite apart from the complete absence of evidence from F & W, here or elsewhere, that SLA is indeed 'flawed,' as they claim, since when has the 'nature of language' or the 'language use' of L2 speakers been the explanandum for SLA? Is SLA really supposed to be concerned with language, and 'most centrally' with L2 use, as F & W assert that it should be, or with L2 learning? The answer, surely, is clear: as Kasper (1997) underscores in the title of her trenchant response to F & W, "'A' stands for acquisition." The same point is made by Poulisse (1997), Gass (1998), and Long (1997b) in their replies. Few researchers would deny the *potential* importance of the interactional and sociolinguistic context in which SLA occurs, least of all the individuals F & W criticize most strongly as representative of the mainstream, many of whom, paralleling work in L1A, have spent years studying the putative effects on acquisition of modified input, output, feedback, task-type, and various kinds of negotiation opportunities, in conversations between learners and NSs, and among NNSs themselves (for review, see Gass, 1997; Long, 1996a; Pica, Lincoln-Porter, Paninos, & Linnell, 1996).

Theories of (S)LA differ, of course, in the significance of the role they attribute to the linguistic—let alone the social and sociolinguistic—environment, and researchers also differ in their interpretations of the empirical findings to date. Some, e.g., Grimshaw and Pinker (1989), view linguistic input to the learner as degenerate and underspecified;

others, e.g., Bohannon, MacWhinney, and Snow (1990), see it as tailored for acquisition in various ways. Some view negative evidence and negative feedback, e.g., in the form of recasts, as non-existent, unusable, unused, or non-universal (and thus non-essential), or irrelevant for competence, in any case; others think they have shown that such feedback is available and can be at least facilitative of acquisition (Doughty & Varela, 1998; Mackey, 1995; Oliver, 1995; Ortega & Long, 1997). Some schools within SLA, particularly some groups within the UG camp, are highly skeptical of this latter work on theoretical grounds (see, e.g., Schwartz, 1993), and given the well documented resilience of the human language-learning capacity in child L1A, even in cases of severe linguistic deprivation, theirs is surely a very reasonable starting hypothesis. But my point here is that, as Eckman (1994) argued in connection with a similar debate over research on the role of variation in SLA, this is surely an empirical matter, not one to be settled a priori by fiat by the UG-ers, by the cognitive-interactionists, or in the present case, by the new champions of social context. I suggest, moreover, that the same standards apply to anyone, whether working inside the field or, like F & W and some other critics, mostly outside it, who claims that SLA should or should not focus on a broader array of social factors (e.g., on other social identities of speakers than 'NS' and 'learner,' such as 'father,' 'friend' or 'business partner'), that researchers should or should not use this or that methodology in discourse studies (for F & W, Schegloffian conversational analysis), or should or should not adhere to this or that theory. Instead of dismissing all past work as 'narrow' and 'flawed,' and simply asserting that SLA researchers should therefore change their data base and analyses to take new elements into account, F & W should offer at least some evidence that, e.g., a richer understanding of alternate social identities of people currently treated as 'learners,' or a broader view of social context, makes a difference, and a difference not just to the way this or that tiny stretch of discourse is interpretable, but to our understanding of acquisition.

No-one is preventing F & W from doing work of the kind they advocate. In fact, it would be welcomed. Their protestations at feeling like 'trespassers' on SLA "private property" (F & W, 1998, p. 91 et infra) are simply absurd. However, it seems that evidence is in fact unlikely to be forthcoming, from them at least, any time soon. Near the end of their 1997 paper, they write:

> In essence, we call for work within SLA that endeavors to adopt what we have referred to as a holistic approach ... A crucially important and challenging *next step* is to develop, in much greater detail, the *theoretical bases*, a *research agenda*, and a set of *methodological approaches* that are aligned with the "reconceptualization" here espoused. (F & W, 1997, p. 296, emphases added)

It seems that these critics, at least, have not only not produced any work of the kind they nevertheless think the whole field should start doing, but have also yet to work out even what is to be done, or how. Until F & W or someone else antes up (for one attempt, see Tarone & Liu, 1995), the basic stance of SLA researchers will likely remain the same, and in my view, for good reason.

Whether eventually turning out to be influenced by social factors and social context or not, SLA is certainly, in large part at least, a mental process: the acquisition of new linguistic knowledge. Language acquisition usually takes place in a social setting, to be sure, as do most internal processes—learning, thinking, remembering, sexual arousal, and digestion, for example—and that neither obviates the need for theories of those processes, nor shifts the goal of inquiry to the settings themselves. Remove a learner from the social setting, and the L2 grammar does not change or disappear. Change the social setting altogether, e.g., from street to classroom, or from a foreign to a second language environment, and, as far as we know, the way the learner acquires does not change much, either, as suggested, e.g., by comparisons of error types, developmental sequences, processing constraints, and other aspects of the acquisition process in and out of classrooms (see, e.g., R. Ellis, 1989; Johnston, 1985, 1998; Lightbown, 1983; Pica, 1984). A 12-hour flight from a foreign language to a second language environment does not alter a learner's brain, after all, so why should one *expect* any basic differences? Conversely, alter the *cognitive*, or *psycholinguistic*, dimensions of a task or of task conditions—attentional focus, planning time, familiarity, difficulty, complexity, provision of negative feedback, processing constraints, etc.—and performance changes in ways that seem relevant for acquisition (see, e.g., Doughty & Williams, 1998; N. Ellis, 1995; N. Ellis & Laporte, 1995; Hulstijn, 1992; Hulstijn & Hulstijn, 1984; Mackey, 1995; Manheimer, 1993; Newton & Kennedy, 1996; Ortega, in press; Pienemann, 1989; Rahimpour, 1997; Robinson, Ting, & Irwin, 1995; Schmidt, 1995).

Unfortunately, the cognitive and the psycholinguistic are not the dimensions of context that interest F & W and their "discourse" colleagues, probably because they see language primarily as a social and cultural phenomenon, and, as we have seen, are more interested in language use than acquisition. Psycholinguistic results do interest SLA researchers, however, since one of our major goals is to understand how changes in the internal mental representation, or interlanguage grammar, are achieved, why they sometimes appear to cease (so-called stabilization and "fossilization"), and which learner, linguistic, and social factors (and if relevant, which instructional practices) affect and effect the acquisition process. F & W may be making a valuable contribution by arguing for a broader, context-sensitive, participant-sensitive, emic approach, and for the use of

more naturalistic data sampled from a wider range of acquisition settings (and, I would add, on a wider range of languages). Given the thus-far-productive focus on SLA as a mental process, however, and given the absence of any evidence to support a fundamental change in approach, cognitive factors and what F & W refer to as "cognitively oriented theories and methodologies" will inevitably remain high on the agenda, whether those laying siege from the "discourse" camp approve or not.

   Before closing this section, it is important to note that, symptomatic of the confusion among several of SLA's loudest critics, F & W attempt to bolster their "social context" case by an unfortunate appeal to epistemological relativism, thereby conflating what are two quite separate issues. Already in the second paragraph of their article, they write:

> Our critical assessment of some of SLA's core concepts is, in part, a reaction to recent discussions on theoretical issues in the field [by Beretta, Crookes, Ellis, Gregg, and Long, which reflect a desire] to introduce "quality control" on the basis of "established" and "normal" scientific standards. (1997, pp. 285-286)

Noting that Block (1996) had challenged assumptions underlying such discussions, F & W side with him in disputing the desirability of "normal science" and in viewing the existence of multiple theories of SLA as unproblematic. Indeed, F & W go further than Block in one regard, asserting multiplicity of theories to be a *given* in their world:

> the fact remains [NB: there are *facts* in F & W's world] that the branch of the discipline dealing with discourse and communication is, and always has been, *of necessity multitheoretical* in its adopted approaches and conceptual apparatus." (1997, p. 286, emphasis added)

F & W offer no evidence or reasoning (there could be none) to support their assertion as to the *necessity* of such a situation. The remark is important in the present context, however, because it reveals basic misunderstandings of the differences between (a) accepting multiple theories and multiple competing, or oppositional, theories, and (b) the interpretations relativists and rationalists, e.g., realists, would put on either situation. Both are subjects to which we will return. As we shall see, as part of their post-modernist critique of SLA research, Block (1996) and Lantolf (1996a) make the same egregious error—confusing pluralism and relativism—and are then allowed by journal editors to use it as a stick with which to beat rationalists. While F & W's subsequent re-analyses of excerpts from other researchers' work show that, in fact, they do not really share Block's relativist views, some critics of SLA do, so let us now turn to the second group of besiegers, the post-modernists.

Modernism

After some two decades of ferment in many literature, cultural studies, philosophy, and political science departments at universities in Europe and North America, postmodernism made an appearance in SLA and applied linguistics only fairly recently (see, e.g., Pennycook, 1996; van Lier, 1994). The pace is picking up, however. A 1996 issue of *Language Learning* included a lengthy article by Lantolf (1996a) purporting to provide a "postmodernist critical analysis" of some work on theory construction and evaluation in SLA by Beretta, Crookes, Gregg, and Long, and an issue of *Applied Linguistics* in the same year unleashed what can only be described as a post-modernist attack article (Block, 1996), aimed principally at the usual suspects, but once again encompassing more general expressions of its author's unhappiness with the whole field. An evaluation of these criticisms requires some understanding of their philosophical underpinnings.

Dating from the late eighteenth century Enlightenment, *modernism* is associated with beliefs in the self-motivated, rational subject, individual freedom, self-determination, the possibility of steady progress through scientific discovery, and a capacity for self-emancipation in the struggle for a more just, humane society. Modernists put their faith, in particular, in rational thinking and science. *Postmodernism* is the antithesis of all that. (For accessible introductions to post-modernist ideas, see Rosenau, 1992; Usher & Edwards, 1994). Post-modernists espouse a belief in decentered, socially constructed subjects. In place of science and reason, various of them value chaos, desire, and the unconscious. As with all forms of epistemological relativism, claims for the existence of "correct" ways of knowing (e.g., scientific ways) or for "correct" knowledge systems (e.g., accepted research findings) are regarded as spurious. Belief systems like rationality and science are examples of what Lyotard (1984) and other post-modernists call 'grand narratives,' which they view with 'incredulity.' For them, *all knowledge is socially constructed.* It exists only in discourses, in texts, and not even objectively in the texts themselves, but in *each writer's or reader's interpretation of those texts.* There is no fact of the matter (although *that* is supposed to be accepted as a fact), but instead, multiple interpretations and multiple realities, and for some relativists, at least (so-called judgmental relativists), anyone's construction of reality is as good as anyone else's. Thus, Lechte (1994, p. 236) reports that in a notorious series of articles in the French newspaper, *Liberation*, a leading French post-modernist, Baudrillard, appeared to claim that the Gulf Massacre of 1991 had not taken place. The personal horror stories and misery of 200,000 young Iraqi widows and orphans had no more *truth* to them than the musings of an aging French "intellectual" thousands of miles away. For post-modernists

and relativists in general, we all "construct our own narratives," our own versions and understandings of events and of the world, and there is no objective way of distinguishing among them, and no point in doing so. (This, incidentally, is one of many places where F & W can be seen not really to be relativists at all, despite their endorsement of Block, since they attempt, whether successfully or not, to show that their analyses of other researchers' data are truer to reality than those of the original authors.)

