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AHISTORICITY REVISITED

Does SLA Have a History?

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In our reply to Margaret Thomas's article "Programmatic ahistoricity in second language acquisition theory," we first review pertinent literature, concluding that historical awareness is evident in SLA, though it is not as far-reaching as Thomas would like it to be. We then argue that the attitude of most scholars in SLA toward the past is reasonable given that no significant work in SLA from antiquity has been discovered—by Thomas or anyone else—and that if such work exists Thomas has the burden to bring it to light before declaring the field guilty of ahistoricity. We consider various ways to define the field of SLA, arguing that it should be defined theoretically first, and historically second. We claim that the point at which SLA separated itself from language teaching is a logical point from which to date the beginnings of SLA as a true discipline. We consider and reject Thomas's comparison of SLA and its history to various other scientific disciplines and their histories, arguing that these disciplines have true milestones to point to in the distant past, whereas SLA does not. Although we agree with Thomas that a general awareness of the history of philosophy and science is beneficial for scholars in all fields, we make a sharp division between that history and the history of SLA proper. We conclude by arguing that respect for the field of SLA can come only through sound scientific progress, not by appeals to history.

Margaret Thomas, in her article "Programmatic ahistoricity in second language acquisition theory," does the field of second language acquisition a service by raising the consciousness of the field about the past.[1] The article is, in

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part, an appeal for the field to see itself in a broader context, a goal which we laud and with which we are in complete agreement (cf. Gass, 1993; Schachter, 1993). We also recognize that the issue she raises—one that has been largely ignored throughout the history (whatever that may be) of the field—is important. But we disagree with Thomas's portrayal of the facts of the field and with the putative impact that the past may or may not have or could have had on current research programs.[2]

Thomas (1995) explicitly discusses the relationship between pre-twentieth century research and SLA stating, "It is not my purpose to try to show that modern ideas of UG or of L2 acquisition do or do not derive from those of the Middle Ages . . . " (p. 345). What she merely wants to show is that SLA has a past and that the recognition of that past should be reflected in how the field views itself and, as a consequence, how the field is viewed by others. The literature she points to in her present paper as well as in others (Thomas, 1995, 1996) is interesting and valuable in its own right and, as an object of inquiry, requires little justification. We are therefore puzzled by the polemical stance Thomas takes in this article. Thomas argues that SLA researchers and the field as a whole have been myopic with regard to SLA's history and that this myopia is damaging to the field. Although it may be the case that many SLA researchers are not familiar with the historical literature that Thomas presents, we do not agree that knowledge of that history is essential to the field as a whole nor that such knowledge (by virtue of presenting the field in a broader historical context) will further the progress of the field. In short, we think the history Thomas presents has its own inherent value and need not be justified by utilitarian concerns (i.e., by arguments that ignorance of this history is potentially damaging to SLA as a field). In fact, we believe that such arguments detract from Thomas's presentation of L2 acquisition history by drawing attention away from the historical record into empty arguments about the usefulness of that history and by forcing her to construe that history in a way that can be deemed "useful." In other words, we see little reason to frame the discussion in the context of historical forgetfulness. Thomas does not explicitly define what she means by history. If "history" refers to everything that has happened in the past, then her comments regarding SLA as a discipline are true but not particularly interesting. If, on the other hand, "history" refers to intellectual enterprise of the past that bears on current thinking, we are in disagreement with her. This is our point of departure.

In our response to Thomas, we focus on four principal areas: (a) the validity of Thomas's claims about SLA's shortsighted view of itself; (b) the relationship of the past to current research agenda in SLA; (c) what is SLA? and (d) the appropriateness of comparing the relationship between SLA and linguistics to its past with the relationship of other new sciences to their pasts.

DO WE INDEED IGNORE OUR PAST?

At a recent panel discussion at a large second language acquisition conference,[3] two of the four discussants (all well-known and well-respected senior

