THEORY FORMAT AND STRUCTURE AND SLA THEORY

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SLA theory development has reached the stage where a meta-understanding of the forms and structures is needed to facilitate theory development. This paper reviews work in the philosophy of science pertinent to SL theory formats and structures, relating it to recent SLA theories.

1. Introduction

Within applied linguistics, calls for a theory of second language (SL) learning or acquisition (SLA; the terms will be used interchangeably) have become increasingly common since the 1970s, and a number of extended position statements have been produced (e.g., Krashen, 1980, 1981, 1985; Schumann, 1978; Spolsky, 1989; cf. Beretta, in press; McLaughlin, 1987)[1]. We have also begun to see some of these critiqued using criteria established within the philosophy of science (Gregg, 1984, 1989; Long, 1985). The use of such a basis for criticism has alerted the SL research field to the existence of an extensive body of work containing standards against which SL theoretical work can be judged. Most SL theoretical efforts, however, have not yet benefitted from a concern for such matters or a knowledge of the relevant literature. Foundational questions of this sort concerning the target of the SL research program and methods to attain it (cf Chaudron, 1986) are, however, essential to the development of the field.

In this paper, I review conceptions of theory most relevant to SLA with particular reference to their structure (i.e., components, such as hypotheses, and their arrangement—logically-related, or otherwise) and to their format or surface characteristics (i.e, whether theories are primarily instantiated in logical

1 Such proposals have been variously labelled 'theory', 'theoretical model', 'conceptual framework' etc. For discussion of the distinctions that can be made between these terms, see Giere (1979; cf. Brodbeck, 1959, Youngquist, 1971).

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symbols, computer code, grammatical symbols, thought, and so on). A multiplicity of meanings have been accepted for 'theory' because of the initially differing orientations of the various domains of science (see, e.g., Kantorovitch, 1988, and for applied linguistics, Stern, 1983), together with the persistence of older understandings of the term in some disciplines coexisting uncomfortably with newer senses established in other disciplines. In addition, the tacit understanding of 'theory' by scientists has not always agreed with the meaning given it by philosophers of science, and the potential for confusion is further exacerbated by the fact that the nature and intent of theories is the subject of continuing debate in the philosophy of science.

I take it as axiomatic that we cannot efficiently construct SL theories without a good understanding of what they are, both inside and out. I will initially lay out conceptions of (primarily) theory format, separating them by historical sequence, and to some extent by discipline—this refers to the "outside" of theories. In the next section I will present an overview of theory structures with reference to a key function of theory, explanation—this concerns the "inside". In the final major section of the paper I show how this determiner of theory structure should aid assessment of past work in SLA theory and guide future SL theorizing; I also return to format by discussing the role formalisms may have in future SL theories.

2. Theory formats

The interdisciplinary nature of applied linguistics has resulted in SL theorists inheriting several partial understandings of 'theory' and thereby theory format from our various contributory disciplines. In addition, SLA theories, precisely because of their interdisciplinary concern, may need to have the characteristics of theories from more than one discipline. Because of its historical precedence, the earliest and perhaps most dominant ideas concerning theory formats are those associated with the physical sciences, which I will consider first, followed by those of cognitive science. I will then briefly note recent developments applicable to SLA.

2.1 The 'Received View': the traditional philosophy of science perspective on theory format

Concerning what a theory should look like, according to Giere (1985, p. 344), "[s]ince Euclid there has existed a more or less continuous tradition of representing theoretical knowledge in the form of an axiomatic system". However, until the turn of the present century, there was little explicit direction as to what form a theory should take. Ideas about theory format became most prescriptive with the development of the philosophical movement of the nineteenth century known as positivism. Positivists (e.g., Mach, 1886, cited in Robinson, 1985) agreed with earlier scientists that theories should consists of statements of "general facts", that is, laws, but required additionally that theories be in no way "metaphysical", containing only empirically observable terms. In the subsequent development of logical positivism, implementing this requirement was seen as possible through the application of the newly developed science of logic, first to physics (e.g., Frege, 1893/1964; for historical review see e.g., Suppe, 1974) and thence to all other sciences.

> Positivists conceive of theories as organized only according to the canons of deductive logic... The effect of this is to force them to conceive very narrowly of theory and its ideal logical structure. (Harré, 1985, p. 53)

In this conception, a theory is an abstract system of propositions, stated and related logically and derived from initial axioms, which represent a set of experimental laws and depict their interrelationships. Centrally, certain sentences are identified as generalizations, from which hypotheses can be deduced. The theory is therefore 'hypothetico-deductive', best expressed as logical propositions within a first-order predicate calculus (one of many possible logics). Such a theory has been referred to as an 'axiomatic' theory;[2] casting a theory in this form is 'axiomatization'. Until around the 1960s, there was general acceptance that all good theories were of this form.

