

## REVIEW ARTICLE (Part One of Two)

Larry Selinker  
The University of Michigan

Attempting Comprehensive and Comparative Empirical Research in Second Language Acquisition. A Review of *Second Language Acquisition by Adult Immigrants: A Field Manual*. Edited by Clive Perdue. Rowley, MA: Newbury House, 1984.

### INTRODUCTION

The volume reviewed here may be the most important book in second language acquisition (SLA) published in the past five years. The first version<sup>1</sup> has provoked discussion with colleagues from several continents. It is my impression that North American readers in particular are generally unaware of the impressive SLA research being conducted in Europe. It is hoped that this review article will help alleviate some of that. The material discussed in detail in the *Field Manual* (FM) raises questions about how we normally go about conducting research in SLA.

I have found both the first and the current versions very helpful as tools in graduate education in SLA, ESL/EFL theory, and applied linguistics. In fact, I have used the FM extensively with several classes of students.<sup>2</sup> I

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<sup>1</sup>Most of the volume under review was originally published by the European Science Foundation in 1982. The Newbury House edition, published in 1984, has an important Postscript to it, Chapter 10, which discusses the project 18 months into its work. Though the following colleagues bear no responsibility for what appears in this Review Article, I wish to thank them for discussing some of the ideas presented here: Dan Douglas, Klaus Faerch, Lily Wong Fillmore, Susan Gass, Gabby Kasper, Eric Kellerman, Carolyn Madde, Dennis Preston, Jacquelyn Schachter, John Schumann, and Russell Tomlin.

<sup>2</sup>The most useful experience for me in discussing this book with students was at the 1984 TESOL Summer Institute (Oregon State University, Corvallis, Oregon) in a course team-taught with Russ Tomlin, entitled; "Current Issues in Interlanguage Theory." Students and colleagues participating in that course included: M. Achiba, L. Allen, S. Creed, G. Graeber-Wilson, K. Graner, A. Johns, L. Landolfi, L. Lewis, D. Milkowski, J. Winn-Bell Olsen, E. Penti, I. Pozzi-Escot, C. Rinnert, G. Roelofs, M. Roth, K. Still, R. Vanikar. I also wish to thank Karl Drobnic and Wayne Haverson for setting up the intellectual climate that made that Institute such a success.

find that there is so much useful detail in the FM, that it helps solve some problems seemingly endemic to graduate education in these areas, two of which I will discuss here. First, there is the problem of the student who comes to the teacher at mid-term and asks, "What can I (should I) do for my term project?" One can now politely suggest that the student turn to the FM where there are dozens of suggestions for topics to study. Second, there is the perennial problem of "How do I do research?" Once again, "Turn to the *Field Manual*." Not only will the student find many ways to go about studying SLA data, but also, to my delight, there are detailed discussions of the pitfalls inherent in them. I recommend the FM as an auxiliary text, particularly in data analysis and field methods seminars in SLA.

To my surprise, however, there is no mention of the classic and now out-of-print 1967 *Field Manual for Cross-cultural Study of the Acquisition of Communicative Competence* (Slobin, 1967), which must have served at least as a source of inspiration. Both the Slobin FM and the present one are attempts at collecting comparable cross-linguistic data, and each is also a joint-product of a number of authors.

The 1984 FM is organized into 10 chapters:

1. Objectives of the Project
2. Lay-out of the whole Project
3. Previous work
4. Understanding, misunderstanding and breakdown
5. Thematic structure of utterances
6. Processes in the developing vocabulary
7. Reference to people, space, and time
8. Data collection and preparation of data for analysis
9. Quest for information
10. Postscript

Chapter 10, the Postscript, reports progress 18 months into the European Science Foundation (ESF) Project.<sup>3</sup> There is an Appendix entitled: "An Overall Scheme for Analyzing Interactions," and Chapter 8, in addition, has an internal appendix, labelled as an "Annexe," which is entitled, "Data Preparation Protocol." There is a reference section of approximately 180 items which is especially useful for European sources. But I find the

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<sup>3</sup>The entire five-year project, of which the 1984 FM is a part, is referred to in this Review Article as the European Science Foundation (ESF) project since the ESL is the project sponsor.

referencing in the FM a bit annoying. Although the authors have incorporated ideas from North American SLA colleagues, they have not, with some exceptions, adequately referenced us. I suspect, however, that, given our normal "bits-and-pieces" approach to research, we have more to learn from our European colleagues at the present time than the other way around.

