IN SEARCH OF EXPLANATIONS FOR INTERLANGUAGE PHENOMENA[1]

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Introduction

In the history of any discipline, vigorous theoretical debates often result when investigators propose alternative explanations for a phenomenon. Alternative explanations are of much interest in data-based research because they require an examination of theoretical underpinnings and therefore often lead to significant advances in understanding the phenomenon which is at the center of the dispute. A growth in the number of potential explanations for a phenomenon and in the frequency with which new explanations are brought forth to challenge traditional ones can therefore be seen as a sign of health in a young theoretical discipline.

For some time now, researchers and theoreticians have been laboring to find consistent, comprehensive explanations for interlanguage phenomena. Developmental explanations are opposed to explanations that involve L1 transfer, problem-solving or conscious learning. Explanations which take syntactic form or universal grammar as the starting point are opposed to explanations that take communicative function or conversational discourse as basic. While some have opened the door to a unified theory of language learning and language change (L.J. Dickerson 1975, W.B. Dickerson 1976), others have concluded that second language learning, especially in the adult case, must have a fundamentally different explanation from first language learning or language change (Bley-Vroman forthcoming).

On a formal level, the endeavor to explain interlanguage phenomena amounts to a search for the simplest possible theory that accounts for the facts of second language learning. Unfortunately, there are no direct routes to this goal, and even the goal itself is unclear. For what counts as "simplest," "possible," "theory" and even "fact" is highly debatable. Depending on the

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investigator's slant on each of these notions, different things will count as explanations. Some investigators will be looking for the "truth," i.e., for the "true" or "correct" account. Some will insist on comprehensiveness, i.e., on an explanation that accounts for no less than all the facts. Others will be satisfied with a plausible account for a relatively small set of observations.

It is a sign of health in a maturing discipline when theoreticians start to analyze their own theories and explanations, and the process by which they are derived. We have recently reached this point in SLA (second language acquisition) research, as the title and contents of the conference for which this paper was written testify. In order for the field to continue to grow in an orderly manner, it is essential for researchers and theoreticians to carefully weigh the implications of the types of descriptions, models, hypotheses and theories that they propose on the basis of their observations. It can no longer be considered sufficient (if indeed it ever was) to propose explanations for interlanguage phenomena which are not explicitly set into the context of prior experimental results and of the larger theoretical discipline. As a corollary, it follows that second language investigators must be cautious about overturning traditional explanations or building grand theories on the basis of individual experiments or small amounts of data.

This paper offers a meta-perspective on second language theory by examining the types of explanations which have currency in the field. It begins by a general discussion of the properties of theories, models and explanations, providing examples relevant to the study of language, with particular reference to phonology. After demonstrating how the meaning of theoretical explanations is bound up with their context[2], the historical evolution of the use of certain widely accepted constructs and specialized terms in SLA is examined. It is shown how implicit disagreements or misunderstandings may arise from variable use of central terms, e.g., **development** and **acquisition**, and from different underlying assumptions, e.g., about the nature of language. The importance of clarity and consistency in all aspects of theory construction is emphasized throughout. The paper concludes with some general reflections

[2] The relativistic, non-formalistic perspective on theories of the present paper is reminiscent of the discussion in Feyerabend (1965) and Bromberger (1969). It also draws on, though in many respects refutes, classical and formalistic accounts of theory construction such as Achinstein (1968); Eberle, Kaplan and Montague (1961); Grünbaum (1969); Hempel and Oppenheim (1948); Nagel (1969); Scheffler (1963); and Smullyan (1961).

on the construction and the evaluation of explanations within theoretical disciplines and some specific suggestions for SLA research.

Stages in Theory-Construction

The starting point for theoretical discussions is, logically, a phenomenon in need of an explanation. A **theory** is a set of propositions which characterize a phenomenon and which provide a basis for developing an explanation for the phenomenon. At one level, a theory is simply a set of statements describing the observations of a phenomenon which have been made up to a point in time. At another level of theory construction, a theory describes more abstract properties of a phenomenon which can be inferred from observations or from other information available to the theoretician. In many of the most interesting cases, the phenomenon which is the object of investigation cannot be directly observed—either because it is too large, or too diffuse, or too variable, or because it is in some sense "invisible." In moving away from descriptions of observables, theoreticians seek to characterize deeper, more general and potentially more significant aspects of phenomena.

A simple representation of the process of theory-construction might look like this:

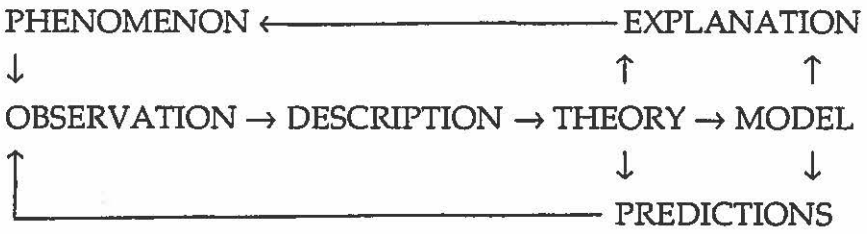

Figure 1: A Simplified Representation of Theory Construction

Theory-building can be analyzed as a multi-stage process. At the first stage (center row of Figure 1)—which possibly does not ever exist in the real world—the observer is in a pre-theoretical state. At this stage of theory construction, raw data, or observations, are described in observation statements, often accompanied by descriptive devices such as graphs and tables, and a theory or theories is/are constructed on the basis of those data. In this early stage of investigation, a potential explanation or explanations for the phenomenon under investigation can then be generated on the basis of the

theory or a derived model. Models may be invoked or constructed to test the theory, to help decide among alternative explanations, or to probe the meaning of the phenomenon under investigation through analogy and comparison with other familiar entities or properties of the world.

A general theory provides a framework for explaining different types of specific observations which investigators have made. Hence a theory can be seen as providing the foundation for an explanation. An **explanation** defines a relationship between observations and theoretical constructs, relating unfamiliar or abstract entities to familiar terms, descriptions or models. An explanation in this sense links the most obvious and concrete aspect of a phenomenon, the observations, to the deepest and most abstract level, the theory. A theory is therefore at a greater level of abstraction than an explanation, and is ontologically and historically prior to an explanation.