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scholars in the field) remarked about the youthfulness of the field of SLA, pointing to its origins of only about 25–30 years ago. That we have a collective sense of the field as it is currently practiced, originating in the 1960s or 1970s, is probably a fair assessment and that this sense may, "with every unexamined iteration" (Thomas, p. 390) in fact, be "inscribed in informal doctrine" (p. 390) is also a reasonable conclusion. This does not imply, however, that we are unaware of our past or that we do not appreciate the intellectual thought of earlier times. It only implies that the past does not impact the issues of today and that the discipline, as it is currently defined, does not have a systematic and relevant body of knowledge prior to a particular point in time. Thus, we deny Thomas's charge that the field of SLA exists in a state of "programmatic ignorance" (in the sense of Amsler, 1993, p. 51, "what a discipline or a knower refuses to know," cited in Thomas, this volume, p. 390), on the grounds that there is little evidence that what we do know of the past has in any way influenced our present or, conversely, that lack of knowing has clouded current perceptions and hindered investigations. Thomas herself agrees that there probably are not any "specific data, hypotheses, tools, or results that could be readily incorporated to solve current problems facing the field" (p. 396), yet despite this recognition, Thomas continues to argue through reference to work of Kuhn (1970), Percival (1976), and Elffers-van Ketel (1991) that we have lapsed into programmatic ahistoricity and that this is detrimental to our image.

Thomas cites a number of authors (one of the present authors included) to justify her claim of (perhaps benign) neglect of research prior to 25–30 years ago. But there are numerous sources that could have been used to show just the opposite, some of which are included in the works she cites. For example, Selinker, who has long championed the need to return to our roots, states explicitly (1992):

> Rethinking several older literatures, this work shows that in some important sense for a very long time hints of the notion interlanguage have been in many of the writings about learning a second language, albeit buried and unnamed. The attempt painstakingly to gain insights from the wisdom of older literature for today's questions leads to a rethinking of crucial earlier texts, or *founding texts*. Our ultimate goal in explicitly contemplating our founding texts would be to seek general laws of second language acquisition. . . . Historical depth related to current questions is usually unavailable in the literature. One would like to know why our field is not more conscious of where its history fits into current work.
>
> In this volume we re-examine founding texts with the aim of reframing the IL hypothesis and tie together ideas over time with current IL thinking. Here we take the historian La Capra's quote . . . quite seriously, that 'A field is in constant dialogue with its founding texts.' (pp. 1–2) [Emphasis in original]

There are two points to make: First, Selinker's work shows that we do have a sense of SLA history that goes back more than the 25–30 years that Thomas

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acknowledges, although we admit that he does not go as far back as Thomas would like us to go. Second, Selinker's work is an example of a scholar who, in attempting to trace one of the early concepts of SLA—interlanguage—back to its roots, found pertinent notions only in this century. It is significant that there are scholars who have taken the past seriously and, more importantly, who have recognized the contribution of the past to the shaping of current knowledge. In a similar vein, Gass (1993) refers to the nineteenth century, in particular to the work of Baudouin de Courtenay (1870/1972), stating, "I will look at the history of applied linguistics as a way of understanding the position of SLA today" (p. 101).

A second point made by Thomas is that many authors (again, one of the present authors included), even when acknowledging the past, dismiss it as unimportant. Although it may be the case that these authors do not elaborate the past, it must be recognized that the works Thomas points to (introductory texts in L2 acquisition) have as their purpose an exposition of present day L2 acquisition studies and not a description of past research unless, in their view, it has direct bearing on today's research agenda.[4]

Thomas points to what she calls "concessive" language that acknowledges that the past exists (e.g., Gass & Selinker, 1994; Larsen-Freeman & Long, 1991), arguing that regretfully the authors do little more than pay a brief tribute to its existence. We cannot speak for Larsen-Freeman and Long, but in the case of Gass and Selinker, even though they do cite references that go back more than 50 years, they noted that the research questions and areas that reflect current work in SLA (which was the topic of the book) are not informed by work of very early times.

In sum, we argue that the field of SLA has and does recognize its past in the sense that there have always been people who have been curious about SLA,[5] and there have always been L2 learners to stimulate that curiosity. However, where we part company with Thomas concerns the significance of that past and the extent to which it has or could have impacted modern SLA research. This, we argue, is so because there is little evidence of an organized discipline prior to modern times, nor are we aware of significant and influential findings prior to the SLA research of this century. In short, we maintain that the burden of proving a significant history, if such exists, rests on Thomas and that she has not met that burden in her paper.[6]

THE RELATIONSHIP OF PAST TO PRESENT

Thomas explicitly states that the field views itself as having "an early boundary" somewhere in the 1970s. As we have argued above, this is not exactly the case. Furthermore, the issue of boundaries includes more than just historical notions. As Thomas also recognizes, disciplinary boundaries must be determined along theoretical lines, not just historical lines. Moreover, there is a complex relationship between historical and theoretical boundaries. We would argue that it is necessary to determine the theoretical boundaries of a