The linguistic problems of immigrants are of interest to SLA theory because by-and-large these are untutored learners. It has been claimed many times that because such learners are unfettered by classroom constraints, they can provide insights into the acquisition process which may be more basic than the intuitions of classroom informants.

In the five Western European countries of concern to the authors of the FM (France, Great Britain, Holland, Sweden, and West Germany), there are some 11,000,000 foreign workers (Preface) mostly from Southern Europe. One also finds large numbers of immigrants from the Indian sub-continent and the West Indies in Britain, from England in Sweden, and from North Africa and Southeast Asia in France. In general, as the economies of Western Europe begin to shrink, many of these "guest" workers who helped to build European prosperity twenty years ago, are becoming less welcome. Even so, importantly, the foreign worker population that has remained is stabilizing and "renewing itself by its own growth." The birthrate of immigrant families is so high that there is a prediction by the Council of Europe that by the end of the decade, immigrant children will make up more than 20% of the grade school children. Some West Berlin classrooms already have 80% Turkish children.

Thus, as time goes on, much of the younger population can hardly be called immigrant. However, unemployment rates among foreign workers are higher than those of the indigenous population. Many of these workers and their families live as isolated minorities. There have been political calls for repatriation of all immigrants and their families, and, as a writer for *The Hindu* points out, discussion of race issues has become "almost a national pas-time" in Britain (February 25, 1983). An October 25, 1982 *New York Times* article about West Berlin discusses the "influx of foreigners, who now account for a tenth of the city's 2,000,000 residents." Problems relating to the decline of the city's economy and to the "ghettoized" community of 121,000 Turks, such as harassment by government clerks, sporadic violence, and run-down neighbourhoods are also discussed. Although, understandably, not much space in the FM is given to such sociopolitical issues, these problems do form an important and overt background to the

discussions in the FM. One of the strengths of the FM is its integration of the background, real-world context into its linguistic concerns and the attendant desire to look at informants in a most humanitarian way.

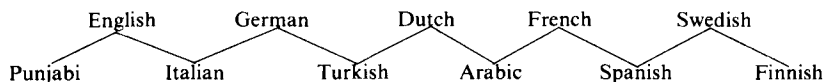
Before going into detail, there are several general concerns of the FM and the ESF project that need to be identified.

1. *Choice of linguistic phenomena to be studied.* Chapters four through eight delineate the categories and units of language and learning to be studied: understanding, misunderstanding, and breakdown; thematic structure of utterances; processes in developing vocabulary; and reference to people, space, and time. There is danger that these are, in varying degrees, preconceived categories. Coherent rationale for why these particular categories of language and learning are chosen for detailed study and, more important, why numerous possible others were rejected is never given, at least not to this reviewer's satisfaction. It is assumed, apparently, that the areas to be studied in the project are somehow central to the problems of the learners concerned, but nowhere is this explicitly argued. This may be a major defect in the undertaking.
2. *Comparability of SLA data across languages and countries.* The authors of the FM state:

The project consists of a series of *co-ordinated comparative studies* on the spontaneous acquisition of a second language by adult immigrant workers.

(Preface, emphasis added)

Their design is shown in the following diagram (Section 2.2):



Each target language (TL) concerned appears on the top line and each source language (SL) or native language, on the bottom.<sup>4</sup> The Postscript (Chapter 10) makes it clear that there are serious problems in terms of

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<sup>4</sup>In this Review Article, I will use the abbreviations provided by the authors of the FM. But the reader should note that the abbreviation TL for target language is standard, whereas SL for source (or native) language is not. This has caused minor confusion to some students who are used to the latter signifying "second language."

comparability. It can be argued, however, that even if perfect comparability is not in the end achievable, *some* comparability of subsets of the data should be. Also, what is empirically discovered in each setting in terms of interlanguage (IL) particulars may be of great value to the young field of second-language acquisition. Moreover, this comparative effort is worth making if only to help us discover what is replicable in SLA studies and what is not.