For post-modernists, as Usher and Edwards (1994) describe, one sort of evidence—or, at least, what post-modernists count as evidence—that modernist beliefs are ill-founded consists of the continuing existence of social ills: war, famine, disease, genocide, and environmental pollution. If science and reason can do what modernists claim for them, why have they not been able to deal with those problems? Thus, for some post-modernists, horrific, "irrational" human acts count as evidence for their position. Bauman (1992) and Lyotard (1992), for example, see the Holocaust as the ultimate in the cold, calculated use of scientific principles for social engineering, and hence an indictment of rationality and science. The allegation that major human problems continue to exist due to a failure of science and reason—as opposed to the more obvious culprits, the selfish acts of corporate-sponsored elites wielding state power[6]—while silly enough, deteriorates still further into a denunciation of "the" scientific method (as if there were just one), usually equated with positivism. Predictably, therefore, as we shall see below, positivism is one of many accusations made by Lantolf (e.g., 1996a, p. 714) against named individuals in SLA, despite its being a philosophy to which, to the best of my knowledge, not a single SLA researcher subscribes. Given its allegedly poor results, "the scientific method" (sic) must also be flawed, post-modernists claim, particularly when it comes to its advocates' (alleged) claims to objectivity, neutrality, and an ability to produce value-free, true knowledge. Knowledge is not universal or independent of time, place, and the people producing it, the post-modernists claim; instead, it is incomplete, local, particular, socially and historically conditioned, fluid, not static, and serves the interests of those in power.

---

[6] Several of the leading lights in French post-modernism spent their early years as members of a variety of Stalinist and Maoist sects. It comes as no surprise, therefore, given the traditional authoritarian socialist need to discredit the libertarian ideas they rightly fear (see, also, Brumfit, 1997, p. 25-26), that some French (or French-style) intellectuals claim that the writings of one the most widely respected living anarchist philosophers, Noam Chomsky, employ outdated strategies that are "unable to accommodate the subtleties of political movements" (Barsky, 1997, p. 197). Chomsky's reply to this is that the French intelligentsia are unwilling to see what is clearly set out before them, and should learn "how to tell the truth, to pay attention to facts, and to reach standards of minimal rationality" (31 March, 1995, letter to Robert Barsky, quoted in Barsky, 1997, p. 197).

Ideological underpinnings and motives aside, however, some of the *issues* post-modernists raise are serious ones. They have a direct impact on the ways researchers proceed (or if they proceed at all), and those, in turn, can have major consequences for the success of their work and the benefits to interested parties. A key example is the role of theory construction. To illustrate, let us consider how things play out when a realist—perhaps the most common variant of rationalist among today's SLA researchers—and a relativist confront the same situation in SLA: a case of multiple theories in the same domain.

While there are several different varieties of *realism* and many nuances within those varieties, realists believe that even if it can never be fully comprehended, an objective, external world exists, independent of any individual's or group's (social) construction of it, and that there are *universals*, as well, of course, as individual differences and particularities of time and place. Thus, realists do not believe that, say, the laws of gravity or of thermodynamics only apply in Spain, only to short people, only on Tuesdays, or only to those who believe in them. They maintain that it is possible to demonstrate this by repeated observation, e.g., that apples always fall to the ground, never float up into the sky, whenever and wherever a study is conducted, and no matter who conducts it. That is to say, they believe *there are facts of the matter*. Realists also believe that while imperfect, our methods of inquiry permit us to discover whether at least some of our beliefs about that objective world are true, or more or less likely to be true than other beliefs, i.e., to differentiate among conflicting views, such as rival theories, by measuring the degree to which they are or are not borne out by observations of events and experiences. In other words, it is possible at least to approximate the truth, without necessarily ever being able to be sure that a belief *is* the truth. Contrary to what is often alleged, realism entails no allegiance to any one method of inquiry, or to the idea that one method (e.g., the so-called "scientific method") is better than others—simply an allegiance to *rational inquiry*. Nor, it needs to be emphasized, does it entail a belief in absolute truth. Thus, as Gregg (1997, pp. 11-12) points out, if an epistemological realist sees two or more theories in some domain of SLA, especially, but not only, oppositional ones, his or her reaction will be that, since there is only one objective reality, (a) one of the theories *may* be correct, (b) all of them *may* be false, (c) all but one *must* be false, and (d) empirical observation, among other approaches, can potentially find out which is which. One might add, although this is not part of the realist's brief *qua* realist, that if that is then done, and if the increased understanding provided by the supported theory offers some practical benefits, e.g., a potential application in technology, medicine, or

language teaching, then our collective knowledge of the world has increased, and some people's lot within it may improve as a result.

As noted above, the same situation for a *relativist* is quite different. If a relativist sees the same multiplicity of theories, the tendency will be simply to accept the situation as unproblematic, an inevitable reflection of everyone having "their own narrative," their own construction of reality. Even if the texts in which theories live are "interpreted," and however "richly," there is no independent way of arbitrating between rival interpretations in the event that the participants cannot agree, and no need to do so. If the meaning of a text is rightfully, and in each case differently, in the mind of the writer and each reader, then as Gregg notes, "there's no arguing about what a given text means" (1997, p. 5). And there's the rub. Contrary to what the post-modernists claim, comparing and evaluating theories is not an option, not a luxury, not an ivory-tower activity, not a game, not impossible, but *essential* for progress in science. In fact, it is what progress consists in, since theories are our current approximations to truth—perhaps as close as we will ever come—our interim understandings, or explanations, of how things work. And not all explanations are as good as the others. Maintaining that they are is a fundamental flaw in the relativist argument. It misses the fact, and it is a fact, that what decides whether a theory is good or not, or better than another theory, is not whether we say it is, but how well it works. How does the theory fare when tested against the way the world is, i.e., when tested empirically? Holding that a plurality of theories is unproblematic, even inevitable, therefore, is to obstruct progress in SLA, and shows that in science, as in the political arena, post-modernism and relativism are inherently reactionary and inhibitory of progress, for they permit any position to be defended equally well, regardless of its apparent truth and regardless of the social consequences. Far from threatening the status quo, therefore, post-modernists and relativists help perpetuate it. As with Marxist, Stalinist, and Maoist political goals, there is no interest in changing the system itself, just who, or in this case, which theories, are in a position of power within it.

Building on assertions first made in his plenary address to the 1995 BAAL conference (Lantolf, 1996b), Lantolf's main claim in the *Language Learning* article (Lantolf, 1996a) was that theories of SLA, like all theories, in his view, are simply *metaphors*, whose productivity as stimuli for research, he further asserted (against all the evidence from the natural and social sciences, as Gregg, 1997, notes), is in inverse proportion to their acceptance within a discipline. Theory (or metaphor) proliferation is something to be welcomed, therefore, as new theories (or metaphors) are likely to stimulate more and more research of diverse kinds. Beretta (1991) and Long (1993), conversely, had argued that progress in SLA was being hampered by an unwillingness to test or cull theories,

especially oppositional theories, or to evaluate them comparatively using reasonably well established standards from other sciences. Long (1993) had suggested four candidate criteria from a much longer list of possibilities: (a) simplicity; (b) the ability to explain phenomena different from those for which the theory was invented to account; (c) (possibly) the ability to make surprising novel predictions; and (d) (provided it is not applied too early in the theory-development process) empirical adequacy. Lantolf rejected any such proposals, arguing that the field should let all the flowers bloom.

Lantolf's view is reminiscent of the metaphor of SLA theories as "pictures in an art gallery," proposed by Schumann (1983) during his brief flirtation with relativism 15 years earlier.[7] It is a stance, in my view, that trivializes the field and ignores (for those who believe in such things) some rather salient facts, not least the fact that most SLA researchers, like researchers in other fields, do not simply trade 'metaphors' (or 'pictures') back and forth; they formulate increasingly precise predictions, and test them against what is thought to be known about language learning, however imperfect and incomplete, *and very possibly wrong*, that knowledge may turn out to be. While a new claim's initial acceptance may sometimes depend in part on the power or rhetorical persuasiveness of its advocates, in the long run, it will survive if it receives empirical support, i.e., appears to be correct when compared against events in the natural world, and will be dropped if not. Something fundamental the post-modernists refuse to deal with is that the success or failure of theories depends on how they match up with the way the world is (in this case, how people learn languages), not the way the theorists say it is. This is a cardinal distinction between science, on the one hand, and *irrational* belief systems, like relativism, on the other. Relativists can continue to do business because they can find enough people willing, for a variety of reasons, to accept what they say the world is like, regardless of perceived reality, without the need for evidence or independent verification, or because, while personally disagreeing with some claims, they believe that "anything goes" and are fatalistic with regard to human progress. Rationalists may not have higher standards, but they do have different ones.
When rival theories exist in a domain, there are two basic possibilities, Lantolf, says. If the observer is of the *judgmental relativist* persuasion, which holds that all knowledge—being socially constructed and merely a reflection of the current distribution of power—is equally valid, that will be the end of the matter. If an *epistemic relativist*, the individual concerned may seek to determine which theory is more successful, not via empirical

---

[7] Schumann publicly disavowed any continuing allegiance to relativism in 1991 at the Michigan State University conference on 'Theory and research methodology in SLA'.

testing, but through the interpretation (sometimes the interpretation of interpretations) of texts, i.e., what Lantolf (1996a, p. 734) refers to euphemistically as "rational social discourse." In a cogent, as yet unpublished, response to Lantolf, Gregg (1997) notes that epistemic relativism, as portrayed by Lantolf, sounds like having your relativist cake and eating it, too. He writes, paraphrasing Lantolf:

> we can maintain that there is no objective reality out there and hence no way of preferring one theory to another on grounds of explanatory adequacy or empirical adequacy, while still making rational choices among theories ... . Would that it were that simple. For one thing, on what *relativistic* grounds does one reject judgmental relativism? (Gregg, 1997, p. 15)

In his paper, Gregg dismantles Lantolf's theory-as-metaphor argument and provides some timely comments on the post-modernist "contribution" to SLA in general. He also documents a number of major misrepresentations, misunderstandings, and ill-founded accusations in Lantolf's critique of the rationalist SLA theory-construction literature. For example, offering no evidence (there is none), but following the standard post-modernist script described earlier, Lantolf accuses Gregg et al of

> a commitment to the rationalist epistemology and (despite claims to the contrary) the *positivist* legacy that continues to pervade SLA research" (1996a, p. 714, emphasis added).