After a theory has been constructed, the theory defines new directions for observation, generally codified in hypotheses or predictions which may or may not be represented in a formalized model. In the next round, any observations that are made are now theoretically grounded, that is, they are bound to be both conducted and interpreted in the terms of the theory, often causing some redefinition of previously obtained observations. In this way, many investigated phenomena end up being constructs or abstractions rather than observables.

While this latter stage of theory-construction may be analytically distinguishable from that in which the investigator observes the phenomenon with a "pure" and unbiased pre-theoretical state of mind, in reality, the mind of the observer is never a *tabula rasa*. Any human investigator has to observe according to pre-existing perceptual schemata, or biases. In this sense, researchers are always observing and describing data within the framework of a theory, whether or not that theory has been explicitly articulated, and the attempt to maintain a clearcut classical distinction between observation terms and theoretical terms (Nagel 1969, Grünbaum 1969) is misguided.

Clarity is essential for progress in any research endeavor. Implicit theoretical biases and hidden assumptions impede progress by undermining many types of activities which are at the heart of research. For one thing, vagueness or incompleteness of any kind in the reporting of a study makes replication difficult. Moreover, these shortcomings may distort findings and

lead to misunderstanding of results. Lack of clarity also may be the cause of numerous unproductive disputes which turn more on semantics than on substance. As we will see below, lack of clarity in underlying assumptions and use of terms may cause researchers in second language acquisition to work at cross-purposes.

Models

A **model** can be defined as an instantiation of a theory. In other words, a model is a theoretically grounded representation of the phenomenon under investigation. Models of different types exemplify and substantiate theoretical phenomena, generally by focusing on one or another aspect of a particular phenomenon:

> A deep philosophical assumption underlying modern science is that the complexity of reality can be understood by understanding a collection of models—some that describe macroscopic behaviour by ignoring detail, others that successively explain increasingly microscopic behaviour. Whether this simplifying assumption is entirely valid can be debated, but our limited intellectual capacity **forces** us to make it. Without this assumption, we could not cope with the complexity surrounding us. (Wulf, Shaw, Hilfinger & Plon 1981: 3)

A typology of models is outlined below, with examples of each type taken from phonology.

TYPES OF MODELS	PHONOLOGICAL EXAMPLES
Simple predictive	Set of hypotheses predicting future experimental observations in the development of English /r/
Interpretive (imagistic)	Mathematical model of relationships among articulators
Analogical (physical)	Physical models of the vocal tract
Analogical (imagistic)	Wave metaphor as mechanism of sound change
Simulation (physical)	Computer synthesized speech
Simulation (imagistic)	Parallel distributed processing (PDP) simulation of operation of network of phonological relations
Constructed type (idealized)	Cardinal vowel space
Occurring type (optimal/unmarked)	Maximally sonorant vowels or maximally closed, voiceless consonants (voiceless stops)

Figure 2. Kinds of Models and Phonological Examples

A **simple predictive** model is an attempt to extend a theory's observational and explanatory domain by predicting future observations according to a set of hypotheses. An **interpretive** model provides an alternative way of representing the entities and relationships of a particular theory, e.g., by using the language of mathematics. Interpretive models are often simplified representations of complex phenomena. In psychology, for example, the description of the complex phenomenon of human learning is approached by modelling basic aspects of learning such as habituation and sensitization (Hawkins and Kandel 1984) in mathematical systems which have certain

properties (Wickens 1982). A mathematical model is a type of **imagistic**, or non-concrete, representation, like a diagram or other type of graphic representation. Other kinds of models may have actual **physical** realizations. **Analogical** models may be physical—e.g., a plastic model of the vocal tract—or imagistic—e.g., use of a wave metaphor in describing how sound changes propagate through speech communities (Labov, Yaeger and Steiner 1972).

Physical or mathematical models can be mechanized and so simulate the operation of the phenomenon under investigation. **Simulations** model a phenomenon by generating outputs according to the measured or predicted properties of that phenomenon. For example, the production of strings of phonemes can be simulated through a combined physical and mathematical representation of vocalization that is a type of speech synthesis. This type of simulation, which aims to replicate speech in its physical aspect, differs from a purely imagistic simulation of language such as PDP—parallel distributed processing—models (Rumelhart and McClelland 1986) which do not output actual vocal sounds.

Modelling according to **types** is a way of providing reference points for the discussion of entities and systems. In phonology, cardinal vowels located in idealized phonetic space provide useful reference points for describing and for predicting those vowel systems that actually occur in languages. Actually occurring sounds may also serve as models of unmarked or optimal reference points on the basis of which less optimal or marked sounds are described. For example, [a], which is maximally open and sonorant, can be thought of as the unmarked, optimal vowel type, while the voiceless stops [ptk], which are maximally closed and non-sonorant, can be thought of as the unmarked, optimal consonant types.

A model helps to relate a theory grounded in a specific observational domain to other theories and domains; in this way, a model can help to clarify the meaning of a theory and to fully develop its implications. An analogical model may help to make an otherwise opaque phenomenon understandable to beginning students or non- specialists. A typological model aids in conceptualizing the relationships among the items of a system and in predicting new items and relationships on the basis of systemic properties. Mathematical models and computer simulations based on these can also test a theory in a relatively abstract way, by examining the relationships and

interactions of quantities and properties under varying conditions such as differences in the specifications of the parameters which define the dimensions of the model.

The Delicate Balance between Observation, Theory and Explanation

The continuing process of theory construction can be thought of as a sort of meta-looping in which previously articulated theory affects data collection, description and further theory construction and model-building. Disciplines very quickly move to this meta-level, in which the discussion is in terms of meta-entities rather than observables. In fact, the progress of a discipline is intimately tied in with this sort of process, which is represented in Figure 1 as a continuous looping. As the theoretical foundation of a field continues to build upon itself, the observation, description and explanation of phenomena become increasingly interrelated, and in increasingly complex ways. In a very short time, newly created theoretical entitities—even observation statements—become abstract. As a result, any theory can quickly become a "house-of-cards," in danger of falling apart if the underlying phenomena have been imperfectly observed or described.

Imperfections in observation, description or explanation occur as a result of several types of shortcomings. The observations or the description based on these may be:

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A theory provides a way of restricting one's universe of inquiry and of deriving specific foci for research. While such restrictions are valuable and perhaps necessary for productive research, buying into a theory and its constructs often creates a kind of "tunnel vision," a narrowing of the field of inquiry to a domain too small to be representative of the phenomenon which the theory purports to characterize. This is probably the greatest potential danger in theory construction: that the researcher purports to be describing phenomenon A but is actually describing phenomenon B, a different phenomenon all together, or a subset of phenomenon A which does not fully represent all the complexity of phenomenon A.