3. *Training of the Five Project Teams.* Central to the success of the project and especially to the goal of comparability is the matter of the qualifications of researchers. How well-trained will each of the teams in the field be? Will they be equally trained? What sorts of research background does each member of the team have? Some team researchers, who are named, are clearly well qualified as their published records would indicate. Others are unknown to this reviewer and background details are unfortunately not provided.

In judging the ESF project and the FM, it is important to realize that we are dealing with *a very young field*. Most researchers date the inception of the study of learner language to Pit Corder's (1967) paper: "The significance of learners' errors." In contrast, the study of grammar and the thinking and talking about language pedagogy are at least 2,500 years old. Our subject matter here is no more than 18 years old. This is another reason I find the FM so impressive. There are no other comprehensive, well-planned studies, which are comparative and longitudinal and which look at a number of TLs and SLs intersecting. This is truly a pioneering effort.

The outline of this Review Article is as follows: First I will discuss the Preface and the overall plan of the five-year EFL project. Then I will divide the FM into four parts (Chapters 1, 2 and 9; 10; 8; and 4, 5, 6, and 7). Chapters 1, 2, and 9 unite the objectives and design of the project with the selection of informants. Chapter 10, which stands alone, presents a summary of work done 18 months into the project. Chapter 8, by far the longest (72 pages), is about methods of treating the data. Chapters 4, 5, 6, and 7 all concern categories of language and learning to be studied in the project. Finally, Chapter 3 presents a useful summary of work done in the five countries *prior* to the ESF project, but will not be discussed here. Chapters 1, 2, and 9 are discussed in this issue of *Language Learning* and the remainder will be considered in a future issue.

## **PREFACE TO THE FM**

I will uncritically summarize the aims of the FM, including the authors' claims to uniqueness in terms of previous research. The FM itself is the result of the first year of work on a five-year research project, housed at the Max Planck Institute in Nijmegen, The Netherlands. The project is sponsored by the European Science Foundation (ESF) and is entitled "The Ecology of Adult Second Language Acquisition." The FM has the primary goal of providing to workers in the project a "theoretical and practical framework for the whole project." It is intended that this comparative and longitudinal project on untutored SLA by adult immigrant workers in Western Europe be carried out by teams working in the five European countries, with the five TLs, the six SLs and the 10 separate IL situations. Though acknowledgment is made of differences for immigrants among the five countries concerned, it is claimed that "the social and linguistic situations are similar enough to justify a parallel and coordinated investigation" (Preface). The FM presents and defines what has to be kept parallel in the five countries in order to successfully carry out this comparative project:

1. type and number of informants in each country to be studied,
2. the areas of language to be studied,
3. the "common core" research methodology to be used.

A detailed methodology for this kind of research is described in sufficient depth to be useful to anyone contemplating a comparative research effort.

The authors of the FM claim that there are at least five aspects of their project which go beyond previous studies in SLA:

1. the number of TLs and SLs studied,
2. the execution of a coordinated and longitudinal two and one-half year study in different linguistic environments,
3. the range of linguistic areas in SLA studied,
4. the relating of these areas of each other and to non linguistic phenomena,
5. the range of techniques used in data collection.

They also believe that the FM will provide information for other researchers in SLA, as well as for those interested in problems of immigration and in relationships between host (i.e., majority) and immigrant (i.e., minority) communities. They state that they hope the FM will lead to "a better understanding of the communication and relationships between majority and minority communities" (Preface).

The FM was edited by Clive Perdue of the Department of English Studies at the University of Vincennes at Saint Denis, Paris, and a member of the "Groupe de Recherches sur l'Acquisition des Langues." Perdue is currently at "The Planck," in Nijmegen, where the project is housed. Written contributions were provided by twenty named members of the teams from the five countries: Jens Allwood, Angelika Becker, Rainer Dietrich, Sian Dodderidge, Guss Extra, Daniel Faita, Ani Gamirian, Christine de Heredia, Colin Hindmarch, Wolfgang Klein, Tom Jupp, Michele Mittner, Collette Noyan, Clive Perdue, Hugh Pigeon, Celia Roberts, Peter Sayers, Sven Stromqvist, Kaarlo Voionmaa, and Daniel Veronique. Ideas in the original proposal, written by Allwood and Klein for the European Science Foundation in 1980, form the basis of Chapter One of the FM. Finally, the project has a distinguished international steering committee composed of: Ayhan Aksu, Norbert Dittmar, Tom Jupp, Willem Levelt, John Lyons, Bengt Nordberg, and Dan Slobin.