Lantolf does not reference the 'claims to the contrary,' but, as Gregg (1997, p. 18, fn. 9) points out, was presumably referring to the explicit denial in Gregg et al's response to Block (1996), to which, as one of the editors of *Applied Linguistics*, he had access before publication. In that paper, not only did Gregg et al explicitly reject any allegiance to positivism (it is doubtful whether anyone in SLA has ever really subscribed to that position), they also stated quite clearly what their allegiances were, and are:

> a 'critical realist' ontology ('reality exists but can never be fully comprehended') combine[d] with a 'modified objectivist' epistemology ('objectivity remains a regulatory ideal') and a 'modified experimental/manipulative' methodology" (Gregg et al, 1996, p. 550)

This is about as explicit as one could get, and many steps removed from positivism, but it did not deter Lantolf from making his groundless accusation, nor *Language Learning* from publishing it.

Elsewhere in his paper, having stated, without evidence and quite untruly, that "SLA theory builders share ... a common fear of the dreaded 'relativism'" (p. 715), Lantolf cites Long's statement concerning the typical co-existence of three or more theories even

during a period of Kuhnian normal science, and concludes that "[a]pparently, for Long relativism is not so bad after all, provided it is a constrained relativism" (1996a, p. 732).

Echoing the earlier discussion, I can do no better than cite Gregg on this blatant misunderstanding of fundamental concepts:

> Lantolf confuses relativism with pluralism ...The situation Long describes, where there are three competing theories in a given domain, justifies no conclusion as to what the epistemological commitments of the theoreticians are. A relativist would presumably think that all three theories are OK, where a realist or a positivist (they are not the same) would argue that of the three, at least two are incorrect, if not all three ... In other words, the question of plurality of theories is a red herring as far as the relativism issue goes. (Gregg, 1997, p. 12)

F & W, we saw, made the same error when they stated that discourse studies in SLA must of necessity always be multi-theoretical.

Another accusation that occurs with some frequency in the writings of the post-modernist critics of SLA, although it is not limited to them, is the accusation that those of us interested in theory evaluation are suffering from some form of misplaced "science envy". Lantolf writes, for example, that Long (1993, p. 235) illustrates the "SLA theory builders' reverence for the natural science model" (1996a, p. 716), and later, that "[T]he theory builders are experiencing an episode of ... 'physics envy'" (1996a, p. 717). Block (1996, p. 73, to appear) dutifully repeats the allegation twice. Gregg et al (1996, p. 544) point out, however, that this name-calling reflects more on those making the accusations than on those charged. Lantolf, Block, and others apparently think that SLA is not a science, and that the rationalists' goal is to make it one. In fact, rationalist SLA researchers, i.e., the overwhelming majority of SLA researchers, are already doing science, however well or poorly. The writing on theory construction is no different from that by some of the same individuals and many others on L2 research methods; the goal in each case is to improve the quality of work in our field and to speed up progress. Writing on research methods addresses research quality issues at the grass-roots level, so to speak; writing about theory construction and philosophy of (social) science issues deals with broader disciplinary matters. Why the latter (alone) should come in for so much abuse remains a mystery. But it does, and none more so of late than from Block (1996, to appear).

An article by Block (1996) in *Applied Linguistics* is a distressingly convoluted and ill-informed exposition of the relativist case. Among numerous charges against the usual suspects, Block chastises Long (1990) for having suggested the existence of some 'widely accepted findings' in SLA for which a theory of SLA should account. Block denies that

such findings exist—without providing any evidence for his view, so presumably based on the relativist assertion that there is no "correct" knowledge, thereby confusing 'correct' with what had been claimed, i.e., that the findings were 'widely accepted.' He rejects even the *surely* uncontroversial generalization to the effect that

> (I)nterlanguages, the psycholinguistic equivalent of idiolects, exhibit sytematicity and variability at any time in their development (Selinker, 1969; Huebner, 1985)" (Long, 1990, p. 658)

Yet Block approves of the existence of multiple theories of SLA. As Gregg et al (1997, pp. 549-550) point out in their response, this is an odd position, even for a relativist. There is normally some agreement in science about many of the phenomena (i.e., putative "findings") to be explained, but disagreement about the correct explanation for them. Block, conversely, endorses a multiplicity of explanations for phenomena he claims SLA researchers have not yet agreed exist.

Block confuses 'theory' and 'paradigm,' by which latter term Kuhn meant 'dominant theory' (see, also, Laudan & Laudan, 1989), and also fails to grasp Kuhn's discussions of 'pre-science' and 'normal science.' As Gregg et al (1997, p. 546) underscore, Kuhn said that fields begin with a pre-scientific period, characterized by a chaos of competing theories that makes progress difficult. When a dominant theory emerges, a stage of normal science begins, during which research becomes cumulative, and applications of the theory can be harvested. This is achieved because the scientific community concerned has agreed on the basic issues and unified behind the dominant theory, or paradigm. Non-paradigmatic research typically continues, however, with histories of science showing three or more theories to be the norm, *even during such periods* (Collins, 1989), and substantive discoveries arising from research conducted outside the dominant paradigm.

Block also denies the need for, or indeed the very possibility of, either replication studies or control of extraneous variables in SLA research, which he states is "probably not even desirable" (p. 74), while admitting that this is "a major philosophical difference between my stance and that of many authors" (p. 74). Indeed, it is. He asks rhetorically (p. 74), "What good does a theory developed to explain the general do for a situation which is particular?," and thereby reveals another fundamental misunderstanding about the scope and purpose of all theories. He strongly rejects any move to reduce the number of competing theories in SLA in favor of a controlling paradigm, although that is something none of the accused had ever suggested, or could bring about even if they wished to do so.

It is not just replication studies, control of variables, and normal science to which Block objects, but rational inquiry in general. In its place, he advocates relativism as a viable basis for a research program in SLA, yet he offers absolutely no indication as to what such an alternative might look like, and [like F & W] points to no examples of such work—probably because, to the best of my knowledge, none exists. Instead, he simply pledges allegiance to "constructivism" (see Lincoln, 1990). Constructivism involves the belief that the results of inquiries are always a function of the interaction of the inquirer and the inquired into, such that "what can be known and the individual who comes to know it are fused into a coherent whole" (Guba, 1990, p. 26). The methodology is hermeneutic and dialectic, with each inquirer required to compare and contrast their individual construction of reality with every other such individual construction "so that each respondent must confront the constructions of others and come to terms with them" (Guba, 1990, p. 26). What (on earth) this would mean in SLA research, Block says nothing about. Would advocates of mutually exclusive oppositional theories have to negotiate a compromise: half the abstract syntactic principles posited for UG are innate, and half learned, and so on? A hint of the "insights" in store can perhaps be found, however, in Block's criticism of Long's (1990) according of primacy to cognitive factors (over social and affective ones) in SLA theory. Block claims (1996, p. 76) that this risks widening the gap between researchers and practitioners because, he says, it ignores the fact that "language lessons [not SLA theory, note] are essentially social events which are co-constructed by the individuals participating in them."

As if all this were not more than enough, Block throws in several sociology of science issues for good measure—"black boxing," linguistic imperialism, male chauvinism in teacher education, unequal power relationships in applied linguistics, the obligatory "science envy" charge, and on and on—alleging that such problems exist, as always without providing any evidence, but making insinuations, nonetheless. To give but one example, he suggests that writers languishing in the (undefined) geographical (note, not intellectual) "periphery" and/or espousing views that ran counter to prevailing opinion, might find their work rejected by journals in the (undefined) anglophone "center" if theoretical pluralism were reduced, and then uses that entirely unsupported speculation as an argument against the suggested need for theory evaluation. It is worth quoting him on this point, as I will be returning to the question of the editing of purportedly scholarly journals in our field when suggesting some ways of breaking the siege. In a stunning series of paranoid speculations and non sequiturs masquerading as an argument, Block writes:

While *I have no reason to believe* that qualitatively different views about research exist in [other] parts of the world, *I would venture to say* that *if they did exist* they would not be *likely* to get into print ... In addition different views *might* run into resistance from the gatekeepers of the profession, the journal editors and the reviewers themselves, who *might* not consider the research reported to be in line with the expressed 'line' of the journal." (Block, 1996, p. 67, emphases added)

He offers no evidence for any of this, as usual, and again, it is not something that anyone could ever prove does *not* occur. The onus is on Block to come up with, if not an example of this happening, at least some reasons for believing that it might. He fails to do that, while spicing the pot with equally unsubstantiated allegations of anglophone cultural hegemony (repeated in Block, to appear, despite explicit rebuttal by Gregg et al, 1997). As documented by Gregg et al (1997), the rest of Block (1996) contained numerous additional vague charges against individuals and organizations in SLA— charges of dishonesty, bad faith, discrimination, etc.—all unsubstantiated, as well as gross misrepresentations of our and others' work. And again, Block's piece was accepted for publication by the editors (Lantolf & McCarthy) of a supposedly scholarly journal in applied linguistics. It took over 18 months for them to publish Gregg et al's reply, which, due to the time lapse and separation from the original, will never have been seen by many readers of the attack article.

It is *not* true that a multiplicity of theories in a field is unproblematic—especially when some of those theories are oppositional, but not only then—for that is tantamount to a declaration of irresponsibility, or else a belief that progress is unattainable, an acceptance that "anything goes." And that is untrue if SLA is, as I have tried to show, a field that has made considerable empirical progress during its short existence, and unacceptable if, as I have also tried to show, it is a field with considerable social consequence for millions of people all over the world. Claims that theory proliferation is of no concern because there is no objective reality, no facts of the matter, that everyone is his or her own theorist, that all knowledge is socially constructed and non-generalizable, are quickly forgotten in the face of a rapidly approaching Mack truck. Every time relativists cross a street, they make use of the same rapid mental calculations based on distance and the velocity of oncoming traffic as everyone else. I find it hard to believe that "anything goes" is an attitude, moreover, that the post-modernists or relativists would dream of accepting for one moment in the purveyors of social services they routinely utilize—doctors, dentists, nurses, architects, engineers, mechanics, airline pilots, and so on—people whose work is successful precisely because it is based on the accumulated research findings of decades of *rational* inquiry in the basic and applied sciences, the very

kind of research the critics claim to reject in SLA[8] Are post-modernists and relativists prepared to accept something less in language teachers, in educators, or in those who work with disadvantaged populations? Why is SLA different? Why are the six related fields and the countless people mentioned at the outset who stand to benefit from SLA research findings different? Can Lantolf, Block, and their supposed sympathizers, like Firth and Wagner, explain why those people and their needs are unimportant and why SLA is unimportant? And will the editors of journals who publish such shoddily argued material explain their reasons for doing so?

Irrelevance for Language Teaching

The third group currently laying siege to SLA are some defenders of the status quo in language teaching. This may come as something of a surprise, particularly to those many SLA researchers who, like myself, entered the field after substantial and varied experience as classroom teachers and teacher educators. Many SLA researchers have witnessed first-hand the relatively few successes and the widespread failures of even the best-intentioned classroom instruction, and many of us were first motivated to undergo training as SLA researchers with a view to improving that state of affairs.