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field before tracing its historical roots. We assume that a discipline can be defined as such when there begins to be a coherent set of facts that scholars attempt to explain and verify in accordance with accepted methods[7] of science.[8] We also assume that partial evidence for the existence of a discipline (as is the case when any class of objects is defined) is having criteria to determine what is part of that discipline and what is not. As Foucault (1972) states with regard to disciplinary boundaries:

> disciplines are defined by groups of objects, methods, their corpus of propositions considered to be true, the interplay of rules and definitions, of techniques and tools: all these constitute a sort of anonymous system, freely available to whoever wishes, or whoever is able to make use of them, without there being any question of their meaning or their validity being derived from whoever happened to invent them. . . . For a discipline to exist, there must the possibility of formulating—and of doing so ad infinitum—fresh propositions. (pp. 222–223)

Although it is certainly the case that many over the centuries have given thought to[9] how people learn second languages,[10] we are unaware of methodical work in this area prior to this century.[11] Earlier thinking by and large concentrated on disparate and isolated facts without there being either a common thread that developed over time or a cohesive and continual progression of ideas. For a discipline's history to have relevance (and, by extension, for a discipline to be guilty of ignoring its past), we argue, that history must have bearing on later thought. With SLA, although we acknowledge that there was interesting speculation that considered inter alia what it means to know a second language, we are not convinced of the significance of that speculation for today's practices or for the development of the early stages of the discipline of SLA.

If we consider present-day SLA as being concerned with how languages are learned as separate from how languages are taught,[12] then it seems fair and reasonable to trace earlier stages back to Corder's important 1967 paper "The Significance of Learners' Errors" (see also Selinker, 1992) in which learners' errors were seen as providing information about learner systems. We consider this a landmark paper (see also Sharwood Smith, 1994) because it delineated the field of SLA as something separate from language teaching and as an area of scholarly inquiry in its own right, thereby allowing for the development of the concept of, for example, interlanguage[13] (Selinker, 1972) or approximative systems (Nemser, 1971). Not to consider earlier influences on Corder (starting perhaps with Fries, 1945) and continuing through Lado (1957)[14] and some early works in Contrastive Analysis and child language acquisition[15] would be missing a large part of the influencing history of how SLA came to be. Not to include work in behaviorism (e.g., Bloomfield, 1933), which directly influenced Fries and Lado, would also be missing the influencing history. These could all be considered part of our "founding texts" (cf. Selinker, 1992). This is what we argue are our "traceable roots."

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To restate our position, we do not wish to deny premodern texts, only to say that they do not seem to have played an informing role in the development of the field of SLA. It is only in the '50s, '60s, and '70s that we begin to see a flurry of intellectual activity that converges on a coherent body of scholarly work—a body of work that begins to ask the important how and why questions of second language learning to which widely accepted methods of analysis are applied.

The argument that there needs to be a recognition of an SLA history must be substantiated or at least bolstered by evidence that such history has influenced SLA and will continue to do so in the future. Further, we argue that it is important to separate the general past, particularly the history of science, from the specific past of SLA. We will argue below that although an awareness of such a past, particularly the history of science, is helpful to current SLA theorists, such an awareness does not constitute an agreement with the claims that Thomas is making about an SLA past.

In the preceding section, we pointed out examples in which authors argued for the relevance of the past in understanding the present—this is clearly Selinker's point in quoting La Capra's remarks on history and Wittgenstein's "I should not like my writing to spare other people the trouble of thinking. But, if possible, to stimulate someone to thoughts of his own" (cited in Selinker, 1992). Crucially, however, these are general examples, not specific SLA examples. With regard to SLA, the specific relevance of the past must be explained; the founding texts must be identified. Unfortunately, Thomas's article fails to do that.

Thomas claims that "the discipline confidently assumes that no relevant past exists (p. 390), yet we are still left not knowing how our research questions or research programs might have changed had we had a better understanding from whence we have come. Her argument would be well founded particularly if the lack of awareness, as she argues, prevented us from seeing what is relevant. The difference between ignorance and lack of relevance is, of course, subtle. But in order to make a convincing argument that the past needs to be explicitly acknowledged (when, how often, and by whom is not clear), one has to show how not doing so would lead us or would have led us to a different research paradigm, or if not to a different paradigm, at least in a different (perhaps more fruitful) direction.