## OBJECTIVES, INFORMANTS, AND THEORETICAL ISSUES

The term "foreign worker" is defined as: "... workers and the adult members of their family," and "foreign" refers to language rather than citizenship. (1.1)<sup>5</sup> In their daily lives these workers "acquire what is most urgently needed, some even attain a certain fluency." Although fossilization is not named here, the notion is alluded to: "But normally, (the foreign worker's) acquisition slows down and even stops at a level that is far removed from the language of the world they have to live in" (1.1). Explaining the widespread existence of fossilized ILs is one of the underlying themes of the FM.

In a paper, written after the above, one of the authors of this section of the FM, Klein (1984), suggests that there are *advantages for fossilized learner varieties* if the freezing takes place "at not too elementary a level." First, such ILs are stable; second, they are "easy to master." If research proves this latter point to be correct, we may be approaching an understanding of the widespread prevalence of fossilization. This could prove especially important to language teaching colleagues who worry

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<sup>5</sup>Numbers in parentheses with no further qualifications refer to sections of the FM.

about this phenomenon. Researchers might wish to devote some effort to investigating the positive as well as the negative aspects of permanent fossilization. I have always thought that pessimism was unwarranted here, that one could *teach around* fossilization, especially within certain discourse domains (see Selinker, 1980 and Selinker and Douglas 1985). Researchers might wish to look at this as well, for it seems that fossilization (and language transfer and other strategies and processes as well) might well differ according to discourse domains.

The FM has as one of its secondary goals to contribute to language pedagogy “albeit indirectly” (1.1). This latter would, of course, be an important contribution of the overall project since the authors of the FM are also interested in contributing to both social understanding (i.e., helping to ameliorate the lack of understanding between host and immigrant communities) and scientific understanding (e.g., providing a distinction between what teachers might think a learner’s knowledge of the TL is and what it actually is).

The authors of the FM are primarily interested in the *processes* and *determining factors* of SLA. They have narrowed their concern to immigrants who “acquire some, often very restricted, knowledge of the language of their social environment” (1.1). This is called “spontaneous SLA” as distinct from “classroom SLA.” The term “spontaneous” prejudices the issues, in my view, since it is possible that, even without classroom experience, a learner could work hard at mastering the TL over a long period of time, something which can hardly be called spontaneous. Throughout the FM, the term “untutored” is used as a contextual synonym for “spontaneous.” “Untutored” does not seem to cloud the issue. Thus I prefer untutored SLA (USLA) to their spontaneous SLA (SSLA). In studying USLA, the major areas of investigation for the FM are the acquisition process and its determining factors, especially in the context of native/non-native (N/NN) interaction, namely the investigation of (attempted) communication between native speakers and foreign workers. It is possible, in principle, to investigate N/NN interaction *without* discovering anything significant about the acquisition process in SLA. But I believe that the authors of the FM would claim that the reverse is *not* true. Consequently, they anchor their study in the interactive domain.

A third major area of interest—in addition to the acquisition process and its determining factors—is a particular aspect of language use: “What does a learner’s language in use look like at a given time in the acquisition process?” (1.3.1). This links up with a large number of current SLA studies



in North America and Europe which concern such topics as avoidance strategies, paraphrase devices, nonverbal devices, etc. I feel the FM authors should have cited more of their North American colleagues here. They also include the notion of success, that is, how (un)successful a particular interaction is “felt to be” by both the learner and the native TL speaker. They are interested in the structure of the learner’s IL at a particular point in time and the structure of the underlying acquisition process, which for them includes a detailed analysis of what they call the “tempo” of the acquisition process; that is, what causes the process to accelerate, to slow down or even to cease. This research focus on tempo, should significant data develop, could become one of the important and lasting contributions of the ESF project to the field of SLA.