Language teaching has been going on for a very long time. Histories of the field (see, e.g., Howatt, 1984; Kelly, 1969; Musumeci, 1997) show that many of the same issues debated by pedagogues today have occupied their predecessors for centuries, with first one side, and then the other emerging victorious—temporarily. In the absence of either a widely accepted theory of language learning or a solid empirical base for classroom practice, teachers and learners have always been, and will always be, vulnerable to drastic pendulum swings of fashion, the coming and going of various unconventional and unlamented "Wonder Methods" being an obvious recent example. The sad truth is that after at least 2,000 years, most language teaching takes place on a wing and a prayer— sometimes successfully, but often a relative failure. As Richards (1984) showed, this makes the field prey to charismatic personalities, e.g., the Methods gurus of the 1970's and 1980's, and to powerful governmental and commercial interests, e.g., quasi-governmental entities promoting the books of publishers from the country whose commercial interests they represent, and the publishers themselves with a heavy investment in, and huge profits from, a textbook series implementing this or that

---

[8] Redhead (1995, p. 15) put it this way: "For quarks it may not matter so much, but in everyday life I believe it really does make a difference whether we believe in medical science as against witchcraft and spells, and I know for sure which jetliner I want to travel in – the realist's not the relativist's!"

approach to teaching. Bigger political forces are often also at work.[9] When then U.S. Vice-President Dan Quayle, not a renowned expert on language matters, publicly urged thousands of young Americans to join the Peace Corps and go to newly "liberated" eastern Europe to teach thirsty Hungarians, Rumanians, and others English, was it because of a sudden interest in foreign language learning on his part, or because of a realization that English is the language of international capitalism, and that, as Pennycook (1995) pointed out, 'English in the world' quickly becomes 'the world in English'? And it was not just the Peace Corps whose credibility suffered from Quayle's support. Once again, teaching EFL was tacitly represented as an activity requiring no special knowledge or training, something that could be done at the drop of a trade barrier by the next God-fearing citizen with a sudden urge to share American culture with unsuspecting foreigners.

Against this backdrop of many more L2 beginners and repeatedly "false beginners" than finishers, a lack of theoretical or empirical grounding, and a consequent susceptibility to pendulum swings, one might have expected research on SLA to be welcomed with open arms, especially, but by no means only, when it has applications, or even just implications, for the classroom. Again, SLA is the process, as noted earlier, that language teaching is designed to facilitate. Welcomes, at least from self-appointed pedagogic gatekeepers, are few and far between, however. Instead, suggestions from SLA researchers (many of them experienced classroom language teachers themselves, remember), especially if explicit, and, it seems, especially if perceived as presenting a challenge to current commercial interests and orthodoxy, have been swiftly and repeatedly subjected to attack. Explicit, relatively detailed proposals, such as those for various kinds of task-based language teaching (e.g., Long, 1985; Long & Crookes, 1992; Nunan, 1991), seem especially threatening to some, who miss the point that explicitness is, of course, valued in rational inquiry since the more explicit a claim is, the more easily testable, and hence quickly falsifiable, it is. Explicitness is a plus for people who are seriously interested in progress, for it helps if bad ideas are easily identifiable as such. Opaque distinctions and vaguely worded claims, conversely, are less easily recognized as empty when that is the case, and so tend to have unwarrantedly long life expectancies, thereby inhibiting progress. People will be less likely to search for alternatives as long as they think existing knowledge is adequate.

---

[9] For an analysis of the (sometimes unwitting) role of various quasi-governmental agencies, including the Peace Corps, in projecting U.S. foreign policy abroad, see the special issue of *Covert Action Information Bulletin* 39, Winter 1991-92.

Advocates for the status quo, or in some cases for times past[10], offer a variety of arguments against SLA-influenced language teaching proposals. Sheen, for example, has repeatedly asserted that proposed innovations which lack massive, repeated, longitudinal, field testing in real classrooms should not be allowed into real classrooms (Sheen, 1994, and elsewhere). The fact that any pedagogic innovation, from SLA research or anywhere else, must, by definition, be allowed into classrooms before it can be tested there, much less validated, seems to escape him. So does the lack of any such testing, which is simply ignored, much less empirical vindication, of his own preferred practices, e.g., grammar translation and "principled eclecticism" (Sheen, 1998), whatever that might be, different proposals apparently warranting different standards. Sheen and his ilk arrogantly claim perceived classroom successes as victories for their "methods," and dismiss failures, if they acknowledge them at all, sometimes even blaming the students. During an attack on an EFL teacher in Japan who had questioned the widespread use of grammar translation in that country, for example, Sheen wrote:

> The eclectic approach which Bailey espouses does not preclude the use of elements of the GTM [Grammar Translation Method]. As both a learner and teacher, I would regard them as essential ... I have taught both French and English ... and have often found the students equally baffled by the simplest of questions after more than five years of study using methods ranging from the GTM to the most functional. If we have not learned much else in the last decades of intensive research, surely we have learned to resist the temptation to lay such store by the method used and lay at its feet the failings of the students. (Sheen, 1992, p. 45)

When things go wrong, it is not the teacher or the Method that is to blame, Sheen says, but the students. The Sheens of this world ignore the very real possibility that some learners succeed despite their methods, and that it is the failures for which they should claim credit.

Some of the language teaching establishment's loudest critics of SLA research-influenced proposals ignore the fact that there is often a complete lack of anything more than impressionistic evidence for the teaching strategies they advocate. These include (a) the imposition of a pre-set, external, synthetic grammatical syllabus, regardless of learners' current readiness to learn the items on Monday morning's menu or to do so in the order presented, (b) the use of explicit grammar rules, and (c) "error correction" (sic) to prevent or undo "fossilization" (sic), something which would be literally impossible if fossilization were real. The strategies are frequently incompatible with SLA theory and research findings in both naturalistic and instructed settings, but that is, apparently, of no

---

[10] See, e.g., 'In defense of grammar translation' (Sheen, 1992)

concern to them. At the same time, some, like Sheen, complain bitterly about what they assert (usually wrongly) is a lack of evidence for proposals emanating from SLA researchers. It is as if a physician continued to advocate blood-letting as a cure for cancer in 1998, claimed success in the cases of the few patients who recovered for other reasons, dismissed the numerous failures as having been caused by aggravating factors beyond their control (such as the patients not trying to stay alive hard enough), and vociferously attacked any researchers and practitioners with other ideas and research findings to back them up, especially if they proposed trying them out in hospitals.

Another line of assault on SLA, and a handy way of dismissing whatever ideas are under discussion without having to deal with them, is to deny that SLA research alone constitutes an adequate basis on which to make proposals for the classroom. But who has ever suggested that SLA alone is an adequate base? To my knowledge, no-one. Most SLA researchers with an interest in classroom teaching have espoused some variant of the far more inclusive approaches described by Crookes (1997a, 1997b); R. Ellis (1997a, 1997b); and Long (to appear); Long and Crookes (1992), among others, in which information from *multiple* sources is utilized in program design—needs and means analyses, SLA, micro- and macro-sociolinguistics, language analysis, psychology, education, educational psychology, and more—and in which, moreover, the information flow between those sources and classroom teaching experience is anything but unidirectional. A variant of this gambit is to assert that language teaching is "an art," and that it is, therefore, wrong and ultimately hopeless to try to bring "scientific" findings to bear on the potentially endless, sometimes centuries-long, arguments among self-appointed experts on pedagogy. Would the same people claim that medicine is an art, or engineering, or law, or architecture? Why is education different in kind? When all else fails, as it does, the assailant may always resort to a charge of political correctness. Thus, Sheen pronounced Task-Based Language Teaching, as proposed by Long and Crookes (1992), to be a product of "the liberal ethos that has permeated the (sic) approach to teaching in recent decades, particularly in the field of ESL" (Sheen, 1994, p. 144).

Despite having been exposed as confused, confusing, and riddled with misrepresentations (see, e.g., Long, 1994; Nunan, 1994), Sheen's endlessly repetitious diatribes against a mystifyingly select sub-group of SLA researchers (mostly Krashen, Long, R. Ellis, and Nunan) have been published for five years now in *TESOL Quarterly*, *RELC Journal*, and *Applied Linguistics*, among other places. Why?

Fortunately, not all discussions of SLA-influenced proposals for language teaching within pedagogy and applied linguistics circles are pitched at such a disappointing level. Several prominent applied linguists, including some who have made important

contributions to syllabus design, have begun to reevaluate their own earlier proposals in light of SLA findings, and, probably more important in the long run, increasing numbers of experienced practicing teachers with formal training in SLA are doing so. However, even here, the discussion is sometimes less substantive or rigorous than might have been expected. Thus, to cite but one example, due to space limitations, in a plenary address to the Korean Association of Language Teachers conference, Wilkins (1994) reviewed some basic options in syllabus design in the light of SLA research findings, particularly proposals for Task-Based Language Teaching (Long, 1985, to appear) as summarized in Long and Crookes (1992).

Unlike Block or Sheen, Wilkins starts by acknowledging several facts about interlanguage development uncovered by SLA research, e.g., that learning often exhibits U-shaped and zigzag learning curves; that students do not learn isolated L2 items one at a time, in additive, linear fashion; that learners rarely, if ever, move from zero to target-like mastery of new items in one step; that both naturalistic and classroom learners pass through fixed developmental sequences, stages that often entail quite lengthy stages of non-target-like use of forms; and that these sequences (*not* SLA in toto, note) seem impervious to instruction. He also recognizes the problems such findings pose for approaches to language teaching which involve synthetic (e.g., structural) syllabuses and accompanying methodology, as well as for the use of linguistically simplified texts, as found in commercially published "graded readers" series, and the research findings' greater compatibility with analytic, e.g., task-based approaches. So far, so good.

Wilkins next claims that the notional syllabus, with which his name is rightly associated, is not really synthetic, as Long and Crookes had stated, simply because it itemized linguistic content. Referring to his crucial distinction between synthetic and analytic syllabuses, he writes:

> It was my view that all syllabuses that provide linguistic diversity should be considered to be analytic and this would include the notional and functional syllabuses ... Generally speaking, any individual social function or semantic category is associated with diverse linguistic forms. It may be that the difference of view arises from different perceptions of how those syllabuses might actually be implemented. (Wilkins, 1994, p. 47)

First, quite apart from the fact that 'linguistic diversity' is undefined (would a grammatical syllabus that included a wide range of target structures qualify as 'linguistically diverse,' and so as analytic?), this is a rather different understanding of what constitutes an analytic syllabus from that Wilkins proposed 25 years ago. The synthetic/analytic distinction then had to do, respectively, with the learner's role either as

synthesizer of linguistic forms presented discretely, or as analyzer of gestalt target language samples presented holistically. Second, Wilkins may be right in suggesting that the way both types of syllabus might be implemented could affect perceptions of them. However, there have been one or two (admittedly small-scale) studies of how variants of notional-functional syllabuses and communicative language teaching, at least, are implemented (see, e.g., Long, Adams, McLean, & Castanos, 1976; Nunan, 1987; Phillips & Shettlesworth, 1975), which all found that they tended to look uncannily like structural language teaching in the classroom. Wilkins does not refer to any of those studies, or to any in support of his speculation that notional syllabuses really can be implemented differently.