The sampling of the phenomenon, i.e., the data, may not be representative because of limitations in the observations made. This happens in

phonology, for example, when the number of subjects is small or when the data consist entirely of readings of word lists or passages, which cannot be taken to represent natural speech. It also happens that the description of the data may be unnecessarily restrictive and so may be considered to misrepresent the phenomenon being investigated. For example, Labov's work on short-a /ae/ in English (Labov 1966, 1981; Labov, Yaeger and Steiner 1972) has been criticized for not categorizing the phenomenon according to a fine enough scale (Milroy 1982; Milroy and Milroy 1985; Romaine 1982). Similarly, standard, M.I.T.-style feature notation (Chomsky and Halle 1968), in restricting the phonologist to only two values of each variable, allows for only a certain type and amount of detail in observation and description. Some have argued (e.g., Ladefoged 1982) that multi-valued features are needed for accurately describing and predicting phonological phenomena.

The use of a particular kind of descriptive system or set of conventions—e.g., in transcribing speech samples—represents a major and unavoidable observational and theoretical bias. As Ochs (1979) reminds us, transcription is a selective process. The choice of one style of description rather than another focuses the researcher's attention on certain aspects of the phenomenon under investigation while de-emphasizing or backgrounding other aspects. Because of such pre-existing biases, a phenomenon may be reported not only incompletely but also incorrectly.

I have found in my own work on Japanese-English interlanguage a number of theoretically significant phenomena which other researchers have failed to notice or to transcribe. For example, an interesting developmental relationship has begun to emerge between the occurrence of epithetic vowels (mid or high central vowels epenthesized, i.e., appended, to consonants in final position), pause-filling syllables which might be transcribed as *ah* and *uh*, and weakly stressed function words (Pennington 1987). Such a relationship could never have been noticed if epithetic vowels or pause-filling syllables had been excluded from the transcription—as they often are—as unnecessary details, i.e., as unsystematic "clutter."

To take another example, many productions obtained from adult Japanese learners that would normally be classified as devoiced final stops from the perspective of English or Indo-European transcription are more accurately represented as devoiced syllables made up of a voiceless Stop +

Vowel sequence. Such a finding has significant implications for discussions of the relative importance of developmental phenomena vs. transfer in SLA since devoiced syllables are common in Japanese. Reports of widespread final devoicing in adult SLA has provided a major source of support for arguments asserting developmental parallels to child language acquisition (see, e.g., Eckman 1981, Tarone 1978), where final devoicing is said to be common (e.g., in Macken and Ferguson 1981). However, it seems that many of the recorded instances of devoicing might in fact be technically non-final, in that the devoiced consonant is followed by a devoiced (full or epithetic) vowel. If so, then the Japanese- English interlanguage devoicing data may have more in common with Japanese devoicing rules than with natural processes in child phonology. Or, as seems plausible to me, the data may show the combined influence of Japanese and natural phonological processes similar to FLA (first language acquisition).

In some cases, the researcher may find that the descriptive system which s/he applies to the data does not fit it very well. That is, a particular set of descriptive conventions may make it difficult for the researcher to find a consistent (or correct) pattern in the data. For example, Anderson's (forthcoming) attempts to find regularities of prosodic acquisition in English conversational discourse data from Japanese learners were frustrated by his use of a widely known English-based system for transcribing stress and intonation. When he switched to a Japanese-based system for transcribing word-level pitch-accent, some clear patterns began to emerge from the data. Eventually, he found that a description of the data in terms of both the English stress-accent and the Japanese pitch-accent systems of transcription yielded the most consistent and theoretically interesting results.

While there are some cases in which important facts or theoretically interesting observations are missed or distorted, in most cases it is possible to argue that different protocols for observation and description are all valid ways of categorizing a phenomenon, though each emphasizes a different perspective. If researchers are aware that they are placing certain limitations or biases on data collection and description, and that these restrictions constrain the possible types of explanation which will be generated, then they can deliberately compare and contrast the strengths and weaknesses of different theoretical bases and methodological approaches. Unfortunately, investigators are often not sufficiently aware of the limitations inherent in their methods and

theoretical bases, and so unaware of the restrictions they are placing on possible explanations for the phenomenon which they are trying to understand.

In what are presumably a small but recurring number of unusual cases in any field, data may be forced to fit certain observational or descriptive categories, altered in subtle or not-so-subtle ways (fudged), or even made-up (fanciful)—all of this potentially without any conscious intention to do so.

An interesting example is provided by Trefil (1988). Trefil recounts the history of telescopic observations of Mars by scientists such as Percival Lowell, who were thought to be meticulous observers of reality and who were held in the highest regard by other astronomers. On the basis of his observations of Martian canals, Lowell concluded that life must exist on the Red Planet. He published his results in 1908 in a book entitled, *Mars the Abode of Life.*

In spite of the great care with which he conducted his research, Lowell's findings were entirely illusory, as Trefil points out:

> What I wasn't prepared for was the sheer thoroughness of Lowell's research... Lowell, who initiated a major Mars research program at his observatory, claimed to have identified no fewer than 522 [canals]. Not only that, but he drew detailed maps of their layout on the Martian surface....What is frightening about all this is that Lowell was obviously talking about many observations, made over a period of years by highly trained and competent observers. All of this elaborate collection of data, calculation and conclusion was assembled **in spite of the fact that there is not a single canal or straight line on the Martian surface!** (p. 34)

A kind of pre-theoretical bias ("wishful thinking"?) had apparently guided scientific observation in this case, causing the scientist to "see" and even to carefully examine things that simply were not there to be observed. Trefil finds this case "frightening" because of its implications in the present-day research context:

> If a good scientist like Lowell could be so completely taken in, how much of what we do in modern science will someday join the canals of Mars as examples of human folly? (p. 36)

Some may find this a "quaint" example which has little relevance to other fields of investigation such as second language research. Others may have an uneasy suspicion that there may be analogous cases in the research field within which they work, or at least that such a thing could happen in their own field. Trefil believes that neither "hard" sciences nor social sciences are immune from this kind of occurrence. We may want to dismiss his fear as an overreaction; or we may see it as a correct reaction to an uncommon occurrence. At the very least, Trefil's discussion of the Lowell case should put those of us who are working to develop second language acquisition as a rigorous field of investigation on our guard. Specifically, we must guard against developing theories which determine incorrect data.