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2 As distinguished within this tradition from 'set-of-laws' theories, which consist merely of sets of empirical generalizations not deductively related (Reynolds, 1971; for a more detailed classification of such theories cf. Hawes, 1975).

2.2 Cognitive science practice and theory format

Theory formats in cognitive science[3] have rarely fully conformed to Received View prescriptions. Consider the work of Hull (Hull, et al., 1940), one of very few in psychology to have direct contact with prominent figures of the logical positivist movement, and apparently the one major psychologist concerned to establish a formal axiomatic theory. In a systematic comparative analysis of pre-cognitive learning theories (Estes, et al., 1954) his efforts with regard to "explicitness of axiomatization" are evaluated highest of the five surveyed, yet fail to satisfy the standards of his commentator (Koch, 1954, p. 59), who observes that

> the formalistic impression created by the elaborate verbiage of the postulates, the many symbols, and the mathematical trim tends to obscure many sources of ambiguity in the theory.

Hull's attempts to develop behaviorist learning theory upon logical positivist principles were regarded as thoroughly misguided by members of this school (Smith, 1986). In Koch (1959), the other major comparative survey of psychology conducted in this period of Received View dominance, it is clear that theorists had mixed feelings about whether axiomatization was desirable or not. Even those who thought it useful tended to see their work as failing in this respect. Succeeding Hull as the major figure in this field, Skinner "explicitly reject[ed] theory construction by the axiomatic method" (Verplanck, 1954, p. 300), but this anti-theoretical push was rebuffed for the study of language by Chomsky's (1959) review of Skinner (1957). Chomsky's initiation of modern linguistics provides a lead-in to the beginnings of cognitive science. Because of its dominant historical and ideological position (cf. Newmeyer, 1980), the Chomskyan research program has played a major role in determining what one important section of cognitive science conceives of as theory. Formed in isolation from mainstream conceptions of scientific theory (because of its

[3] Hunt (1989, p. 603) defines cognitive science as consisting of "psychology, linguistics, anthropology, philosophy, computer science, and the neurosciences". Theories of SLA are primarily located within this grouping, with the possible exception of those which deal with SL learning as a psychosocial phenomenon (e.g., Schumann, 1978).

earlier association with anthropology) and influenced by its role in attacks on the preceding antitheoretical trend in psychology, theories in linguistics (and because of the influence of linguistics, in cognitive science generally) do not represent a smooth development of Received View conceptions of theory (neither format nor content).

2.2.1 Theory formats in linguistics

Modern theoretical linguistics primarily results from the development of Chomsky's ideas about language, and has been characterized by the proliferation of a family of formalisms—typically, directed graphs (phrase markers) and symbol strings (rewrite rules). For many outside the field, its theories might seem to have been correctly described by Pylyshyn (1973) who referred to them and to other competence theories in cognitive science as "set[s] of formal logical rules" (p. 31). Chomsky's ideas, however, have undergone several major conceptual shifts (Botha, 1989; Smith & Wilson, 1979; Starosta, 1987; Steinberg, 1982) in which old terms have been retained while their meanings have been altered; there have been some consequent difficulties. From the earliest phase of Chomskyan linguistics, the connection between grammars, linguistic theory, and mainstream scientific theory was not clear. Sanders (1974, p. 3) notes that

> considerable misunderstanding has been generated ... by the largely idiosyncratic terminology that linguists have traditionally used in referring to the properties and components of linguistic theories. Thus in place of such familiar general scientific terms as "theory", "law", "axiom", "proof", and "theorem", one typically finds in linguistic discourse an entirely different metalanguage comprised of such specifically linguistic terms as "rule", "derivation", "grammar" ... These striking differences in metalanguage could easily lead one to suspect that linguistics is much different from other sciences than it really is.[4]

4 In a footnote (1974, p. 3) Sanders explains how to make the necessary conversions:

> The law-like character of directed... rules of grammar is... apparent... [A]ny phonetically-directed rule of the form "X—>Y" is translatable without loss into a clearly law-like statement of the form "(for all X) (for all Y)[(X=Y) and (Y is more phonetically proximate than X)]". It is also possible to translate directed rules into