Thomas, citing Elffers-van Ketel (1991), considers three possible ways that understanding our past may affect present thinking if not practices: (a) It may yield specific data, hypotheses, tools, or results (in Elffers-van Ketel's words, "knowledge of the history of science directly yields scientific insights which can be utilized in the present", p. 327); (b) it may inculcate a more tolerant scientific attitude (presented by Elffers-van Ketel as her third benefit); and (c) knowledge of the *history of science* may yield "an analytic instrument for the critical evaluation of present scientific ideas" (emphasis ours). Thomas dismisses (a) and (b) but accepts (c). Thomas here conflates the general past, particularly the history of science, with the SLA past. Elffers-van Ketel (1991),

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as cited by Thomas (p. 396), asks the general question, "to what extent can it be legitimately claimed that work in the historiography of science provides a 'wholesome effect' on science?" This is a different question than the specific one that Thomas asks—namely, to what extent can the history of SLA provide a wholesome effect on SLA? We agree with the argument that the history of science is beneficial (see also arguments in Beretta, 1991, 1993; Beretta & Crookes, 1993; Crookes, 1992; Gregg, 1989, 1993, 1996; Long, 1990, 1993; Schumann, 1993), but we do not accept Thomas's substitution of the history of SLA for the history of science ("Applied to the particular case at hand . . . " p. 397). The history of a particular field is not the same as the history of science in general.[16]

Thomas argues that "examination of the history of L2 acquisition theory may lead us to reassess its constituent terms, 'L2,' 'acquisition,' and 'theory' " (p. 397). Few would argue with this because most researchers are probably in favor of such reassessment, and in fact there is modern work in SLA attempting just that. For example, in the SLA works mentioned above (e.g., Beretta, 1991; Crookes, 1992; Gregg, 1993; Long, 1990, 1993; Schumann, 1993), there has been an attempt to define theory and establish methods of theory construction. However, the overwhelming majority of examples used by these authors are not drawn from SLA literature, past or modern, but, rather, drawn from diverse natural sciences, philosophy, and theoretical linguistics. The reasons for this are straightforward: there simply is not a preponderance of convincing examples from SLA, and it is necessary to step away from one's field to gain perspective on its problems and their possible solutions. Such perspective is what Elffers-van Ketel appears to be advocating. Here we are in agreement. However, it does not follow that the same benefits can be gained from digging into SLA's pre-scientific past. To the contrary, unless someone can show specific discoveries buried in our past, we see little reason for SLA researchers to dwell on this portion of the past as they focus on moving the field forward.

Returning to the three ways in which knowing the past may affect present thinking, Thomas, as we pointed out, rejects the first two and accepts the third: The past is in and of itself an analytical instrument with which we can evaluate our present and, in particular, come to an understanding of the "fundamental notions" (p. 397) of the field. If we interpret Thomas correctly, she is claiming that awareness of the past serves as a guiding force that constantly reminds us of where we have been and hence of where we are going. It may serve to remove blinders. In principle, this is reasonable and we do not disagree. However, we do disagree with the facts in the example Thomas uses to illustrate her point. She points to more recent treatments of contrastive analysis that, she claims, ignore the type of contrastive analysis done particularly in Eastern Europe. She is, of course, correct that the tradition in Eastern Europe is quite different from that in North America, Western Europe, and many other parts of the world. But she is incorrect to say that this has not been treated (see, for example, Gass, 1982, 1996). To the contrary, it has been found

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to be different enough as to have little bearing on current research practices and agenda.[17]

WHAT IS SLA?

Defining our boundaries is a first step in determining the relevance of the past for the present. That is, if we define SLA loosely to include language teaching, there is little argument with Thomas. However, if we define SLA in line with the current orthodoxy (i.e., to be concerned with the nature of L2 knowledge and how that knowledge comes to be), we need to consider what and how rigid the boundaries of the field are. Here, there is a wide range of possibilities and viewpoints, ranging from Gregg's notion in a recent article that "The domain of a L2 acquisition theory is not the behavior of speakers (linguistic performance), but rather the mental system (competence) underlying that behavior" (1996, p. 53) to some beliefs that L2 acquisition incorporates everything that has to do with learning and teaching second or foreign languages. Perhaps Gregg's statements are overly limiting and statements that include all of language teaching are not limited enough. The question then of the boundaries of SLA needs to be settled before we can determine which past is relevant to which present.