Theories are historical entities. A theory has a place in history in that it is tied to a particular time and circumstance. Taken out of a unique historical context, most theories would not have ever existed. Another way of putting this is to remark that a theory is relevant only in the context in which it is generated and in fact is made sensible only by that context. Theories widely accepted in one age—e.g., the theory of Martian canals—appear ridiculous or incomprehensible in another age. This is perhaps the strongest proof that a theory is no more (and, what is perhaps more telling, no less) real than anything else constructed in the minds of human beings.

There is another important sense in which a theory is a historical entity. A theory is required to account for both the data which has been gathered up to a particular point in time—i.e., all available data—and also all future data. Another way of putting this is to note that theories are supposed to provide for explanations and predictions of phenomena.

It follows that theories should comprehend the greatest possible amount of data, i.e., they should be as general as possible. At the same time as a theory should be maximally general, theories are domain-specific. A viable theory must both account for the available data and predict the characteristics of future data in the same domain. If certain phenomena do not appear to be explained or predicted under a certain theory, one might simply decide that they are drawn from an incorrect domain, i.e., a domain other than the one addressed by the particular theory. For example, one second language researcher might contend that certain data are drawn not from language per se, but from the larger domain of communication; or from discourse rather than from grammar. A theory should, however, be both inclusive and exclusive in

the sense of making explicit what is as well as what is not in its domain.

Generality is a desirable characteristic of a theory. As a rule, we can say that the more general a theory is, the better, in that a general theory forms the basis for comprehensive explanations of phenomena. However, a general theory must be relatable to particular observations, either through induction or deduction. While a theory may be at least partially deductive, i.e., not data-based, it must be data-testable.

Simplicity is also a desirable characteristic of a theory. If a theory is complex, the lack of simplicity implies that the broadest possible generalization, the underlying principle, has not been uncovered. On the other hand, one can argue that a particular theory or principle is too simple, in the sense of being a superficial property of the investigated phenomenon or in the sense of not having been derived from enough cases. When this happens, the investigator must often search for a deeper underlying generalization by re-examining data already collected or by collecting a larger sample.

Ultimately, a research field is seeking a maximally general and elegantly simple theory, i.e., one which accounts for the most data and which does so in the most direct and economical fashion possible. The property of **elegance** is not only a requirement for comprehensibility and aesthetics, but also relates to internal consistency and parsimony. In other words, elegance is itself an independent test of a theory's validity. If, as stated by Lass (1980: 3), "[e]xplanation by law applies to objects in the natural world, [and not to] cultures and their artefacts (like languages)," then elegance becomes an expecially important attribute of SL theorizing.

Often, the elegance of a theory can be said to **cause** it to make certain predictions. For example, the symmetrical properties of mathematical group theory as applied in physics made predictions about certain subatomic particles that were later discovered to exist in other than a mathematical sense. In phonology, Crothers (1978) has shown how principles of symmetry can yield descriptions of vowel systems from a specification of number of phonemes.

There is a sort of trade-off between elegance and simplicity of theory, on the one hand, and complexity of explanation, on the other, i.e., of statements establishing the relationship between theory and data:

> Insofar as we succeed in finding unifying principles that are deeper,

> simpler and more natural, we can expect that the complexity of argument explaining why the facts are such-and-such will increase, as valid (or, in the real world, partially valid) generalizations and observations are reduced to more abstract principles. But this form of complexity is a positive merit of an explanatory theory, one to be valued and not to be regarded as a defect in it. It is a concomitant of what Moravcsik (1980) calls "deep" as opposed to "shallow" theories of mind, and is an indication of success in developing such theories. It is important to distinguish clearly between complexity of theory and complexity of argument, the latter tending to increase as theory becomes less complex in the intuitive sense. (Chomsky 1981: 15)

Thus, a theory cannot be criticized for seeming intuitively obvious or simple. On the other hand, what passes for an elegantly simple theory may just as well be a simplistic one, risking vacuity. It often happens that the cause and effect relations are too specific, or too simple—e.g., they do not take account of enough factors, or proximate or immediate causes are considered identical to root causes. In some cases, the wrong factors as either cause or effect or both are adduced, or the relationship is reversed.

As an example, many have taken for granted the notion that transfer is causally related to adult second language acquisition in a very obvious way. However, the phenomenon of transfer and the ways it may operate in second language acquisition seem to be potentially quite complex. Even to claim that transfer causes transfer errors, and developmental phenomena cause developmental errors (Major 1987)—which on first view seems patently obvious and even tautological—is dangerously circular and possibly incorrect. It is not impossible to imagine that transfer causes or initiates certain types of developmental errors, or to put it a different way, affects the operation of certain developmental processes in interlanguage. It is also conceivable that some developmental processes set up the conditions for certain kinds of transfer. The causality of transfer and developmental processes may thus be two-way and cyclic rather than sequential and one-way: For beginning learners, transfer feeds acquisition, i.e., developmental processes; as acquisition increases, the opportunities for transfer increase (Anderson 1983), i.e., developmental processes in at least some cases feed rather than bleed the process of transfer; and these increasing opportunities for transfer in turn again

enhance the acquisition process.

Competing Theories, Models and Explanations in SLA

Two theories compete when they both purport to characterize the same phenomenon. Two sets of propositions which purport to characterize the same phenomenon and which are not mutually incompatible are necessarily either (1) not both applicable in the same universe of discourse or (2) not both theories. In other words, they cannot both account for the same phenomenon. In the former case, one set of propositions may characterize a related but not identical phenomenon, or may form a subset of the other set of propositions. In the latter case, one set of propositions may not be comprehensive enough to count as a theory.

Research disciplines often develop their theoretical base through elaborating competing explanations for phenomena which are of interest to that discipline. This process of elaboration, which generally takes place over a period of many years, helps to set different theories in sharper relief against one another and to generate a variety of models to test aspects of the theories. Two explanations compete when they both might logically account for the same phenomenon. Sometimes, it turns out that both types of explanation can be valid but for different aspects of the phenomenon. One explanation holds for one part of the data, and the other for another part. In such cases, the two types of explanations may be part of a larger model or theory in which the two kinds of explanation are explicitly related. Thus, competing explanations may cause investigators to look for a broader generalization or higher level of abstraction within which the two explanations may be complementary.