Additional complications may have arisen from a general understanding that 'a grammar is a theory of a language' or '... of linguistic competence', which might have been stimulated by comments such as the following:

> What we seek, then, is a formalized grammar that specifies the correct structural descriptions with a fairly small number of general principles of sentence formation and that is embedded within a theory of linguistic structure that provides a justification for the choice of this grammar over alternatives. Such a grammar could properly be called an explanatory model, a theory of the linguistic intuition of the native speaker. (Chomsky, 1962, p. 533)

Assuming the set of grammatical sentences of English to be given, we now ask what sort of device can produce this set (equivalently, what sort of theory gives an adequate account of the structure of this set of utterances). (Chomsky, 1966, p. 18). Postal (1964/1967, p. 3) observes however, that whereas

> a grammar must be an explicit formal device which enumerates all and only the well-formed strings and which automatically assigns to each sentence a correct structural description (SD),

a linguistic theory, by distinction, is to be a general account of common features of grammars, and must contain

> 1) a precise characterization of the possible types of grammatical rule and their possible interrelations.
> 2) a characterization of the kinds of SD [structural description].
> 3) a mechanical procedure (algorithm) for associating a unique SD with each enumerated sentence.
> 4) an evaluation procedure or metric of simplicity for grammars to choose the best grammar out of all those compatible with the data. (Postal, 1964/1967, pp. 3–4)

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conditional statements, e.g., "For any linguistic object S, if X is a representation of S, then Y is a phonetically more proximate representation of S".

However, this remark seems to indicate that here, a grammar is indeed being assumed to be equivalent to a hypothetico-deductive theory, rather than a formal device, à la Postal (1964/1967).

The position that linguistic theories were formal symbol systems which generated "sentences" allowed the perception that linguistic theories were closest in format to other theories in cognitive science (those in AI, for example) as opposed to those in the hard sciences. However, in the last decade mainstream American theoretical linguistics has become interested in specific grammars mainly for the light they throw on general characteristics of human knowledge of language (universal grammar/UG), so it may be more accurate to say now that linguistic theories (as opposed to grammars) primarily consist of general statements from which together with specific grammars can be deduced hypotheses about language competence (indirectly testable via performance data).[5]

2.2.2 Computational theory formats

Elsewhere in cognitive science, the position developed that theories of human cognition can desirably be set out as computer programs which normally make use of production system models (Newell, 1967; Newell & Simon, 1961) typified by pairs of If-Then statements. Most generally, the position is that

> a computational theory of thought must define the mentalese language and describe a hypothetical machine that can execute programs written in it. (Hunt, 1989, p. 604)

Though the 'theory as program' position has been widely accepted, it has been attacked on both relatively traditional grounds concerning aspects of theory construction, and also as a result of newer developments. Under the former category, Simon (1979) remarks that

> bits and snatches of a program, reconstructed programs, a detailed program in its entirety, or even 'the theory behind a program' could

5 Linguistic theories are naturally 'property' rather than 'transition' theories—see Section 3 below, and they explain primarily by 'reducing the number of independent phenomena' (as defined in Aronson, 1984, pp. 171-184) rather than in a deductive-nomological fashion (Dretske, 1974; Miller, 1990; Starosta,1987; but cf. Hintikka & Sandu, 1991, p.5, who state that "GB theorists... use an antiquated hypothetico-deductive model of science").

> each constitute the theoretical ingredients of a computer simulation. Yet, practically speaking, it is doubtful that a complete computer program ever constitutes a theory. Many elements of a program... are irrelevant to theory construction... [T]he theoretically relevant features [must be extracted] from the program. Unfortunately... any number of verbal theories may be extrapolated from the same program. Moreover, simulationists must constantly guard against ... constructing a program that is more complex than the phenomena being studied. Yet, if we keep these difficulties in mind, it seems legitimate to speak, however loosely, of programs as theories.(p. 234)

Very similar comments are made by Pinker (1984, p. 351), and for Luce (1989, pp. 126–7), the programmed simulation of vision utilized in the respected work of Kosslyn (1980) was

> simply one programmer's version of what he believed Kosslyn had in mind as was evolved from discussions and informed by repeated computer printouts.

Luce also observes of this approach to theory construction in general that

> the programs are in no way uniquely determined by the principles, and so far as I can see they are communicable from one person to another only in the form of long listings of computer code.