There then follows, in light of his earlier acknowledgment of SLA research findings, a rather puzzling attempt to defend synthetic syllabuses, which is even more troubling. For example, he asserts that structurally graded, inauthentic materials are not inevitably bad, and notes that: "(N)o evidence is cited [by Long & Crookes, 1992] that learners cannot learn language from what the authors would regard as non-authentic material" (1994, p. 51)

Once again, one is being asked to offer evidence that something cannot occur. Along with the impossibility of meeting such a demand, might not at least some evidence more legitimately be expected of, and more easily produced by, those advocating an approach that has long been implemented in classrooms all over the world? Lack of authenticity, Wilkins continues, moreover, is virtually a given, and, in any case, in his view, unproblematic:

> The fact is that in any ordinary sense of the word authentic there are virtually no authentic uses of language in the classroom other than those which relate to the functioning of the classroom itself ... This is not a problem ... It is doubtful whether authenticity is an issue with which learners themselves are greatly concerned ... The urge to (so-called) authenticity should be seen as the shibboleth that it is. (1994, pp. 51-52)

Here, Wilkins, like every discussant of the 'authenticity' issue from a pedagogic perspective that I am aware of, misses its crucial psycholinguistic dimension (see Long, 1996b). In any case, Long and Crookes had not argued for the use of 'found' texts (texts originally produced for communication among NSs, not written for language teaching, which is the traditional meaning of 'authentic'). He also misses the distinction between authentic *texts* and authentic *tasks* as the relevant starting point both in classroom language learning, and for the discussion itself (see Long, 1996b). Next, apart from the fact that it is pure speculation, the suggestion that (some? all?) learners may not be

'greatly concerned' about authenticity is surely irrelevant. Learners may or may not even be aware of, much less concerned about, a great many issues and options in language teaching, but then, that is not their responsibility, but ours as teachers and applied linguists. Far from being 'a shibboleth,' successful manipulation of the psycholinguistic dimension of authenticity (e.g., where elaboration, rather than simplification, is utilized to increase comprehensibility without removing learnable grammatical or lexical L2 forms from the input) is probably a critical factor in determining the success or failure of language teaching. As yet, no-one knows for sure, but Wilkins dismisses the issue out of hand, apparently unaware of the numerous studies of the relative effectiveness of (what he considers) 'authentic' texts, and simplified and elaborated versions thereof (for review, see, e.g., Long, 1996b; Yano, Long, & Ross, 1994).

Elsewhere in his discussion, Wilkins suggests that structural syllabuses do not necessarily preempt the operation of developmental processes:

> It can be said, though, in defense of the structural approach to language teaching, that *however damaging this may be to the principles on which such an approach is based*, the practice inevitably involved a degree of holistic learning. (1994, p. 48, emphasis added)

But even if that were true (and Wilkins provides no evidence that it is), how is the *possibility that X might not always fail* a legitimate argument against the *probability of Y succeeding*? Then comes a demand for *more* empirical studies and for answers to questions (most already available in the SLA literature) before a suggestion should be seriously entertained, while, again, like Sheen, glossing over the absence of any such evidence for the position he is defending:

> ... what is the sequential relationship, if any, between the different syntactic categories [in the developmental sequences]? ... what were the methods by which the acquisition data were collected? ... how much variation is there between individuals? ... just how impervious to instruction are these sequences? [etc.] ... (1994, pp. 49-52)

There is sometimes a tinge of straw-man argumentation here, too:

> ... the step from seeing second language learning as a holistic process to concluding that it consists of "fixed developmental sequences" "impervious to instruction" is a very large one. In my view the evidence needs to be far more comprehensive and convincing than it is at the moment before we would wish to establish this as the sole basis for language teaching practice. (1994, p. 49)

Long and Crookes nowhere claim, as is implied here, either that SLA is "impervious to instruction" (for reviews of research showing multiple *effects* of instruction, see, e.g.,

Long, 1983, 1988) or that this idea and "fixed developmental sequences" should form the "sole basis" for language teaching practice, or indeed, for anything at all. And once again, there is a demand for "far more comprehensive and convincing" evidence. How much more? Convincing to whom? What is inadequate about the dozens of studies of these issues to date? Why is the lack of empirical evidence supporting the position Wilkins is defending not of equal concern?

In sum, while not typical of all discussion of SLA findings for language teaching, many of which are positive, these papers by Sheen and Wilkins are, in my experience, representative (at very different levels) of much of it. In addition to the problems pointed out, there is a disappointing unwillingness to recognize the insights that language teaching has quietly absorbed from SLA theory and research over the past three decades, with their source now seemingly forgotten. Since the hey-day of the Contrastive Analysis Hypothesis and Audiolingualism just 30 years ago, for example, when linguists ruled the language teaching roost, SLA has furnished far from complete, but very much improved, understandings of language learning in and out of classrooms, including: the role of positive and negative evidence; L1 transfer; learner error; a range of implicit and explicit forms of negative feedback; a number of options in modifying target language samples so as to improve their comprehensibility without sacrificing learning targets; practice, automatization, and restructuring; variation; accuracy, complexity, and fluency; attention and metalinguistic awareness; individual differences; incidental, implicit, and explicit learning; and the relative utility of a focus on the target language as object or medium of communication. For accessible sources on the implications for language teaching of research findings on many of these and other topics, see, e.g., Brown (1994), Doughty and Williams (1998), Lightbown and Spada (1993), and Robinson (1997, 1998). There may occasionally have been some ill-founded proposals, but with this fairly solid record overall, why should current ideas be dismissed out of hand?

BREAKING THE SIEGE

The Danger of a Siege Mentality

Paying too much attention to our besiegers risks legitimizing some of the woollier critics of SLA, who might be better simply ignored, as well as some of their more absurd criticisms. Worse, it could gradually create a siege mentality and isolate the field from its constituents and from neighboring disciplines. On the other hand, completely ignoring all the current assaults (and insults) also seems ill advised. First, while most of the charges are unsupported now, as far as one can tell, perhaps in the case of the "social context"

criticisms, at least, some may eventually turn out to have merit. Second, however convoluted the prose, the attention post-modernist critiques draw to the often hidden dimensions of expertise, patriarchy, and power relationships in science, among other matters usually treated by the sociology of science, while coming decades after anarchist exposés of many of the same issues (see, e.g., Hales, 1986; Igor, 1985; Kropotkin, 1899a, b; Martin, 1991; Purchase, 1996), is also valuable, even if the post-modernists' solutions are not. Third, equally clearly, it would be foolish to ignore critiques of language-teaching proposals by experienced and reputable applied linguists, like Wilkins. Fourth, discouraging or reducing diversity (as opposed to chaos) is almost always a bad idea in any walk of life, and theoretical and methodological diversity in SLA research is likely to be no exception.

By the same token, it does seem important to respond swiftly and forcefully to some critics and charges before they damage SLA as a field, and its potential beneficiaries, more than they already have, especially when the criticisms are published in what in a few cases are currently (but how much longer?) considered major journals. Some of these people, especially the post-modernists, a few journal editors and publishers, are having the effect of portraying SLA, unjustifiedly, as fundamentally misguided, as a dangerous threat, or both. This is harmful for research-funding prospects, obstructs progress, diverts attention from the real issues, wastes a lot of people's time and energy, and hinders improvement in language teaching and other domains which stand to benefit from SLA findings.

Post-modernism, in particular, has had a pernicious effect in many areas, from literature, art, and architecture to political science and cultural studies, as well as on social activism (see, e.g., Albert, 1996, and discussions in subsequent issues of *Z Magazine*). The hollowness of the post-modernists' critique of science has been revealed on numerous occasions, not least by publication of Sokal's famous hoax article (Sokal, 1996) in the North American post-modernist journal, *Social Text*, along with the substantial commentaries that followed elsewhere. The post-modernist critique of SLA should be exposed for what it is right away, before this field, too, becomes mired in the same unproductive "debates" that have side-tracked other disciplines. Even in the short term, strident voices repeatedly asserting that research is futile and that academic debates within SLA are just a game (see, e.g., Block, to appear) are likely to damage SLA's credibility with such groups as university administrators, language teachers, funding agencies, and individuals from other disciplines sitting on university promotion and tenure committees. The standard of scholarship and argumentation found in some of the published critiques, moreover—*whatever* one thinks of their contents—is sometimes so

low (replete with ad hominem attacks, distortions, straw men, unsupported assertions, and mis-citations) that the credibility of one or two of the journals concerned has already been adversely affected, thereby potentially risking *all* publications in the them being tainted in the eyes of some observers. One can easily envisage the day when work in some journals will no longer be considered relevant by university administrators and faculty making decisions about such matters as the future of units and degree programs concerned with SLA, just as has reportedly already happened in some instances when applied linguists have attempted to establish new programs or have sought promotion and tenure from committees made up primarily of members from other disciplines, such as linguistics.

The hostility towards language teaching proposals based partly on SLA research findings from some (although, thankfully, by no means all) applied linguists and language teachers is a different matter. A healthy skepticism towards premature proposals is obviously to be welcomed, and not a problem at all. Reasoned debate of the *issues*, moreover, suggests a growing maturity in the field, e.g., the existence of a recognized knowledge base (widely accepted findings) to which proposals can be held accountable. Trashing suggestions for no good reason, conversely, apart from being insulting to the people doing the work, is likely (perhaps intended?) to frighten off teachers and prospective graduate students who could benefit from a grounding in SLA research, but who understandably do not yet know enough about the original studies to judge for themselves, and so have to rely on the self-appointed intermediaries, some of whom obviously know precious little about SLA research, either. It is also damaging to the ultimate consumers, i.e., classroom language learners, whose instruction will remain subject to the vicissitudes of fashion and to the undue influence of commercial publishers until the situation changes. As should be obvious from the previous discussion, this is *not* to say SLA research has all the answers, of course, or ever will, and certainly not that current proposals, including those for Task-Based Language Teaching or focus on form, for instance, are 'correct.' It is simply to protect the integrity of a field, many of whose participants have already contributed much to language teaching, and who will assuredly contribute much more if given a hearing.