Two competing explanations often form two poles within one theory, dividing the universe of discourse into two analytically or definitionally opposite categories, e.g., acquisition vs. learning in the sense of Krashen (1981, 1982, 1985). In many cases, one of the constructs is historically prior to the other, the second having been developed to solve some of the problems arising from the restricted view of the universe associated with the first term. Theoreticians often argue as though the alternatives represent mutually exclusive choices, i.e., competing theories, only one of which can account for a given phenomenon. In reality, the truth often lies between the two poles, or is a combination of the two alternatives.

In the historical development of theories, compromise positions often arise after some period of ascendency of one explanation followed by a period in which two or more explanations are developed as competitors (Kuhn 1970). Thus, it is a sign of theoretical advancement when an explanation for a phenomenon invokes interaction of two factors previously viewed as competitive and mutually exclusive. For example, models of second language phonology stressing the importance of developmental processes (Macken and Ferguson 1981, Wode 1976) arose in reaction to strict transfer accounts (Briere 1966); aspects of these accounts were soon incorporated into "interactionist" explanations involving both transfer and developmental processes (Tarone 1978; Hecht and Mulford 1982). The most recent and sophisticated of these transfer-cum-development explanations is Major's (1987) Ontogeny Model of second language phonology.

According to Major's (1987: 103) model:

> At an early stage of acquisition, interference processes predominate at the expense of developmental processes, which gradually increase and then decrease over time. The reason developmental processes do not operate with a high frequency at early stages is that interference processes prevent developmental processes from surfacing. As interference is eliminated, developmental processes can blossom.

Bley-Vroman's (forthcoming) model of adult language learning as in essential respects more like problem-solving than like first language acquisition appears to be a reaction to the learning/acquisition duality and to extreme nativist explanations. Felix's (1985) cognitive competition model is an attempt to fit both developmental and non-developmental phenomena under one theoretical umbrella and perhaps to salvage some aspects of the Monitor Model. According to Felix's model, adult language learning shows evidence of competition between problem-solving and language acquisition capacities.

Models derived from the same theory may be variants of each other, and so not competitive as representations of a phenomenon. In some cases, one model may be wholly or partially included in another, i.e., the models intersect or stand in a subset relation to each other. Competing models do not always derive from competing theories. Sometimes two models of the same phenomenon are competitors in the sense that one characterizes the properties

of that phenomenon better than the other—e.g., by generating a more consistent set of predicted observations. Different models may be applicable on different conceptual levels. Since a model characterizes the properties of a theory in a simplified or abstract form, or in a representation brought to bear on a particular body of research from another sphere, it is common for two models to represent different aspects of a phenomenon, or to model a phenomenon in quite different ways. Thus, two models of a given phenomenon may not be entirely comparable and so not necessarily open to evaluation by a consistent set of criteria.

For example, it is not clear to what extent a parameter-setting model of language acquisition is comparable to, and thus able to be evaluated in the same terms as, other models of language acquisition (Bley-Vroman and Chaudron 1988). This is because the process of SLA, particularly in the adult case, seems to be in some important respects gradual in a way that FLA may not be. Hence the mechanism for resetting a parameter—if, indeed, parameters are reset in SLA—may not be the same as the mechanism for setting a parameter originally in child language.

As another example, the wave model of acquisition of second language phonology (Dickerson 1976) is not obviously comparable to Major's (1987) Ontogeny Model. Dickerson's model stresses the systematic variability of interlanguage phonology and its progress toward target phonological productions over time; Major focuses on the differing influence of transfer (interference) and developmental processes in the course of second language acquisition. While the two models are intended to be representations of the same type of (variable) data and are both concerned with intermediate stages of language acquisition, i.e., with interlanguage, it is not clear whether they should be considered to be competing or complementary representations of the process of acquiring a second language phonology. While he refers to Dickerson's work in discussing his own model, Major does not explicitly challenge or build on the earlier model.

It will be valuable in the future for the relationship between these two models to be clarified, particularly with respect to the important question of the degree to which the learner's approach to target phonological forms is via **phonetic diffusion** or **lexical diffusion**. This question in phonology is part of the larger question of the extent to which modifications of the interlanguage system operate by **rule** or by **item**. Flege (1980, 1981) has found clear evidence

of a type of phonetic diffusion in SLA. Hammarberg's (1984) discussion of the acquisition of Swedish sounds by adult second language learners shows evidence of learning based on phonetic principles, but only for those words which had no near counterpart in the native language. Those words which were similar to native language words tended to be pronounced according to a different, transfer-based strategy—in a sense a kind of lexical strategy. Pennington (1988) found evidence of the operation of natural phonological processes, L1 phonological processes, and other types of gradual phonetic diffusion in the acquisition of English short-a by Japanese learners, along with the operation of some special constraints for certain lexical items. In addition, Pennington (1987) presented evidence for higher level morphological, pragmatic and grammatical influences on the acquisition of some aspects of English phonology by the Japanese learners.

Major's model, like other earlier models employing the construct of development, posits a strong parallel with FLA. Thus, his model is intended not as merely analogical to FLA developmental theory. Rather, the model includes an interpretive representation of the construct of development for SLA. As will be discussed below, however, the evidence for a close parallel to development in child language acquisition may not be strong. Hence, Major's model may be better seen as analogical rather than as an approach to a grand unified theory of LA (language acquisition) linking together FLA and SLA.

Getting Clear on Terms and Concepts in SLA Research

Theories are context-bound in the sense of being tied to a particular discipline. The theoretical base of a discipline develops specialized terms describing the constructs and properties of the domain of inquiry. It is crucial for the terms to be precisely defined and consistently and carefully used by all who enter into the same theoretical arena. Otherwise, terminological problems may obscure real issues, and vague constructs may masquerade as explanations, models or theories. If the terms of a theory are not used in a precise and consistent way, those terms are likely to take on a more general meaning than may have originally been intended. And the more a term becomes a "household word" and takes on a "garden variety" meaning, the more it is likely to engender confusion in the field and to end up being theoretically vacuous.