In other critical discussion of such work, again using Kosslyn as an example, Finke (1989) notes that it abandons predictive power in its attempts to make the program fit the data, and thus explain it, and van Lehn, Brown, & Greeno (1984) remark that examples of the degree of fit and associated argumentation rather than proof are the principal means of support for programs. Another problem that has a familiar ring to SLA researchers is that

> nearly every researcher who has developed production system models of significant complexity has developed his own architecture and associated language. (Neches, Langley & Klahr, 1987, p. 18)

An alternative format for computational theories is embodied in models of cognition labeled 'associationist', 'connectionist', or 'parallel distributed processing (PDP)'. These have burgeoned rapidly since the mid–1980s (Schmidt, 1988), and can be seen by some (e.g., Pinker & Prince, 1987) as representing "an intermediate level between symbol manipulation and neural hardware" (Schmidt, 1988, p. 59). In this type of model

> learning takes place through the strengthening and weakening of the interconnections in a particular network in response to examples encountered in input. (Schmidt, 1988, p. 56)

The units in such a network are elementary, no symbol representation or 'if-then' statements are required, and in, for example, a PDP model of first language learning, there is no representation of 'rules': "The child need not figure out what the rules are, nor even that there are rules" (Rumelheart & McClelland, 1986, p. 267, cited in Schmidt, 1988; see also Sokolik, 1990). Currently, hybrid models are being developed which utilize aspects of both parallel (associationist) and sequential (production system) types of computer modelling (cf. references in Klahr, et al., 1987). Both kinds, however, are primarily represented and instantiated as computer programs—associationist versions showing, rather than If-Then statements, matrices of weightings of the elementary units from which their systems are constructed.

2.3 Developments in philosophy of science concerning theory format

Contemporaneous with the rise of cognitive science, philosophy of science underwent changes in many areas. For present purposes, we should note a relatively smooth development of the earlier concern for axiomatization, under which theories became seen not as sets of propositions, but as "extralinguistic entities which may be described or characterized by a number of different linguistic formulations" (Suppe, 1974, p. 221). Initially associated with Suppes (1957), the 'Semantic Approach', or 'Model-theoretic Approach' has been developed by Suppe (e.g., 1989) among others (cf. Balzer, Moulines, and Sneed, 1987; da Costa & French, 1990; Sneed, 1971; Stegmüller, 1973; Westmeyer, 1989).

In this approach, a theory constitutes a set of mathematical models[6] underlying the phenomena or systems to which the theory pertains, and is depicted as a collection of elements and their relations, formally stated using set-theory terms. This development was motivated particularly by an awareness of the inability (by standards of logic) of first-order predicate calculus to adequately represent theories (e.g, Suppe, 1967), as well as the unnecessarily limited perspective of the Received View that the relationships between elements of such structures were simply deductive relations among statements. Currently most axiomatization of theories which have been initially promulgated non-formally utilizes "semantic/model-theoretic methods" (da Costa & French, 1990, p. 250, and cf. Westmeyer, 1989). This approach is a direct outgrowth of a philosophy of science which sees itself as systematizing the work of scientists, as the logical positivists had done previously.

Despite the existence of this more developed set-theoretic form of axiomatization, it is noteworthy that "scientists in the twentieth century rarely present theories in axiomatic form" (Giere, 1985, p. 344, referring to biology, geology and even physics), and Reynolds (1971, p. 97), states that the scarcity of this type of presentation "suggests that it has either been impossible or inconvenient for social scientists to put their ideas into this format". This may not have been a serious failing. From about 1950 onwards, the Received View came under severe attack, and one by one the tenets of logical positivism were eroded (e.g., Quine, 1951; cf. Manicas, 1987, for review). However, the Received View, often widely misunderstood, lived on as an ideal, particularly in the social sciences. One possible reason may have been that the position taken on the correct form of a theory by logical positivist philosophers of science (based on a post hoc, rational rather than empirical, reconstruction of the results of scientific thinking and research) made little contact with scientists' own modes of operation and exposition (cf. Rubinstein, Laughlin, &

6 Care must be exercised concerning the way this line of work understands the term 'model': It is used

> in the sense of a thing depicted by a picture (= by a theory)...e.g., when one says that a woman is the model of a painting. Here, the model is the person depicted and the painting is the picture of it... our use of "model" is consistent with this artistic usage. (Balzer, Moulines, & Sneed, 1987, p. 2)

McManus, 1984; Suchman, 1988), preventing any corrective feedback loop from operating. Scientists' pronouncements on correct methodology might sometimes reflect those of philosophers of science, but there is and was no guarantee that their actions do so (Kantorovitch, 1988).[7]