Thomas's notion of historical relevance often obscures her own distinction between narrow and broad definitions. She argues that if we have a narrow definition of the field of SLA, in particular, one which only refers to "surface features of modern study of L2 acquisition" (p. 393), then programmatic ahistoricity claims are vacuous. If, on the other hand, we have a broader definition of the field, then we are guilty of ignoring our past. But there is a logical fallacy in Thomas's application of these ideas to SLA. Thomas uses the comments of many in the field of second language acquisition to show that the field is indeed "ahistoric," or guilty of historic forgetfulness. But those "guilty" of such comments (by and large, the majority of scholars) make them with reference to a narrow definition of the field, in Thomas's sense. Thomas applies those comments to her broader definition and thereby declares that the field is ignorant of its past. We argue, along with many scholars in the field (see numerous statements in introductory texts), that Thomas's "surface features of modern study" are actually quite important; they reflect the field of SLA—one that had its beginnings at a time when the inquiry into second language acquisition changed from being "target-oriented" (i.e., concerned with language pedagogy) to focusing on acquisition as a tool with which to understand the nature of general learning principles on the one hand, and the nature of the human capacity for language on the other.

Thomas's "broader" definition of the field refers to "mutual engagement between ideas about language and ideas about language learning" (p. 392) and to the underpinnings of language teaching insofar as "in every act of language instruction there must be some guiding notion of what it means to learn a language" (p. 393) (see also our note 10). However, the fact is that the field of

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SLA (particularly that segment of SLA based on generative linguistics) is concerned with the relationship between second language acquisition and the nature of language (see Gass, 1979; Gass & Selinker, 1994), and, for many, pedagogical concerns are beyond the scope of inquiry.

Thomas claims that "framed in the diverse terms of diverse times, places, and purposes, it [earlier research] bears witness to generations of interest in, and speculation about, L2 learning" (pp. 394–395). But isn't this precisely what Larsen-Freeman and Long said when they noted that "People have been interested in second language acquisition since antiquity . . . " (1991, p. 5) and what Gass and Selinker said when they remarked that "scholars for centuries have been fascinated by the nature of questions posed by foreign language learning and language teaching" (1994, pp. xiii–xiv)?

That SLA is concerned with the relationship between learning and the nature of language is hardly in dispute and is clearly stated in numerous sources. For example, White (1989, p. 1) begins with the following: "In this book, potential relationships between linguistic theory and second language acquisition will be explored." Similarly, Ritchie and Bhatia (1996, p. 1) begin their edited volume on SLA with the following: "The phenomena of second (third, fourth, etc.) language acquisition . . . and use and the interpretation of these phenomena have come to occupy an important place in the development of our understanding of the human capacity for language."

Thomas makes the argument that a theory of how languages are learned was at the base of much pedagogical work—that is, that such theories represented the "underpinnings of language teaching." But even if one were to accept this point, there still remains the fact that the early grammarians do not often explicitly state the relationship that Thomas claims existed in their minds between language teaching and language learning. She argues that in today's research climate this early work is dismissed as having only pedagogical value and not having relevance to SLA. The issue, we believe, is not that the value of these works was only pedagogical because, clearly, work in pedagogy assumes an underlying relationship to learning and may therefore be insightful as far as understanding assumptions about learning. Rather, we claim that these works were primarily pedagogical in orientation and did not contain an explicitly stated or even coherent body of knowledge about second language learning.

More central to the points that Thomas makes is the concept of disciplinary status. She questions whether the oft-cited 25–30 year history refers to conceptual or institutional origins. We argue that it refers to both, with the latter being an overt recognition that there exists a body of facts, common questions, and commonly accepted methodologies to address those questions.

THE COMPARISON OF SLA TO OTHER NEWER DISCIPLINES

The comparison of SLA to ecology is made on the grounds that both disciplines are relatively new (both SLA and ecology gained independent institu-

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tional identities in the ’60s and ’70s); yet, ecology regularly acknowledges its debt to past scientists as far back as Aristotle, Mendel, and Darwin. But, there is a fundamental difference: Aristotle, Mendel, and Darwin all contributed substantial ideas to ecology that enabled the field to progress beyond its former state. Therefore, it is natural for ecologists to know this history because contained in it is the progression of ideas that led to modern understanding. Because SLA has no counterparts of this stature in the past, ignorance of the past does not entail ignorance of the progression of the field (defining the field as SLA, not linguistics). Thomas (1996) traces the roots of the field to Greeks whose philosophical beliefs led to the development of concepts of universals of language, to concerns with bilingual education by Romans, and to Christian life, in which L2 acquisition was essential. She then points to Bacon, who put forth the idea of three degrees of knowledge of a second or foreign language. Despite this, however, there seems to be no progression in the development of thought from Bacon (or earlier) to today’s world. The problem with this view of history is that pedagogical and acquisition concerns are often conflated (see note 13). All that is being shown is that the field did not come into existence from a vacuum. It arose out of initial concerns with more practical matters—that is, language teaching.