While the opposition of learning and acquisition is intuitively appealing

and on first glance may seem to be an approach to an elegant theory, the term **acquisition** as used by Krashen and Terrell (Krashen 1981, 1982, 1985; Krashen and Terrell 1983; Terrell 1987) has more than one sense, causing imprecision of discussion and confusion among those trying to explain second language acquisition. These authors use **acquisition** in three senses: (1) the **non-deliberate activity** of processing input in the second language, as when speaking of acquisition activities vs. learning activities; (2) the **goal-directed process** leading up to the goal of internalizing a linguistic system, as when a speaker is said to be acquiring English, or the English tense system; and (3) the **finished state** of having internalized a language or a specific part of it, as when a certain morpheme is said to be acquired before another one. Sometimes Krashen and Terrell seem to be thinking of acquisition in only one of these senses; in other cases, they could easily be interpreted to mean all three senses simultaneously, or to be referring to acquisition vaguely, i.e., without any specific idea in mind (see Gregg 1984 for further criticisms).

The use of **acquisition** as an accomplishment term rather than a simple activity term (Vendler 1967), and in its perfective or completive sense, implies that there is a particular entity which is acquired at a certain point in time once and for all. It implies that acquisition has an end point that can be recognized and therefore specified in advance. This implication in turn belies the underlying theory of language that is presupposed in Krashen's work, as we will see below. Interestingly, nothing logically prevents acquisition in the completive sense from being accomplished not only through acquisition in the sense of (1), but also through learning in the Krashen-Terrell sense. It is almost certain that Krashen does not fully intend these implications; nevertheless, they clearly follow from the use of **acquisition** in his and Terrell's statements. Thus, an unclear use of terminology causes the theoretician to unwittingly imply conclusions with which he himself would not agree.

In the generation of explanations for new findings, sometimes terms and concepts are borrowed from other fields and imported into a new field, in the attempt to relate the phenomenon to other familiar and perhaps better understood phenomena. Generally, a term or concept is taken over into the new field with a meaning altered somewhat to fit the new context. At any rate, once imported into a new context, a term is likely to develop new associations and so gradually change its meaning. The exact denotative meaning of a term

in one discipline seems rarely to be applicable in a different discipline; moreover, the exact meaning of the term is unlikely to be known by those not working within that same specific field of inquiry.

As a result, when terms originating in one field are imported into another field, they are usually to be more broadly understood than in the exporting discipline. Terms used to describe or explain phenomena in the borrowing discipline are often in fact borrowed as metaphors, and explanations based on imported terms should therefore be viewed as potentially metaphorical. Of course, there is always the danger that the metaphor will be taken literally, or that the terms as used in the borrowing disicipline will carry over connotations and implications of the other field.

An example is the use of the term **developmental** in second language studies. On the face of it, the term appears quite vague and so to be semantically highly flexible. At the same time, the fact that the term is borrowed from studies of child language acquisition means that it has connotations of naturalness, innateness, and universal grammar or phonological processes. In the context of adult SLA, the term **development** may be associated with those changes within interlanguage which follow a "natural" path, whether or not they have any counterparts in FLA.

Within SLA, **development** has also taken on new connotations, in being opposed to **transfer** as an explanatory mechanism of second language acquisition. In the context of this opposition, **development** is coming to be used to describe any mode of language acquisition which is not transfer, whether or not an explicit argument is made for its being developmental in **nature**. We can thus end up with forms of development—i.e., acquisition paths—in adult SLA which are "developmental" and "non-developmental"—i.e., the same as or different from FLA.

Discussions of vowel epenthesis in second language acquisition exemplify differing senses of **development**. Major (1987) argues that schwa paragoge (epithetic schwa) is developmental in the sense of having a parallel in FLA and also in the sense of following a certain path in SLA. The parallel with FLA is hard to discern. I am aware of no FLA studies showing that children develop vowel epenthesis in circumstances identical to, or even analogous to, those in which adults do.[3] In fact, Sato (1984) and Riney (1987) present

[3] In studies of child language acquisition, Labov and Labov (1978) and Peters (1983) describe vowels as being used indexically and as fillers or place-holders, but these cases do not seem

evidence that vowel epenthesis of the type described by Major is essentially an adult phenomenon. Eckman (1981) describes it as a result of a "language contact situation," i.e., as a phenomenon that is absolutely different from first language developmental processes.

If schwa epenthesis is a common process in adult second language development but does not occur in child language development, then we have two choices. One choice is to tie together the two senses of **development** under a broader generalization, e.g., by describing them both as information-processing strategies. The other solution is to accentuate the differences between the two types of developmental processes, those of the child and those of the adult, arguing that SLA is fundamentally different from FLA, at least in certain respects. This is the course that Bley-Vroman (forthcoming), for example, has taken (though not based on this particular set of data), arguing that adult language development looks more like general problem-solving than like child language development. In either case, new terms will be necessary to precisely define the similarities and the differences in the two cases, i.e., child and adult learning.

Some elegant theories or forms of explanation and the constructs which are part of them are metaphorical entities. While notions such as charm and color in physics can be useful for conceptualizing a complex system of interrelations, there is the ever-present danger of their "charming" the physicist into a distorted sense of reality or of "coloring" the physicist's world view. Metaphorical constructs can blind theorists and researchers to other competing—and potentially better—constructs, explanations and theories.

It is essential to know when we are talking about observables and when we are talking about constructs, abstractions or metaphorical entities. Sometimes, the level of abstraction that is intended is not clear. For example, in comparing the phenomenon of adult vowel epenthesis to child language development, it is not clear exactly what observations Major (1987) intends to be compared. In describing adult language learning as problem-solving (Bley-Vroman forthcoming) or as a competition between adult problem-solving and universal grammar (Felix 1985), the degree of abstraction from the literal meaning of the terms **problem-solving** and **competition** is not entirely clear. In describing acquisition of second language phonology as a wave mechanism, Dickerson (1976) is apparently speaking quite metaphorically (whether

analogous to the cases of schwa paragoge in adult SLA as described by Major (1987).

intentionally or unintentionally) since he does not make a serious attempt to describe the mechanism and how it works—e.g., why one phonological environment induces a targetlike variant before another.

Chomsky clearly takes **parameter-setting** as an abstract term, i.e., a construct, though he believes this to be less abstract than other approaches to the description of language (see discussion below). It is not at all clear whether other researchers, e.g., Flynn (1987), are using **parameter-setting** on the same level of abstraction (Bley-Vroman and Chaudron 1988).