Gate-Keeping, Structural Fault-Lines, and the Way Ahead

A number of experienced SLA researchers who have commented informally on the recent spate of attack articles have wondered openly how individuals who were in some cases quite unknown and untrained as SLA researchers themselves obtained access to supposedly scholarly journals, especially when their allegations lacked substance and, in one or two cases, were so poorly articulated. SLA, like several areas of applied

linguistics, has its own knowledge base, but one would never know it from reading some of the literature discussed earlier or from many of the posts to at least one email discussion list. How likely is it that people with little or no formal training or publication record in, say, linguistics, psychology, biology or chemistry, and with minimal familiarity with their literature, would be given access to major journals in those fields in order to indict all previous work in them, unless the indictments were well reasoned and supported? Would the individuals and field attacked lose credibility, or would it be the editors and journals concerned? Some of the recent publications discussed in this paper raise many questions about the current state of SLA and its potential as a discipline. They suggest the existence of structural fault-lines, including a number usually dealt with elsewhere under the rubric of the sociology of science, most of which are outside the (already broad enough) scope of this paper. An obvious one in the present context, however, is the state of journals and journal-editing in the field. There are one or two fine journals, and there have been, and are, some fine editors, in my view, but this often appears fortuitous, since there is usually an obvious lack of procedures in place to ensure that quality is maintained, or in some cases, ever achieved. Many aspects of how some journals are run give serious cause for concern, especially when it is remembered that peer review is one of the important ways in which bias is minimized and quality maximized in a field. The refereed literature in any area has a status different from that of commercial publications, and this should be the case in SLA, too.

Who has entry to the supposedly refereed SLA literature? Who are the gatekeepers? Why are almost all SLA and applied linguistics journals controlled or owned by commercial publishers and/or a particular university department? Does it matter that their editors are often selected behind closed doors, on the say-so of company representatives or of one or two powerful individuals inside or outside the field, or that the editors thus chosen are then in some cases free to restock editorial advisory boards with like-minded individuals, some of whom have a minimal track record or none at all in the journal concerned, or even in the field, over which they are now to exercise considerable power? How it is that, in some notorious cases, the individuals attacked have subsequently been denied an opportunity to respond by the same editors? Is it important that few journals have any written policies or procedures at all covering such matters as "right of reply" or even refereeing, or that some editors appear to flout such policies as do exist when so inclined? Is it important that *all* articles in a purportedly refereed journal are in fact refereed? Can any journal in the field be edited satisfactorily by only one or two people any longer, given the diversity and technicality of so much work in SLA today, or is a team of specialist co-editors, each with their own panel of

expert reviewers, now needed? Is it a justified concern that no remedies exist when arbitrary and capricious decisions are made?

Solutions to these problems, if you agree with me that they are problems, may vary, and obviously require careful discussion. In the short term, to set the ball rolling, on a par with (say) the Linguistic Society of America and its highly prestigious journal, *Language*, I believe the time has come to establish one or more international SLA associations, whose principal function would be to publish one or more SLA journals of the highest calibre, a journal or journals whose editors, editorial board members and outside reviewers were selected openly, democratically, and on merit, and whose policies and procedures were written, open to public inspection, applied rigorously and, above all, applied consistently. The association(s) would be inclusive, open to anyone, and charge minimal dues. These would be chiefly to pay the subscription costs of the journal(s). Procedures would be in place from the outset to ensure that all schools of thought were represented, and that no one view-point could dominate through bias in the way editors were selected, or in the composition of editorial boards or reviewer panels. Again following worthy examples in linguistics, psychology, and other more mature disciplines, I think there is a parallel need for high quality email discussion lists in SLA, again, open to all, but some (not all) of them moderated, so as to exclude personal attacks, and perhaps with the number of posts any one person can make per day limited in some rational way in order to prevent subscribers' in-boxes being deluged with incessant posts from the same individuals. Tightening up scholarly journal practices (and in other areas of gate-keeping not discussed here) may be necessary to keep SLA viable long enough institutionally for the field to show what it can contribute in cognitive science and to society as a whole.

Many of these matters are simply teething problems common to any emerging discipline, and fairly easily remediable. In the longer term, I believe the solution is to emphasize and encourage rational inquiry and empirical research. SLA has more than enough intellectually stimulating and socially important problems to be solved, and more than enough hard work to go around. If given the opportunity, it will ultimately succeed or fail as a discipline on its achievements, and there is every reason for optimism here. One area where the training given future researchers in the field could be improved, in my view, however, is in the philosophy of science. Few even of the top graduate programs in SLA and applied linguistics provide coursework in this area as yet, but I think it should become routine, just as training in research methods now is. What sense is there in turning out SLA researchers with a thorough grounding in qualitative and quantitative research methods, in numerous procedures for gathering and analyzing language data, and

expertise in measurement and statistics, but who know little or nothing about theory construction and change, or about how sciences work? Are SLA researchers fully competent if well versed in day-to-day, grass-roots research procedures, but lacking much idea of how what they are doing fits in with other work in the field, that is, of how to assess the big picture? Experts in SLA and applied linguistics with advanced formal training in philosophy of science are sorely needed. Meanwhile, excellent written sources exist for those with an initial interest in such matters (see, e.g., Corvalis, 1997; Diesing, 1991; Kitcher, 1993; Laudan, 1990, 1996; Riggs, 1992), and some of the best known and respected authors in the area, like Laudan, also happen to be first-rate classroom teachers of the material, too. I would suggest that one or more of them should be invited to provide intensive courses designed especially for our field, perhaps at summer institutes, so that not only graduate students, but also the many people currently training future SLA researchers could attend.

Whether or not these ideas would alone solve any of the problems discussed, I look forward to the day when our field is more widely recognized as the serious and socially responsive discipline I believe it is already on the way to becoming. Papers like this one (unpleasant for writer and assuredly some readers alike) would no longer be needed. One could instead concentrate on the genuine controversies and excitement in SLA: the roles of nature and nurture; special and general nativism; child/adult differences and the possibility of maturational constraints; cross-linguistic influence; acquisition and socialization; cognitive and social factors; resilience; stabilization, fossilization, and other putative mechanisms and processes in IL change; the feasability of pedagogical intervention; and most of all, the development of viable theories.

REFERENCES

Albert, M. (1996). Science, post-modernism, and the left. *Z Magazine* July/August, 1996, 64-69.

Albert, M., & Obler, L. (Eds.) (1978). *The bilingual brain: Neuropshychological and neurolinguistic aspects of bilingualism.* New York: Academic Press.

Barsky, R. (1997). *Noam Chomsky: A life of dissent.* Cambridge, MA: MIT Press.

Bauman, Z. (1992). *Intimations of postmodernity.* London: Routledge.

Berent, G. P. (1996). The acquisition of English syntax by deaf learners. In W. Ritchie & T. K. Bhatia (Eds.), *Handbook of second language acquisition* (pp. 469-507). San Diego: Academic Press.

Bley-Vroman, R. (1989). What is the logical problem of foreign language learning? In S. M. Gass & J. Schachter (Eds.), *Linguistic perspectives on second language acquisition* (pp. 41-68). Cambridge: Cambridge University Press.

Block, D. (1996). Not so fast: Some thoughts on theory culling, relativism, accepted findings and the heart and soul of SLA. *Applied Linguistics, 17*(1), 63-83.

Block, D. (To appear). Playing the reply-rebuttal game. *Applied Linguistics, 19.*

Bohannon, J. N. M., MacWhinney, B., & Snow, C. (1990). No negative evidence revisited: Beyond learnability or who has to prove what to whom. *Developmental Psychology, 26,* 221-226.

Brown, H. D. (1994). *Teaching by principles: An interactive approach to language pedagogy.* Upper Saddle River, NJ: Prentice Hall Regents.

Brumfit, C. (1997). Theoretical practice: Applied linguistics as pure and practical science. In A. Mauranen & K. Sajavaara (Eds.), *Applied linguistics across disciplines. AILA Review, 12,* 18-30.

California State Department of Education. (1984). *Studies on immersion education: A collection for United States educators.* Sacramento, CA: California State Department of Education.

Clyne, M. (1977). Multilingualism and pidginization in Australian industry. *Ethnic Studies, 1,* 40-55.

Collins, R. (1989). Towards a theory of intellectual change: The social causes of philosophies. *Science, Technology, and Human Values, 14*(2), 107-140.

Corvalis, G. (1997). *The philosophy of science: Science and objectivity.* Thousand Oaks, CA: Sage.

Crookes, G. (1997a). What influences how and what second language teachers teach. *Modern Language Journal, 81*(1), 67-79.

Crookes, G. (1997b). SLA and teachers: A socio-educational perspective. *Studies in Second Language Acquisition, 19*(1), 93-116.

Cummins, J. (1984). *Bilingualism and special education: Issues in assessment and pedagogy*. Clevedon, Avon: Multilingual Matters.

Curtiss, S. (1980). The critical period and feral children. *UCLA Working Papers in Cognitive Linguistics, 2*, 21-36.

Curtiss, S. (1988). Abnormal language acquisition and the modularity of language. In F. J. Newmeyer (Ed.), *Linguistics: The Cambridge survey. Volume II. Linguistic theory: Extensions and implications* (pp. 96-116). Cambridge: Cambridge University Press.

Davies, A. (1991). *The native speaker in applied linguistics*. Edinburgh: Edinburgh University Press.

Diesing, P. (1991). *How does social science work? Reflections on practice*. Pittsburgh, PA: University of Pittsburgh Press.

Doughty, C. (1991). Second language instruction does make a difference: Evidence from an empirical study of SL relativization. *Studies in Second Language Acquisition, 13*(4), 431-469.

Doughty, C., & Long, M. H. (To appear). Eliciting L2 speech data. In L. Menn & N. Ratner (Eds.), *Methods for studying language production*. Mahwah, NJ: Lawrence Erlbaum.

Doughty, C., & Varela, E. (1998). Communicative focus on form. In C. Doughty & J. Williams (Eds.), *Focus on form in classroom second language acquisition* (pp. 114-138). Cambridge: Cambridge University Press.

Doughty, C., & Williams, J. (Eds.) (1998). *Focus on form in classroom second language acquisition*. Cambridge: Cambridge University Press.

Eckman, F.R. (1994). The competence—performance issue in second language acquisition theory: A debate. In E. E. Tarone, S. M. Gass, & A. D. Cohen (Eds.), *Research methodology in second language acquisition* (pp. 3-15). Hillsdale, NJ: Lawrence Erlbaum.

Eckman, F., Bell, L., & Nelson, D. (1988). On the generalization of relative clause instruction in the acquisition of English as a second language. *Applied Linguistics, 9*(1), 1-11.

Ellis, N. C. (1995). Consciousness in second language acquisition; A review of field studies and laboratory experiments. *Language Awareness, 4*(3), 123-146.

Ellis, N. C., & Laporte, L. (1995). Contexts of acquisition: Effects of formal instruction and naturalistic exposure on second language acquisition. In A. M. B. de Groot & J. F. Kroll (Eds.), *Tutorials in bilingualism: Psycholinguistic perspectives* (pp. 53-83). Mahwah, NJ: Lawrence Erlbaum.

Ellis, R. (1989). Are classroom and naturalistic acquisition the same? A study of the classroom acquisition of German word order rules. *Studies in Second Language Acquisition, 11*(3), 305-328.