Furthermore, the above notions about language universals were not drawn from a study of SLA proper; rather, they were part of a much wider pursuit of philosophy. We therefore argue that they belong more to the history of science and philosophy than to SLA. We find these philosophical roots interesting and would encourage Thomas and other interested scholars to pursue them in detail. Moreover, we agree that knowledge of SLA’s connections to these roots is valuable inasmuch as it helps us understand how our field fits into a more general history of ideas. We again point out that this has been acknowledged by many. However, we maintain that it is reasonable to delimit the history of SLA proper to that period of history during which people have been consciously building SLA knowledge and theory. In other words, we argue that it is reasonable to divide the development of any science into two periods. The first consists of pre-scientific or pre-disciplinary speculation, speculation that exists without a check against reality, or, alternatively, observations about reality without drawing generalizable, testable, abstract conclusions from these observations. The second covers systematic scientific investigation, including such notions as problem stating, documentation, hypothesis testing, and falsifiability. This appears to be standard practice in science.

CONCLUSION

There is no question that Thomas has brought a degree of awareness of past research to the attention of the field and that this, in and of itself, is a positive and interesting contribution. However, as we have argued, framing this in terms of historical forgetfulness, or in her terms, ahistoricity, is neither neces-

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sary nor helpful. And, as we have also argued, the connection between earlier thought and today's field has not been made, either implicitly or explicitly. What is needed is a clear explication of how current thought developed, not merely a statement that others had given what Thomas views as "serious" thought to second language learning. Finally, we have argued that the concern with second language acquisition that may have existed sporadically in earlier times did not represent a coherent body of facts with questions that focused on the nature of acquisition, as opposed to the nature of teaching. In general, we feel, along with Elffers-van Ketel (1991), that we should not overestimate the relevance of the past for present research, nor do we feel that "having a past" is anything remotely promoting a panacea that will give us, as a discipline, more visibility (i.e., lack of isolation) or a greater "position in academic geography." Respect, we believe, comes as a consequence of value and relevance, and value and relevance in turn come from the intellectual and practical consequences of the discipline.

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NOTES

1. We begin by noting our admiration for Thomas's article. Were we to think it irrelevant or even peripheral to the field, we would not have taken the time to respond. It is in the hopes of getting others "to jump into the fray" and begin to understand the significance of the past for current research that we attempt this response. We hope that others will continue the line of systematic investigation that has been begun by this exchange.

2. Most likely, Margaret Thomas would think that we are hedging by limiting our comments with words such as "current" or "the discipline, as currently practiced." Clearly, we are. In fact, Thomas objected to Gass's 1993 use of similar language (not just a coincidence given that one of the authors here and Gass are one and the same person). Thomas objected, claiming that use of such language as "current theory" cannot, by definition, have referred to an earlier period. We believe, though, that it is important to delineate what one is talking about to avoid making overly general claims. We also believe that by making an overt statement limiting the scope of one's claims, one is acknowledging that there may be a time (i.e., the past) for which the same claims could not be made.

3. Thomas also refers to this panel discussion in her paper.

4. Introductory texts in chemistry and physics are similarly limited in their treatment of their pre-scientific history. Kotz and Treichel went back to Democritus and Aristotle in a section on "The Origins of Atomic Theory" but consider them philosophers who did not provide "quantitative explanations of physical and chemical facts" (1996, p. 59). Oxtoby and Nachtrieb (1996) started by comparing eighteenth-century chemists' tools with those of today. Nathan and Bobrow (1993), part of the Cliffs Quick Review Series, stated, "The modern science of chemistry began during the eighteenth century when several brilliant 'natural philosophers' classified the products of decomposition into a small number of fundamental substances" (p. 3). The situation with physics textbooks is similar. Fishbane, Gasiorowicz, and Thornton (1996) argued that, up until Galileo and Gilbert, too much reverence for the ideas of ancient Greeks impeded scientific progress. Krane (1996, p. 1) concurred that "Galileo is often regarded as the father of modern science."

The point is that a wide array of modern sciences make a clear distinction between ancient speculation and modern day science, and when the former is mentioned, it is only mentioned in passing, as a curiosity. It is not claimed to have bearing on today's thinking, nor is it claimed that not knowing the early works damages either the practitioners or the discipline as it is practiced today.

5. One could go back as far as the Bible and, in particular, to the story of the Ephraimites and their pronunciation of Shibboleth (cf. Gass & Selinker, 1994, p. 75).