The focus on tempo is a by-product of their use of a longitudinal design. The authors correctly point out (1.3.1) that there is “little detailed longitudinal information” about the process of adult SLA. They recommend, to alleviate this situation, a “‘qualitative’ (non-statistical) approach” as a necessary first step to a diachronic understanding of NN/N interaction, in their case, between foreign workers and members of the TL communities. They explicitly relate this research approach to what is clearly one of their underlying goals: “... *explaining* the general existence of non-native, fossilized varieties of TLs” (1.3.1, emphasis in the original). They relate this latter phenomenon to the general problem of studying ILs in use, that is how the learner

... applies his restricted repertoire of lexical items and grammatical rules in order to communicate, and how these repertoires interact with more general communicative skills, such as particular non-verbal means and specific discourse techniques. (1.3.2)

With such overall goals, one of the problems of the FM mentioned above, that is, a clear motivation for studying the linguistic and learning areas chosen and rejecting the ones not chosen, begs for a careful discussion which this reviewer finds lacking. To be fair, the authors do in fact claim to have provided this at the end of 1.3.2 and 1.5. What I find, however, is a listing of what they think is important to study but with minimal justification, most of which has come out of earlier research.

The authors do justify the claim that “to understand how fossilization comes about,” one must undertake longitudinal studies of learners “from the very early stages of acquisition” (1.4.1; see also 1.3.1 and *passim*). Their justification is that this is the only way one can determine why “relatively

stable, non-native varieties of the TLs in the target countries" exist (1.4.1): we have to learn whether different stabilized/fossilized varieties<sup>6</sup> result from different processes or whether there is one acquisition process and these varieties represent "different stages" which for unclear reasons, cease earlier in some learners than in others (1.3.2). There is no doubt in this reviewer's mind that the potential for a significant, lasting contribution to the field of SLA exists in the serious attempt to contribute to an understanding of how and why fossilized ILs come about. The FM, in this as in other cases, attests to the value of attempting to carefully explain issues relevant to questions we ask in SLA research.

In the ESF project, newly arrived immigrants will be compared with "long residence groups" (LRG's), especially in England, Germany, and Sweden, where the majority of foreign workers have been in the host countries for some time. Importantly, in spite of extended residence, the social and linguistic problems of these workers "remain acute" (1.4.1). The questions to be investigated revolve, as might now be expected, around reasons for the persistence of stabilized/fossilized varieties. Why, after long-term residence and the acquisition of some TL linguistic skills, do these workers "nevertheless have great difficulty in making themselves clearly and successfully understood"? And why is this apparently true even "for many aspects of their daily lives"? It is the latter point which I find theoretically intriguing. It has been proposed several times (e.g., Corder 1981) that the factor controlling fossilization must be "the meeting of communicative needs." That is, the learner will cease IL development when there is no further "need" to develop. If there were such a need, it is claimed, the learner would develop the IL further. What is claimed in the FM, on the other hand, is that fossilization occurs before communicative needs are met. In fact, if I read the authors of the FM correctly, there is a *propensity* for some workers to *fossilize from the beginning*. An example would be "gatekeeping situations" of Turks in Germany who need housing, work, government forms, etc. In such situations, very early fossilization is linked to avoidance, misunderstanding, and the learner's lack of a right to speak." If the learner's right to speak is limited by the sometimes hostile reactions of native speakers of the TL, and if misunderstandings regularly occur,

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<sup>6</sup>For the distinction stabilized/fossilized see Selinker and Lamendella, 1979. Unfortunately, permanent fossilization vs. temporary stabilized plateaus are not carefully distinguished in the FM.

learners can feel socially and psychologically distant (Schumann 1978). This can lead to avoidance and, it is claimed, early fossilization (1.5.2). Unfortunately, for what is otherwise a convincing argument, it is not clear in the discussion exactly how avoidance is related to early fossilization. Perhaps the problem is that the term avoidance is used in two ways in the FM, and these are not clearly distinguished in the discussion: avoidance of particular linguistic structures (the more traditional use) and avoidance of all but inevitable interactive contact with native speakers of the TL.

I have always thought that very early fossilization of some aspects of ILs is indeed possible, but what appears in the FM is the strongest statement I have seen in the literature to date. If all of this is true, it would be useful for investigators in the ESF project to provide detailed *positive evidence* of early fossilization. (See Selinker 1984 for more discussion.)