SLA and Theories of Language

Underlying any model or explanation for second language acquisition is a theory of language, whether or not explicitly articulated. **Language** may be conceptualized as primarily a pre-existing cognitive **structure**, as a **product** of acquisition, or as a **process** of communication. Grammar may be viewed as a simplified interpretive model of language or of communication, just as a "smoothed out" universe is a model of the real universe in which we live. Language functionalists see grammar as only part of the relevant phenomenon to be accounted for and not a model of that phenomenon because it does not represent the total phenomenon, i.e., its domain is too restrictive. However, some linguists see grammar as the essential phenomenon, and not necessarily as a model for some larger phenomenon which we might call communication.

While acquisition is viewed as both process and product in Krashen's work, language is always described in product terms. From his participation in and sympathy towards the morpheme order studies (Bailey, Madden and Krashen, 1974; Dulay, Burt and Krashen, 1982), Krashen shows clearly that he is thinking of language as a discrete set of items, each of which is internalized at a certain point in time. This view represents a relatively narrow focus on grammatical forms as the heart of language.

One can argue against a Krashen-Terrell view of acquisition of language by noting that neither a native nor a nonnative speaker has acquired grammatical morphemes such as past tense or the definite article in any completive sense at the time when these first appear in the repertoire of linguistic items. Even when a speaker starts using these forms consistently and correctly, we are not at all justified in stating that these morphemes have been acquired once and for all. As the results of much research has shown (for overviews, see Meisel 1987, von Stutterheim and Klein 1987), non-native

speakers may develop early systems for use of the past tense which are entirely consistent but which are limited to only certain contexts or which are quite different from the uses of these grammatical morphemes by native speakers (see also Huebner 1983).

Some subtle uses of the past tense, e.g., in conditionals and in contexts of politeness, are probably not mastered even by native speakers until several years after some other uses of the tense appear regularly.[4] Uses of the article in generics and with complex noun phrases such as gerunds and compounds, especially those of more than two elements, are surely quite late to develop, if at all, in some native and non-native speakers. In fact, one can assume that the rules for article use with complex noun phrases remain highly variable for many speakers throughout their lifetime.

The view of language as a set of grammatical items is opposed by theories of language such as that of Hymes (1974), who sees language as a "system of systems" (p. 153) organizing "the multiple relations between linguistic means and social meaning" (p. 31). Thus learning a language requires learning items as parts of a system, and learning the systems in the context of each other, rather than as isolated entities. In a similar vein, Canale and Swain (1980), elaborating on Hymes' (1972) notion of communicative competence, describe knowledge of a language as a set of competencies which must be learned as much as skills as discrete items. On this theoretical foundation, functionalist approaches to second language research have been developed, in which the starting point of investigation is not only a set of linguistic items, but also a set of communicative functions, making the analysis multidimensional in the sense of Long and Sato (1984).

Considering the fact that a native speaker's competence in a language can increase over time and the fact that an individual's phonological and grammatical systems are variable (Labov 1972) and subject to change at different points in one's life, we can see language not as a static system of systems, but as a dynamic system of interacting systems. From this perspective,

[4] The Rumelhart and McClelland (1986) PDP model of past tense acquisition starts from the assumption of past tense acquisition as a unitary phenomenon learned on the basis of present tense forms. It is therefore unable to model the fact that different lexical forms and different functions of the past tense morpheme are not all learned at the same time. This type of model can therefore not aid in developing explanations for the mechanisms of FLA or SLA diffusion of any kind. Chomsky's models also suffer from this shortcoming.

language may be viewed as a fluid entity with shifting boundaries. In this sense, interlanguage is parallel to other forms of language and can be described in the same sorts of terms, as in the work of Beebe (1980, 1984) and the Dickersons (L.J. Dickerson 1975, W.B. Dickerson 1976).

Even more radically, language can be described as essentially a dynamic process involving communication between at least two people (in the "default" case, the two individuals would be one person and an imagined audience, which might even be that very person). From this perspective, coherence devices and exchanges involving several utterances become an important locus of investigation, and constructs such as negotiation of meaning and discourse processes might be seen as central to the explanation of observed linguistic phenomena.

In yet another conception, Ladefoged (1980: 496) describes a language as "the social institution that permits formalized communication between individuals." From this vantage point, a language is a social and a cultural possession (Hymes 1974, Labov 1972, Milroy and Milroy 1985). As Labov (1981) has speculated, it may be impossible to ever truly "possess" a language other than the first language since lexical exceptions to rules seem to be learned from parents' speech. Such an explanation might also be relevant to the findings of Coppieters (1987) on the variable acquisition of meta-rules of grammar and appropriateness by adult learners of French as a second language. Coppieters' finding that fluent adult non-native speakers of French do not give the same explanations for rules as native speakers may be evidence that adult non-natives are not privy to certain kinds of information in the native speech community which are accessed primarily in childhood. In one sense, a language is what any native-born child in a given speech community knows.

In another sense, the definition of **language** can be broadened beyond what a 5- or 6-year-old child knows, or beyond what anyone knows at any given time. It could be said that what a child knows is not (yet) language, or not fully language, or that what an individual knows is only part of language. From these differing perspectives, language can be seen as quite essentially open-ended, and influenced by increasing literacy and other cultural effects, rather than being a closed set of items or structures. On first glance, this view of language as open-ended may seem diametrically opposed to the view of language expressed in Chomsky's works as essentially grammar:

> the central concept throughout is "grammar," not "language." The latter is derivative, at a higher level of abstraction from actual neural mechanisms; correspondingly, it raises new problems. It is not clear how important these are, or whether it is worthwhile to try to settle them in some principled way. (Chomsky 1981: 4)

However, Chomsky (1981: 7-8) explicitly counters those who claim that his view of language is too restrictive:

> [I]t is hardly to be expected that what are called "languages" or "dialects" or even "idiolects" will conform precisely or perhaps even very closely to the systems determined by fixing the parameters of UG. This could happen only under idealized conditions that are never realized in fact in the real world of heterogeneous speech communities. Furthermore, each actual "language" will incorporate a periphery of borrowings, historical residues, inventions, and so on, which we can hardly expect to—and indeed would not want to—incorporate within a principled theory of UG. For such reasons as these, it is reasonable to suppose that UG determines a set of core grammars and that what is actually represented in the mind of an individiual even under the idealization to a homogeneous speech community would be a core grammar with a periphery of marked elements and constructions.