> Jephthah then called together the men of Gilead and fought against Ephraim. The Gileadites struck them down because the Ephraimites had said, "You Gileadites are rene-

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> gades from Ephraim and Manasseh." The Gileadites captured the fords of the Jordan leading to Ephraim, and whenever a survivor of Ephraim said, "Let me cross over," the men of Gilead asked him, "Are you an Ephraimite?" If he replied, "No," they said, "All right, say 'Shibboleth'." If he said, "Sibboleth," because he could not pronounce the word correctly, they seized him and killed him at the fords of the Jordan. Forty-two thousand Ephraimites were killed at that time. (Judges 12:4–6)

Clearly, the story is a reflection of peoples' thinking about and awareness of the phenomenon of speaking or learning a second language.

6. Thomas (1996) does attempt to make this argument, yet, even there, although we are presented with interesting mention of scholars who through the centuries have been concerned with second language learning, we are unconvinced that these examples constitute anything that could be construed as a unified field.

7. There may be some disagreement about what are and what are not legitimate methods in any particular science (e.g., Selinker's statement about the need for analytical attention to be focused on "*the only observable data to which we can relate theoretical predictions*" (emphasis in original; 1972, p. 13) and more recent controversies regarding the role of acceptability judgments (e.g., Bard, Robertson, & Sorace, 1996; Ellis, 1990, 1991; Gass, 1994; Sorace, 1988), but we think there is wide agreement that, at a minimum, a science must employ some methods of problem identification, data analysis, hypothesis testing, and verification. Additionally, it must not contradict accepted premises of other sciences unless the new science is engaged in a debate about those premises (a case in point is the use of acceptability judgments in linguistics vs. acceptability judgments in SLA).

8. For example, even though many of the ideas of the ancient Greeks have been resurrected, accepted, or used in physics, most of their work is considered philosophy rather than a part of physics. Physics as a discipline is generally thought to have begun with such figures as Kepler and Galileo, who were the first to apply the scientific method of the field, namely, systematic measurements, drawing abstract conclusions from the measurements, and testing the conclusions against reality. Most Greek thinkers, however, failed to realize the importance of systematic, quantitative observation—that is, measurement— and thus failed to follow the steps of this scientific method. Many speculated without testing their ideas, whereas others observed but neglected to systematically verify their conclusions.

9. There are numerous instances in which individuals have thought about phenomena that later became part of a recognized science, but the work of these same individuals is not thought of as being part of that "science." One such example is aeronautical engineering. Over the years, many have given serious thought to flying (for example, consider the very sophisticated drawings by Leonardo da Vinci). But until the Wright brothers' early work, one would be hard pressed to say that there was a field or discipline or science of aeronautical engineering.

10. One piece of evidence that can be brought to bear on the fact that thought was given to acquisition is the obvious, but in our view trivial, point that underlying any work on language teaching must be some notion of how learning takes place. We view such thinking as "undeveloped" and only a by-product of concern with another area of inquiry and thus not part of an argument that would state that work on "acquisition" was taking place. Most scholars recognize that (a) the origins of current L2 learning research stem from concerns about language teaching and (b) underlying language teaching methodologies are implicit (or explicit) notions of language learning. Ritchie and Bhatia (1996, p. 5) state, for example, that "until the mid-1960s, organized concern for L2 phenomena was dominated by attempts to adduce implications for language teaching from the then-current behaviorist thinking in experimental psychology and American structuralist linguistics." But the important point is that SLA is today a field separate from language pedagogy. In a trivial sense only, then, the history of SLA is the history of language teaching. This work (i.e., the history of language teaching) has not been ignored (cf. Kelly, 1969). But this is not the history that Thomas points to.

11. Thomas herself (1995) mentions that thinking about SLA in the medieval period was dominated by the idea that Latin was "the most perfect shadow of the ideal (underlying) form" (p. 350, citing Salus, 1976, p. 78). Thus, "vernacular languages" (i.e., anything other than Latin) were considered to be unworthy of serious study. But, given that there was no possibility of pursuing native speakers' intuitions about Latin, there was little possibility of understanding what the "target" was, the structure of which learners were trying to discern. These and similar notions make medieval ideas about L2 quite different from current work and thus they can hardly be seen as precursors (i.e., founding history) to current work. What might have had greater impact, particularly on generative SLA, was the seventeenth-century work of the Port Royal Grammarians. However, we are aware of no work within this tradition that fell within the purview of SLA in any sense. Consequently, there is a gap between modern ideas of SLA and their nearest potential cousins in history.