To this reviewer's delight, the authors of the FM do not hesitate to provide hypotheses and suggest empirical directions for investigating perplexing questions. In fact, one senses in these scholars the joy of insight. For the reader, unfortunately, many of their interesting hypotheses are difficult to identify, as they are "buried" deep within sections. One goal of this review, therefore, will be to make some of these hypotheses more explicit. For example, they hypothesize that it is *not*, in the case of LRG foreign workers at least, "cognitive prerequisites" that determine fossilization, but "external circumstances independently, and also acting on motivation" (1.4.2). What follows from this is that learners with fossilized ILs should change these ILs with a change in situation. Accordingly, ESF researchers will include as informants LRG's whose life is undergoing change. I await careful documentation of such cases with great interest.

Another important hypothesis buried in a paragraph in 1.5.4, is as follows:

... learners will initially use TL-independent principles in expressing (temporal) relationships, and ... TL-specific devices will progressively be introduced. (1.5.4)

This hypothesis relates to a well-known hypothesis by Corder (1981) (not cited in the FM) that learners, in creating ILs, do not begin with the "full-blown" NL, but with an "initial hypothesis" which consist of some sort of basic core, possibly universal, which is subsequently "complexified."

It is based on previous work (see their Chapter 3) which shows that in the early stages of acquisition, German tense markings of the verb and various

“complex” adverbial constructions are “never used” and that “simple” temporal adverbs are used only in a restricted way. They generalize that in the early stages of acquisition:

temporality is exclusively expressed on the one hand, by a limited number of simple adverbs and by pure nouns in temporal function . . . and on the other hand, by discourse principles. (1.5.3.2.)

Thus we have a strong hypothesis based on earlier, limited research. This is followed by discussion of what they intend to do in the ESF project in order to study their informed guesses. They propose to investigate “how the learner makes use both of the principles governing the order of utterances in discourse” and various pragmatic constraints.

A third hypothesis concerning processes in the learner’s development of vocabulary, is:

If . . . successful communication with native speakers of the TL involves recurrent use of specific items in some environment, then . . . the corresponding lexical field will exhibit a high degree of precise differentiation. (1.5.4)

Thus, there is an attempt to study the developing learner’s vocabulary in “selective lexical fields.” Some of these will be that of “home,” “work” and “social relations.” These have been chosen, specifically because of a fourth hypothesis:

If an environment is perceived to correspond closely to some environment of the SL culture, the corresponding lexical field may be structured according to SL principles. (1.5.4)

Hypotheses such as these, which motivate empirical work, are important since little is known about the developing IL lexicons of learners. This last hypothesis links up with the vibrant research area of language transfer, and it would be interesting if these researchers discover a means of determining when a learner perceives a TL environment as “corresponding closely” to an SL environment. More about vocabulary acquisition will be discussed later.

It is interesting to note that in this area of lexical acquisition, SLA researchers are informing general linguistic theory as to what it lacks if it is to become a full-fledged theory of language. The authors of the FM point to a gap in linguistic research: “There is no elaborate descriptive technique

which would allow a systematic description of full semantic systems in evolution" (1.5.4.), because linguistics, (in this case, lexical semantics) by-and-large describes "more or less stable systems." There may be another reason for this lacuna: the problem which Corder (1981) called "interpretation" of learner utterances. The linguist describing an IL has "no immediate access" to the language being described. For example, while learners may perceive TL lexical items, at times, with *different sets* of semantic features than do native TL speakers, the linguist has only the lexical items as data. The authors of the FM provide examples from previous research. In order to tap the intentions of their IL users, the researchers in the ESF project will use play-back techniques that are standard to ethnography. (They unfortunately refer to these techniques as "self-confrontation" (8.4.4.), a name which may conjure up connotations of conflict.) Their data base will thus be twofold representing what I wish to call "primary" and "secondary data." The former are the learner's utterances and the latter are taped reactions in which learners will be asked to "make explicit" their communicative intent. A nice angle here is the focus on "propensity factors," that is, factors in the areas of linguistic awareness and emotional attitudes that favor acquisition. Secondary data will be examined to test whether and to what extent learners can verbalize these factors. The authors hypothesize that such verbalization is a variable relevant to tempo and structure of a developing IL. I await with interest careful description of this outcome.