Viewed against the reality of what a particular person may have inside his head, core grammar is an idealization. From another point of view, what a particular person has inside his head is an artifact resulting from the interplay of many idiosyncratic factors, as contrasted with the more significant reality of UG (an element of shared biological endowment) and core grammar (one of the systems derived by fixing the parameters of UG in one of the permitted ways).

Those who work from these varied perspectives are trying to describe and explain different phenomena, though perhaps all under the umbrella term **language**. As a consequence, the type of explanation or theory of language learning in each case is bound to vary substantially. At the same time, while each group of researchers may have different research questions, their different

perspectives are not necessarily incompatible or even fundamentally opposed. Indeed, there seems to be no *a priori* reason that, e.g., functionalist and grammatical perspectives on language cannot be seen as complementary, each contributing useful insights to the construction of language learning theory.

Still, the pursuit of competing explanations and theories seems to be such an elemental part of the conduct of research that we can expect opposing positions to continue to assert themselves in SLA and for debates to rage in all quarters. Indeed, if they did not, I fear that much productive research would come to a standstill. Lass (1980:4) reminds us that:

> one of the main ways that progress comes about is by criticism, which is well served by destruction or attempted destruction. If a totally negative assessment causes the babies to be thrown out with the bath-water, this is no great loss; if the babies are really worth keeping and raising to maturity, someone is bound to fish them out again. (As long as we retain a sense of tradition, and are not too hard-nosed about discarding apparently 'refuted' or 'superseded' theories.)

Conclusion

According to Selinker and Lamendella (1981:202):

> [T]he understanding achieved by any field is strictly dependent on the metatheoretical perspective from which questions are asked and answered. Different perspectives attack the same problem based on different goals, each perspective considers different evidence, inteprets the same evidence differently, applies different research methodologies, and (as a result) arrives at idiosyncratic conclusions. We believe that the simultaneous applications of mutliple perspectives to an area of investigation is advantageous. However, this advantage will be lost when they are dealt with as producing competing theories of the same sort rather than as complementary alternatives of different sorts. Each perspective has advantages others lack, while at the same time embodying disadavantages in classical trade-off fashion. The major constraint on which perspective(s) a field should employ is that the perspective(s) chosen must be appropriate for the goals which motivate

the endeavor.

A phenomenon is **explained** when certain knowledgeable individuals, "experts" in the field in which the phenomenon is typically investigated, agree that it is explained. Therefore, the criterion for explanation of a phenomenon depends quite basically on what the phenomenon is and on who is doing the evaluating of the explanation (Achinstein 1971). Different evaluators have different requirements for explanations to the extent that they are interested in different research questions. Even if they are working in the same research environment, two investigators may not agree on exactly which phenomenon or aspect of a phenomenon is to be explained. To the extent that they have different perceptions of what it is that they are investigating, researchers are unlikely to agree on the type and depth of explanation that is necessary in a given case. As Hornstein and Lightfoot (1981:9) note:

> The question of what constitues a valid explanation can arise only in the context of some problem for which there is no self-evident solution. Hence in any domain there is an intimate relation between the way in which a problem is conceived and the kinds of explanation that one should offer.

Thus, explanations vary according to the nature and the specificity of the phenomenon under investigation and according to the expectations of those seeking explanations. Moreover, it is the investigators in a field who determine the phenomenon which is to be explained, i.e., the nature and specificity of the phenomenon of investigation. In the field of language acquisition, a linguist, who is primarily interested in language, might be satisfied only with a unified theory of language learning and language change, while a psychologist, whose primary interest is cognition rather than language, will probably not be satisfied with an explanation that does not refer to neural mechanisms. One whose research environment is sentence grammar may be quite interested in interlanguage explanations having to do with canonical word order, while a person who investigates discourse will find these kinds of explanations unconvincing, uninteresting, even trivial.

As a result of differing preconceptions and goals, it is very difficult to find cases in which a large group of researchers will agree entirely about

exactly what has been observed in a given experiment, much less about how a phenomenon can best be described or explained. Thus, explanations in different fields of investigation tend to be either confined to a narrowly defined phenomenon and agreed upon by a small circle of experts, or else quite general and widely accepted but short-lived. A field advances by changing its investigative perspectives—e.g., by increasing its database or asking new questions—and by refining explanations based on its changing investigative perspectives. As Selinker and Lamendella (1981:218) remind us, theoretical advances in interlanguage studies "will depend first on the character of the metatheoretical stance [i.e., the investigative perspective] in terms of which one identifies relevant data, and most importantly interprets these data as evidence validating one or another theoretical position."

The SLA field has borrowed much from the related disciplines of linguistics and psychology in building explanations for interlanguage phenomena. In many cases, the borrowed terms and constructs have brought along with them all the connotations of the other field. Sometimes these imports greatly increase the explanatory power of a theory in the borrowing discipline, but not infrequently at a cost in terms of clarity or precision. Some homegrown terms also suffer from a similar kind of ambiguity or vagueness.

New research should build on previous research, and improve upon it, continuing to refine the terms and constructs of the discipline. As the field of SLA advances, it should continue to expand its data base and to reexamine data previously collected in the context of current thinking. An increased effort to replicate previous studies can also help to clarify apparently inconsistent findings and to pinpoint the source of any differences in findings which are not artifacts of the methodologies employed.

In seeking clarity and replicability, researchers in the field will want to describe the pre-existing biases that they bring to their work in terms of careful statements of the background of the study and the exact phenomenon under investigation. In addition, methods and results need to be described in enough detail for replication and for a full understanding on the part of the reader of the results of the study. In discussion of results, careful attention should be paid to theoretical implications for the field at large and to other related lines of research.

In the future of SLA research, we can expect a continuing growth and accumulation of data and of explanations for those data. Moreover, we can

expect the explanations to continue to broaden and to account for larger and larger portions of the data. We can anticipate more and more sophisticated modelling by computer, with continued importation of concepts and methods from psychology. Beyond this, it is hard to tell in what exact direction this exciting young field will go. Like a child growing, we cannot predict exactly how it will turn out, but it is fascinating to watch and to participate in some way in building its bright future.

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