Ellis, R. (1997a). *SLA research and language teaching.* Oxford: Oxford University Press.

Ellis, R. (1997b). SLA and language pedagogy: An educational perspective. *Studies in Second Language Acquisition, 19*(1), 69-92.

Firth, A. (1996). The discursive accomplishment of 'normality': On lingua franca English and Conversational Analysis. *Journal of Pragmatics, 26,* 237-259.

Firth, A., & Wagner, J. (1997). On discourse, communication, and (some) fundamental concepts in SLA research. *Modern Language Journal, 81*(3), 285-300.

Firth, A., & Wagner, J. (1998). SLA property: No trespassing! *Modern Language Journal, 82*(1), 91-94.

Fraser Gupta, A. (1994). *The step tongue: Children's English in Singapore.* Philadelphia: Multilingual Matters.

Galloway, L. (1981). The convolutions of second language: A theoretical article with a critical review and some new hypotheses towards a neuropsychological model of bilingualism and language performance. *Language Learning, 31*(2), 439-464.

Gass, S. M. (1997). *Input, interaction, and the second language learner.* Mahwah, NJ: Lawrence Erlbaum.

Gass, S. (1998). Apples and oranges; Or, why apples are not orange and don't need to be. A response to Firth and Wagner. *Modern Language Journal, 82*(1), 82-90.

Gregg, K. R. (1993). Taking explanation seriously; or, let a couple of flowers bloom. *Applied Linguistics, 14*(3), 276-294.

Gregg, K. (1997). A honeymoon in the hand; or, Every man his own theorist: A comment on Lantolf (1996). Unpublished ms. Osaka, Japan: Department of English, Momoyama Gakuin University.

Gregg, K. R., Long, M. H., Jordan, G., & Beretta, A. (1997). Rationality and its discontents in SLA. *Applied Linguistics, 18*(4), 539-559.

Grimshaw, J., & Pinker, S. (1989). Positive and negative evidence in language acquisition. *Behavioral and Brain Sciences, 12,* 341-342.

Guba, E. (1990). The alternative paradigm dialog. In E. Guba (Ed.), *The paradigm dialog.* Newbury Park, CA: Sage.

Hales, M. (1986). *Science or society: The politics of the work of scientists*. London: Free Association Books.

Hamilton, R. L. (1994). Is implicational generalization unidirectional and maximal? Evidence from relativization instruction in a second language. *Language Learning, 44*(1), 123-157.

Hatch, E. M. (1978). Discourse analysis and second language acquisition. In E. M. Hatch (Ed.), *Second language acquisition: A book of readings* (pp. 402-435). Rowley, MA: Newbury House.

Howatt, A. P. R. (1984). *A history of English language teaching*. Oxford: Oxford University Press.

Huebner, T. (1983). *A longitudinal analysis of the acquisition of English*. Ann Arbor, MI: Karoma.

Huebner, T. (1985). System and variability in interlanguage syntax. *Language Learning, 35*(2), 141-163.

Hulstijn, J. (1992). Retention of inferred and given word meanings: Experiments in incidental vocabulary learning. In P. Arnaud & H. Bejoint (Eds.), *Vocabulary and applied linguistics* (pp. 113-125). London: Macmillan.

Hulstijn, J., & Hulstijn, W.(1984). Grammatical errors as a function of processing constraints and explicit knowledge. *Language Learning, 34*(1), 23-43.

Igor, B. (1985). *And yet it moves: The realization and suppression of science and technology*. New York: Zamisdat Press.

Jacobs, B. (1988). Neurobiological differentiation of primary and secondary language acquisition. *Studies in Second Language Acquisition, 10*(3), 303-337.

Jacobs, B., & Schumann, J. H. (1992). Language acquisition and the neurosciences: Towards a more integrative perspective. *Applied Linguistics, 13*(3), 282-301.

Johnston, M. (1985). *Syntactic and morphological progression in learner English*. Research Report. Canberra: Department of Immigration and Ethnic Affairs, Commonwealth of Australia.

Johnston, M. (1998). Development and variation in learner language. Unpublished Ph.D. dissertation. Canberra: Department of Linguistics, Australian National University.

Johnston, J., & Newport, E. (1989). Critical period effects in second language learning: The influence of maturational state on the acquisition of English as a second language. *Cognitive Psychology, 21*, 60-99.

Kasper, G. (1997). "A" stands for acquisition: A response to Firth and Wagner. *Modern Language Journal, 81*(3), 307-312.

Kelly, L. G. (1969). *Twenty-five centuries of language teaching*. Rowley, MA: Newbury House.

Kitcher, P. (1993). *The advancement of science. Science without legend, objectivity without illusions*. New York: Oxford University Press.

Kropotkin, P. (1899a). *Fields, factories and workshops tomorrow*. Edited edition by Colin Ward. London: Freedom Press, 1985.

Kropotkin, P. (1899b). *An appeal to the young*. In E. Capouya & K. Tomkins (Eds.), *The essential Kropotkin* (pp. 10-27). New York: Liveright, 1975.

Lantolf, J. (1996a). SLA theory building: "Letting all the flowers bloom!" *Language Learning, 46*(4), 713-749.

Lantolf, J. (1996b). Second language acquisition theory building? In G. M. Blue & R. Mitchell (Eds.), *Language and education: British Studies in Applied Linguistics* 11, 16-27. Clevedon, Avon: BAAL/Multilingual Matters.

Laudan, L. (1990). *Science and relativism: Some key controversies in the philosophy of science*. Chicago: University of Chicago Press.

Laudan, L. (1996). *Beyond positivism and relativism: Theory, method, and evidence*. Boulder, CO: Westview/Harper Collins.

Laudan, L., & Laudan, R. (1989). Dominance and the disunity of method: Solving the problems of innovation and consensus. *Philosophy of Science, 56*, 221-237.

Lechte, J. (1994). *Fifty key contemporary thinkers: From structuralism to postmodernity*. London: Routledge.

Lightbown, P. M. (1983). Exploring relationships between developmental and instructional sequences in L2 acquisition. In H. W. Seliger & M. H. Long (Eds.), *Classroom-oriented research in second language acquisition* (pp. 217-243). Rowley, MA: Newbury House.

Lightbown, P. M., & Spada, N. (1993). *How languages are learned*. Oxford: Oxford University Press.

Lincoln, Y. (1990). The making of a constructivist: A remembrance of transformations past. In E. Guba (Ed.), *The paradigm dialog*. Newbury Park, CA: Sage.

Long, M. H. (1983). Does second language instruction make a difference? A review of research. *TESOL Quarterly, 17*(3), 359-382.

Long, M. H. (1985). A role for instruction in second language acquisition: task-based language teaching. In K. Hyltenstam & M. Pienemann (Eds.), *Modelling and assessing second language development* (pp. 77-99). Clevedon, Avon: Multilingual Matters.

Long, M. H. (1988). Instructed interlanguage development. In L. Beebe (Ed.), *Issues in second language acquisition: Multiple perspectives* (pp. 115-141). Cambridge, MA: Newbury House.

Long, M. H. (1990). The least a second language acquisition theory needs to explain. *TESOL Quarterly, 24*(4), 649-666. Also in Brown, H. D., & Gonzo, S. (Eds.) (1994), *Readings on second language acquisition* (pp. 470-490). Englewood-Cliffs, NJ: Prentice Hall Regents.

Long, M. H. (1993). Assessment strategies for SLA theories. *Applied Linguistics, 14*(3), 225-249.

Long, M. H. (1994). On the advocacy of the task-based syllabus. *TESOL Quarterly, 28*(4), 782-790.

Long, M. H. (1996a). The role of the linguistic environment in second language acquisition. In W. C. Ritchie & T. J. Bhatia (Eds.), *Handbook of second language acquisition* (pp. 413-468). New York: Academic Press.

Long, M. H. (1996b). Authenticity and learning potential in L2 classroom discourse. *University of Hawai'i Working Papers in ESL, 14*(2), 127-149. Also in Jacobs, G. (Ed.) (1996). *Language classrooms of tomorrow: Issues and responses* (pp. 148-169). Singapore: SEAMEO Regional Language Centre.

Long, M. H. (1997a). Ebonics, language and power. *University of Hawai'i Working Papers in ESL, 15*(1), 97-122. Also in (1997). *Social Anarchism, 24*, 5-29. Also to appear in Ehrlich, H. J., & Pincas, F. (Eds.), *Race and ethnic conflict: Contending views on prejudice, discrimination, and ethnoviolence* (2nd ed.). Westview/Harper Collins.

Long, M. H. (1997b). Construct validity in SLA research: A response to Firth and Wagner. *Modern Language Journal, 81*(3), 318-323.

Long, M. H. (1997c). Focus on form and TBLT. Paper presented at the fourth annual McGraw-Hill Teleconference on Foreign Language Teaching, California State University, Long Beach, October 29, 1997. Also in (1998). *University of Hawai'i Working Papers in ESL, 16*(2), 35-49.

Long, M. H. (To appear). *Task-based language teaching*. Oxford: Blackwell.

Long, M. H., Adams, L., McLean, L., & Castanos, F. (1976). Doing things with words: verbal interaction in lockstep and small group classroom situations. In J. Fanselow & R. Crymes (Eds.), *On TESOL '76* (pp. 137-153). Washington, DC: TESOL.

Long, M. H., & Crookes, G. (1992). Three approaches to task-based language teaching. *TESOL Quarterly, 26*(1), 27-56.

Long, M. H., & Crookes, G. (1993). The authors respond ... *TESOL Quarterly, 27*(4), 729-733.

Long, M. H., & Robinson, P. (1998). Focus on form; Theory, research and practice. In C. Doughty & J. Williams (Eds.), *Focus on form in classroom SLA* (pp. 15-41). Cambridge: Cambridge University Press.

Long, M. H., & Ross, S. (1993). Modifications that preserve language and meaning. In M. Tickoo (Ed.), *Simplification: Theory, research and practice* (pp. 29-52). Singapore: Regional Language Centre/Singapore University Press.

Lyotard, J. F. (1984). *The postmodern condition: A report on knowledge.* Manchester: Manchester University Press.

Lyotard, J. F. (1992). *The postmodern explained to children: Correspondence 1982-1984.* London: Turnaround.

Mackey, A. (1995). Stepping up the pace; Input, interaction and interlanguage development—An empirical investigation of questions in ESL. Unpublished Ph.D. dissertation. Department of Linguistics, University of Sydney.

Mahoney, G. (1975). Ethnological approach to delayed language acquisition. *American Journal of Mental Deficiency, 80*, 139-148.

Malcolm, I. G. (1995). *Language and communication enhancement for two-way education.* Report. Mount Lawley, WA: Edith Cowan University.

Malcolm, I. G., & Koscielecki, M. M. (1997). *Aboriginality and English.* Report to the Australian Research Council. Mount Lawley, WA: Centre for Applied Language Research, Edith Cowan University.