We now turn to a discussion of the design of the five-country project and the quest for informants. One is immediately struck by the practical problems in attempting a five-country longitudinal study with five TLs, six SLs, and ten ILs (see above diagram). For example (2.2), some of the varieties of German which foreign workers are exposed to are so local that other speakers of German find them incomprehensible. This is also partly true for Swedish and French. Thus, the researchers cannot count on the availability of linguistic descriptions for these languages and will have to collect data on the TL actually in use in the environments of the workers. This raises issues for SLA in terms of important notions such as "target" and "input." The input to the learner may become in these situations quite different from the target of the learner, especially if media are involved. I wonder if some insights into what a learner regards as his/her target to approximate will emerge, and whether playback techniques can tap learner intuitions here.

The five-year project as proposed (2.3) intends to gather data on two

groups, one group of four informants per SL over a 30-month period, the other a control group. The choice of 30 months for data gathering is far from arbitrary, having important theoretical implications. (Unfortunately, the reason is buried in 9.1.1.) Earlier research by some of these scholars indicated that learner varieties appear fixed fairly early and that:

length of stay in the target community played no significant role after the first two years. (9.1.1)

This provides further evidence that, in central social situations, early fossilization is indeed the norm. Permanent fossilization is of course hard to demonstrate and I gather (Sue Gass, personal communication) that many researchers operationally establish a five year cut-off point. That is, if a structure (including the structure of discourse organization and pragmatic use) is stabilized for five years, then it will be called "permanently fossilized." What this pre-ESF result shows is that in some situations the lower bound on fossilization (see Seilinker and Lamendella 1979) is much earlier than previously thought, somewhere around two years.

Since there is generally clear separation of minority and majority populations in these five countries, we come upon a built-in bias in the study which the authors of the FM clearly recognize:

a great deal of (the learners') social contacts with the host population will consist of contact with the researchers. (2.3)

This is quite serious. On one tape which Perdue (personal communication) recorded in Paris prior to this study, the Spanish-speaking informant stated that the few hours spent with the interviewers constituted the most French which any native speaker had ever spoken to him. This is clear reference to the "junky data theory" of SLA which Lily Wong Fillmore (personal communication) speaks about. The researchers will try to reduce these "considerable control effects" by setting up one of their informant groups as a control to be sampled less frequently (2.3).

I appreciate this honesty in openly acknowledging potentially vitiating variables. This is a prime characteristic of the FM, and I hope that it is trend setting in SLA research. I also hope that we are getting over the twin curse of "objectivity" for its own sake, and the belief that there is no truth

without statistics. The position of the authors of the FM in these matters, as I read it, is that when we observe something, there is no way of knowing whether the result would have been the same had we not been observing. We must recognize that we, as investigators, are part of the process of investigation, and must try to minimize our effect upon our observations as much as possible. Concerning quantification, I read their position to be that some important phenomena are not easily assessed in quantitative terms. Quantify only if that data can be quantified. In ground-breaking research such as the ESF Project, however, qualitative studies must come first; we can learn much without undue quantification.

So far, we have discussed three kinds of informants: the main longitudinal group (LG), the control group, and the long residence group (LRGs). The fourth informant group will be a small number of TL native speakers who will be interviewed twice in order to test attitudes toward the foreign population.

It was expected in Chapter 9—and confirmed in Chapter 10 (see below)—that the “quest for informants” would be difficult. Chapter 9, a quite useful chapter (since many mistakes by new-researchers are made here), describes how the five country teams should go about finding and motivating informants. The authors of the FM are by and large clear on their informant selection criteria, which are designed as “safeguards against too much obvious dispersion in the background and present situations of informants” (9.1.2). These criteria will be applied in all five countries to help ensure comparability (9.1.2). Of course, they rule out informants with speech or hearing impediments. Also, they want to ensure that the TL is the second, as opposed to a third or fourth, foreign language. This is not only to assure comparability across subjects, but also to assure that SLs are properly identified. One of their major concerns is:

to attempt to characterize language-specific vs. generalizable phenomena in the acquisition process; that is, to examine the extent to which the informant's SL determines the acquisition process. (9.1.2)

This has been one of the most worthwhile themes of recent SLA research and has come about with the revival of interesting phenomena of language transfer. As discussed above (the discussion on early fossilization), the authors stress the importance of studying the SLA process as close to its onset as possible. Thus, in two of the four informant groups (the main

group and the control group), there will be an effort to secure informants who have recently arrived in the TL country (but see discussion of the Postscript in Part II of this review).