12. Not all scholars make a clear distinction between the fields of second language learning and

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second language teaching. Although we recognize and agree with the mutual influence of one on the other, we delineate SLA as being concerned with the knowledge that second language learners have of their second language—where second language is intended broadly to subsume any nonnative language learning—and how that knowledge comes about. Importantly, we see this as separate from the field that is concerned with how to teach second languages. Although SLA as a discipline may have developed out of a concern for language pedagogy, SLA and pedagogy are independent fields with independent goals and methods (albeit with sometimes overlapping interests). Sharwood Smith (1994) provides an example of this relationship when he talks about the early stages of SLA (which he dates back to the 1960s), which considered "problems of foreign language learning in order to provide a scientific backing for teaching techniques and teaching materials (see Lado, 1967 [sic, should be 1957]). In other words, it was 'applied linguistics' in its most commonly accepted sense" (p. 22). Where second language research diverges from pedagogical research is, in part, in the orientation. Pedagogical research aims to determine ways to make learner language congruent (to the extent possible) with native-speaker knowledge. As Sharwood Smith notes (1994, p. 4), this research is "target-oriented." SLA research, as we know it today, has a very different goal: that of determining learner knowledge and how it comes to be. This orientation clearly differs from earlier traditions and it is this difference that we would argue marks the "true" beginning of our field; it is here that SLA "broke away" from its ancestral home. Although Thomas does not state precisely what she means by SLA, we assume that she is taking the "modern-day" definition of SLA. This is clearly implied in another paper (Thomas, 1996), where she states that "[Romans] reflected little on language learning" or "Roman educators never really faced the question of *what it means to learn an L2*" (p. 6, emphasis in original). In other words, there is a clear delineation between language teaching (the context of this discussion) and language learning, although at times we did feel that she wove in and out of the two, particularly when dealing with earlier intellectual thought.

13. This is not to say that Selinker's and others' early works were not influenced by others (for example, Weinreich, 1953; Haugen, 1953), for, as he acknowledges (Selinker, 1992), they were.

14. Lado, in fact, in his selected bibliography to his 1957 book refers to nothing earlier than Bloomfield with the exception of one Prague School paper written in 1931.

15. Pinker (1984) portrays the field of *modern-day* child language acquisition as having its origins in the late 1950s and early 1960s. "The modern study of language acquisition began about twenty-five years ago, and it is no coincidence that its birthday followed fairly closely upon the birth of what we now call cognitive science (p. 1) [Pinker's footnote; George Miller (1979) has proposed September 11, 1956, as the birthday of cognitive science.]" (p. 369). Pinker goes on to refer to such seminal works in the field of child language acquisition as those of Berko (1958), Braine (1963), and Brown (1958).

16. It is to be noted that Elffers-van Ketel (1991) does take the discussion into the realm of linguistics, in particular, into a discussion of the concept of grammar. Elffers-van Ketel points out the "inspirational" factor, referring to the novel ideas that we might get from reading past texts even when the relevance is not immediately or directly apparent. Although Elffers-van Ketel clearly states that "complete historical ignorance of linguistics" (p. 332) is not a favorable situation, she also recognizes the limitations of historical knowledge. "In general I believe that linguists should not overestimate the general relevance for present research of ideas of a past farther removed than a few decades" (p. 331). One of the more interesting points that Elffers-van Ketel makes has to do with "self-awareness." In arguing for the wholesome effect of self-awareness both for present day disciplinary study as well as for historiography, Elffers-van Ketel argues with regard to the concept of grammar that:

> The historically adequate concept of . . . "grammar" is the historiographer's and not the grammarian's *primary instrument*. But the grammarian has also learned something useful by constructing it. He now sees his own (grammarian's) concept "grammar," considered linguistically adequate, in the wider perspective of the historically adequate concept "grammar." This implies that he has observed . . . its ultimate boundaries . . . . At the same time he obtains, by establishing relatively stable non-basic domains, a survey of many types of variable elements of the concept and the regularities they reveal. The historically adequate concept "grammar" may thus function as a basic delineation, but also as a heuristic framework for further research, by revealing a very general picture of existing patterns of variation.
>
> If anything can be called . . . a concept's "identity," it is such a historically adequate version of it. This identity functions as a general background against which existing variants of a concept . . . can be clearly delineated, so that rational comparison and choice become maximally attainable, all concepts which have been made historically adequate can exert this function as analytical instruments. (p. 333)

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To the extent that such an explication is possible for SLA based on earlier texts, we would not be in substantial disagreement with Thomas.

17. Again, if there is relevance for current research programs, Thomas has failed to show what texts are relevant.

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