Manheimer, R. (1993). Close the task, improve the discourse. *Estudios de Linguistica Aplicada, 17*, 18-40.

Martin, B. (1991). *Strip the experts.* London: Freedom Press.

Meisel, J. M., Clahsen, H., & Pienemann, M. (1981). On determining developmental stages in natural second language acquisition. *Studies in Second Language Acquisition, 3*(1), 109-135.

Musumechi, D. (1997). *An exploration of the historical relationship between theory and practice in second language teaching.* New York: McGraw-Hill.

Newport, E. (1990). Maturational constraints on language learning. *Cognitive Science, 14,* 11-28.

Newton, J., & Kennedy, G. (1996). Effects of communication tasks on the grammatical relations marked by second language learners. *System, 24*(3), 309-322.

Nunan, D. (1991). Communicative tasks and the language curriculum. *TESOL Quarterly, 25*(2), 279-295.

Obler, L. (1981). Right hemisphere participation in second language acquisition. In Diller, K. (Ed.), *Individual differences and universals in language learning* (pp. 53-64). Rowley, MA: Newbury House.

O'Grady, W. (1996). Language acquisition without Universal Grammar: A general nativist proposal for L2 learning. *Second Language Research, 12*(4), 374-397.

Oliver, R. (1995). Negative feedback in child NS/NNS conversation. *Studies in Second Language Acquisition, 17*, 459-483.

Ortega, L. (In press). Planning and focus on form in L2 oral performance. *Studies in Second Language Acquisition, 20*(4).

Ortega, L., & Long, M. H. (1997). The effects of models and recasts on the acquisition of object topicalization and adverb placement in L2 Spanish. *Spanish Applied Linguistics, 1*(1), 65-86.

Pennycook, A. (1995). English in the world/The world in English. In J. W. Tollefson (Ed.), *Power and inequality in language education* (pp. 34-58). Cambridge: Cambridge University Press.

Pennycook, A. (1996). Borrowing others' words: Text, ownership, memory, and plagiarism. *TESOL Quarterly, 30*(2), 201-230.

Phillips, M., & Shettlesworth, C. (1975). Questions in the design and use of courses in English for specialized purposes. Paper presented at the 4th International Congress of Applied Linguistics, Stuttgart, August.

Pienemann, M. (1989). Is language teachable? Psycholinguistic experiments and hypotheses. *Applied Linguistics, 10*(1), 52-79.

Pica, T. (1983). Adult acquisition of English as a second language under different conditions of exposure. *Language Learning, 33*(4), 465-497.

Pica, T., Lincoln-Porter, F., Paninos, D., & Linnell, J.(1996). Language learners' interaction: How does it address the input, output, and feedback needs of language learners? *TESOL Quarterly, 30*(1), 59-84.

Poulisse, N. (1997). Some words in defense of the psycholinguistic approach. *Modern Language Journal, 81*(3), 324-328

Purchase, G. (1996). *Evolution and revolution: An introduction to the life and thought of Peter Kropotkin*. Sydney: Jura Books.

Rahimpour, M. (1997). Task condition, task complexity and variation in oral L2 narrative discourse. Unpublished Ph.D. dissertation. Brisbane: CLTR, University of Queensland.

Rampton, B. (1987). Stylistic variability and not speaking 'normal' English: Some post-Labovian approaches and their implications for the study of interlanguage. In R. Ellis (Ed.), *Second language acquisition in context* (pp. 47-58). Englewood-Cliffs, NJ: Prentice-Hall.

Rampton, B. (1995). *Crossing: Language and ethnicity among adolescents*. London: Longman.

Rampton, B. (1997a). Second language research in late modernity. *Modern Language Journal, 81*(3), 329-333.

Rampton, B. (1997b). A sociolinguistic perspective on L2 communication strategies. In G. Kasper & E. Kellerman (Eds.), *Communication strategies: Psycholinguistic and sociolinguistic perspectives*. London: Longman.

Rampton, B. (1997c). Retuning in applied linguistics. *International Journal of Applied Linguistics, 7*(1), 3-25.

Redhead, M. (1995). *From physics to metaphysics*. Cambridge: Cambridge University Press.

Riggs, P. J. (1992). *Whys and ways of science: Introducing philosophical and sociological theories of science*. Melbourne: Melbourne University Press.

Robinson, P. (1997). State of the art: SLA research and second language teaching. *The Language Teacher* (Japan), *21*(7), 7-16.

Robinson, P. (1998). State of the art: SLA theory and second language syllabus design. *The Language Teacher, 22*(4), 7-14.

Robinson, P., Ting, S. C-C., & Erwin, J. J. (1995). Investigating second language task complexity. *RELC Journal, 26*(2), 62-79.

Rosenau, P. M. (1992). *Post-modernism and the social sciences. Insights, inroads, and intrusions*. Princeton, NJ: Princeton University Press.

Sachs, J., Bard, B., & Johnson, M. (1981). Language learning with restricted input: case studies of two hearing children of deaf parents. *Applied Psycholinguistics, 2*(1), 33-54.

Sato, C. J. (1989). A nonstandard approach to Standard English. *TESOL Quarterly, 23*(2), 259-282.

Sato, C. J. (1990). *The syntax of conversation in interlanguage development*. Tubingen: Gunter Narr.

Schmidt, R. W. (1983). Interaction, acculturation, and the acquisition of communicative competence: A case study of an adult. In N. Wolfson & E. Judd (Eds.), *Sociolinguistics and second language acquisition* (pp. 137-174). Rowley, MA: Newbury House.

Schmidt, R. W. (1995). Consciousness and foreign language learning: A tutorial on the role of attention and awareness in learning. In R. W. Schmidt (Ed.), *Attention and awareness in foreign language learning* (pp. 1-63). *Technical Report No. 9*. Honolulu: Second Language Teaching and Curriculum Center, University of Hawai'i at Manoa.

Schumann, J. H. (1983). Art and science in second language acquisition research. In M. Clarke & J. Handscombe (Eds.), *On TESOL `82* (pp. 107-124). Also in *Language Learning* 33, Special Issue No. 1, 49-76.

Schumann, J. H. (1998). *The neurobiology of affect in language. Language Learning, 48*, Supplement 1.

Schwartz, B. D. (1993). On explicit and negative data effecting and affecting competence and linguistic behavior. *Studies in Second Language Acquisition, 15*, 147-163.

Schwartz, B. D., & Gubala-Ryzak, B. (1992). Learnability and grammar reorganization in L2A: Against negative evidence causing the unlearning of verb movement. *Second Language Research, 8*, 1-38.

Selinker, L. (1969). Language transfer. *General Linguistics, 9*, 67-92.

Sheen, R. (1992). In defense of grammar translation. *The Language Teacher* (Japan),*16*, 43-45.

Sheen, R. (1994). A critical analysis of the advocacy of the task-based syllabus. *TESOL Quarterly, 28*(1), 127-151.

Sheen, R. (1998). The case for an eclectic approach. Paper presented at the annual meeting of the American Association for Applied Linguistics, Seattle, March 14-17.

Siegel, J. (Ed.) (1992). *Pidgins, creoles and nonstandard dialects in education* (Occasional Paper No. 12). Melbourne: Applied Linguistics Association of Australia.

Siegel, J. (1996). Using a pidgin language in formal education: Help or hindrance? *Applied Linguistics, 18*(1), 86-100.

Skutnabb-Kangas, T., & Cummins, J. (Eds.) (1988). *Minority education: From shame to struggle*. Clevedon: Multilingual Matters.

Sokal, A. (1996). Transgressing the boundaries: Towards a transformative hermeneutics of quantum gravity. *Social Text 46/47, 14*(1 & 2), 217-252.

Strong, M. (Ed.) (1988). *Language learning and deafness*. Cambridge: Cambridge University Press.

Tarone, E. (1997). A sociolinguistic perspective on an SLA theory of mind. Paper presented at the 17th annual Second Language Research Forum, East Lansing: Michigan State University, October 17-19.

Tarone, E., & Liu, G-Q. (1995). Situational context, variation, and second language acquisition theory. In G. Cook & B. Seidelhofer (Eds.), *Principle & practice in applied linguistics* (pp. 107-124). Oxford: Oxford University Press.

Terrell, T. (1990). Natural vs. classroom input: Advantages and disadvantages for beginning language students. In J. E. Alatis (Ed.), *GURT 1990* (pp. 193-206). Washington, DC: Georgetown University Press.

Usher, R., & Edwards, R. (1994). *Postmodernism and education*. London: Routledge.

Van Lier, L. (1994). Forks and hope: Pursuing understanding in different ways. *Applied Linguistics, 15*, 328-346.

Wagner, J. (1996). Foreign language acquisition through interaction: A critical review of research on conversational adjustments. *Journal of Pragmatics, 23*(8), 215-235.

Warner, S. L. (1996). I ole ka `olelo i na keiki: Ka `apo ia ana o ka `olelo Hawai'i e na keiki ma ke kula kaiapuna. [A language survives through its children: The acquisition of Hawaiian by children in an immersion school. Unpublished Ph.D. dissertation. Honolulu, Department of Educational Psychology, University of Hawai'i at Manoa.

Watson-Gegeo, K. A. (1992). Thick explanation in the ethnographic study of child socialization: A longitudinal study of the problem of schooling for Kwara`ae (Solomon Islands) children. In W. Corsaro & P. J. Miller (Eds.), *Interpretative approaches to children's socialization* (pp. 51-66). San Francisco: Jossey-Bass.

Watson-Gegeo, K. A., & Gegeo, D. W. (1995). Understanding language and power in the Soloman Islands: Methodological lessons for educational intervention. In J. Tollefson (Ed.), *Power and inequality in language education* (pp. 59-72). Cambridge: Cambridge University Press.

Wertsch, J. (1979). From social interaction to higher psychological processes: A clarification and application of Vygotsky's theory. *Human Development, 22*, 1-22.

White, L. (1991). Adverb placement in second language acquisition: some effects of positive and negative evidence in the classroom. *Second Language Research, 7*, 133-161.

Wilkins, D. (1994). Language, language acquisition and syllabus design: Some recent issues. *English Teaching* (Korea), *49*, 41-56.

Williams, J. (1988). Zero anaphora in second language acquisition: A comparison among three varieties of English. *Studies in Second Language Acquisition, 10*(3), 339-370.

Willis, D. (1993). Comments on Michael H. Long and Graham Crookes' "Three approaches to task-based syllabus design." A reader reacts ... *TESOL Quarterly, 27*(4), 726-729.

Wolfe-Quintero, K. (1996). Nativism does not equal Universal Grammar. *Second Language Research, 12*(4), 335-373.

Yano, Y., Long, M. H., & Ross, S. (1994). The effects of simplified and elaborated texts on foreign language reading comprehension. *Language Learning, 44*(2), 189-219.

Michael H. Long

Department of English as a Second Language

University of Hawai'i

1890 East-West Road, Honolulu, HI 96822

mlong@hawaii.edu