The authors discuss why they stipulate that their informants must be between 18 and 30 years of age, but unfortunately the reasoning escapes this reviewer. They wish to exclude immigrants who are undergoing regular foreign language instruction (called "tuition," based on British usage) as a potentially relevant variable. I think they are also saying (9.1.1) that level of formal education in the TL is also relevant and should be eliminated from their study. Thus, for various reasons, all informants should be working class. This leads to another openly-admitted bias, a strong class difference between informant and researcher. They will try to balance out the groups as to sex. In the main group and control group, they want informants who (1) have had no (or little) prior exposure to the TL, but (2) have exposure during the data-collection period in more than one of the "observational domains"; "work place, leisure time, authorities and/or consultation, practical everyday interaction" (9.1.2). For the third group, comprising the LRGs, they want informants who have lived in the TL country for at least five years and whose exposure to the TL is undergoing a change. This will enable the researchers to check whether observed fossilization effects are truly permanent. They also wish to exclude informants who are married to native TL-speakers or "who have children receiving full time education in the TL" (9.1.2). This final restriction may be unfortunate if it should cut down their subject pool significantly, as I imagine it would (see Postscript).

Concerning "finding and contacting informants," the FM states (9.2) that the situation in the five countries appears very different. Though not discussed here, these differences could seriously affect comparability across the five countries and the ten IL situations: Punjabi-English, Italian-English, Italian-German, Turkish-German, Turkish-Dutch, Arabic-Dutch, Arabic-French, Spanish-French, Spanish-Swedish, and Finnish-Swedish (see chart above from 2.2). As an example, details are given (9.2.1) of how very different the situation of Finns in Sweden is compared to the project "ideal." It appears to me that the Finns violate almost every criterion which was set up to assure comparability, particularly in their exposure to the TL, since Swedish plays an important role in Finnish society. One variable which is discussed at length (9.2.2) is "the general mistrust on the part of Turkish immigrants of German authorities." The authors nicely describe their "friend of a friend" approach which seems to



have resolved some of the difficulties, but they they describe how new restrictive legislation has limited immigration, thus posing serious problems for data collection, given their goals. The details and anecdotes are especially instructive here. (See especially 9.2.3, the summary of this section.)

How do they intend to motivate informants? First, financial arrangements are left vague in the FM (9.3) "as these may vary from country to country," another problem for the comparative effort. It is my experience that reasonable payment of an informant is best, as his/her time is valuable, and in this way, motivation for participation, especially for 30 months, becomes stronger. Interestingly, other types of "compensation" appear possible and even desirable in some situations. In Germany and France, pilot work demonstrates that informants appreciate the organization of dinners and parties in lieu of payment. Informants also seem to be motivated when the researcher recognizes the difficulties that foreign workers face and informs them that the results of the project may lead to understanding of the problems and "may be of practical value" for foreign workers in various ways (1.3). One criterion for motivation, however, truly surprised me:

(f) The interviewer is ready to help the informants with concrete problems. (9.3)

Though admirable, this is quite dangerous. Long experience with foreign students and ESL teachers in the U.S. has shown what a bottomless pit this can be and how much energy can be drained. How much more difficult the immigrant situation, often in hostile environs, must be. It would be most interesting to hear, on a practical level, what happens here, and one hopes that this information will be reported.

Ethical questions are briefly and satisfactorily discussed (9.4), and a "social biographical propensity data" profile sheet is provided in the Annexe to Chapter 9. This Annexe is a seven-page questionnaire which will be used to gather important background data on each informant. (I gather from the contents that this is to be filled in by the researcher and is not intended for use by the informants themselves.) Comparison across the five countries and the ten IL situations of this sort of information is important in order to see where the resulting linguistic data and are not comparable.

(Part Two will appear in a forthcoming issue.)

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