Recent major developments within philosophy of science have responded to these sorts of considerations by focusing on the social and in particular psychological aspects of scientific theories. This has been referred to as the naturalization of philosophy of science (Giere, 1985) which has brought this area of investigation closer to empirical rather than rational studies, and to what scientific practice actually is, rather than a reconstruction of what it ought to be. The development of sociology of science (e.g., Latour & Woolgar, 1979) and particularly psychology of science (e.g., Gholson, Shadish, Neimeyer & Houts, 1989) following the historical work of Kuhn (1962) has provided insight into knowledge creation as a cognitive process in which theories are working instruments. In this context, a theory is conceived of as a 'cognitive object', that is to say, something primarily instantiated in the mind (e.g., Giere, 1988). As such it precedes its various possible linguistic or logical formulations, and reflects also the fact that the human mind in general and the scientist in particular make extensive use of analogical and iconic models when explaining, conceptualizing, and attempting to solve problems (e.g., Callebaut & Pinxton, 1987; Clement, 1989; Darden, 1983; Harré, 1960; Hesse, 1963; Leary, 1990). A theory in this tradition is seen as "a statement-picture complex" (Harré, 1970, p. 56) of which the pictorial element relates to the model (or the hypothetical mechanisms; Harré, 1970, p. 54) involved in the theory, and the statements relate to generalizations the theory supports (Giere, 1988; Harré, 1970, 1985a, 1985b, 1986; Suppe, 1974). This perspective on theory format is motivated not by a desire to clean up and clarify the work of scientists, but rather to adequately express what scientists are presenting in their theories. In particular, a new element is added to the list of components implied in the Received View — besides the statements of the earlier view (which no longer

7 Einstein is often cited to this effect, and Suppe (1974), referring to an important symposium on the structure of scientific theories occuring in 1969, notes that many scientists attending recognized the relevance of the discussions to their own practice, but were unable to understand them because of their lack of background!

need be deductively related), we now have in addition the model, which is needed for explanatory purposes.

The failure of traditional ideas in philosophy of science to link up with the actuality of scientific practice may be interpreted to mean they are not adequate bases for critiquing theories in science generally. Cognitive science, as we have seen, has in any case pursued its own idiosyncratic course Now that philosophy of science has caught up with what scientists actually do, does it have anything to offer SL researchers faced with problems of theory assessment? To answer this question, I move from simply describing possible and actual theory forms and elements in historical sequence, to considering a basis for theory choice.

3. Theory structures assessed on the basis of explanatory adequacy

Over time, a number of different aims have been posited as likely to be achieved by the construction of theories, and such aims can be used as the basis for assessing theories typified by their content or structure. The most prominent purpose of a theory is to explain, and although explanation is not valued by all philosophers of science (e.g., van Fraassen, 1980) it seems to be agreed on as a prime criterion for most theories (Suppe, 1972) including SLA theories (e.g., Gregg, 1990; Long, 1990). What is meant by 'explain' has been the subject of extended dispute, however (cf. Kitcher & Salmon, 1989; Pitt, 1988). Two major kinds of explanation were in play during the development of cognitive science, neither particularly appropriate for the needs of SLA. First is "deductive nomological (D-N)" explanation, central to the Received View and recognized by logical positivists as the most desirable[8] (Hempel, 1966; Hempel & Oppenheim, 1948). In this type of explanation, to explain an individual fact we deduce it from one or more other such statements in conjunction with one or more generalizations of laws. (Brodbeck, 1968, p. 363)

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8 Other kinds of explanation were considered, especially in Hempel (1965), where notably, explanation by analogy in science and in scientific theories is reviewed and dismissed, because it is part of the "pragmatic"aspects of explanation, and does not contribute to the logical analysis of explanation.

The fact in question is in almost every case an event or a regular occurrence of events. For example, to explain an occurrence of water freezing, the Received View requires a set of initial conditions (the water in the beaker was cooled to 0 degrees Celcius), a law (water freezes at 0 degrees Celsius), and the event (the liquid water became solid ice) is thereby "explained". To refer to a more large-scale example, this is also the kind of explanation presented by the (Newtonian) laws of mechanics—the most paradigmatic theory in science, and one often presented as a formal, axiomatic, hypothetico-deductive system.

The second kind of explanation is systematic, or functional explanation. In this type of explanation

> the explanandum [that which is to be explained] of a systematic explanation is always a capacity, as opposed to an event or regularity (the typical explananda of deductive-nomological explanations). The capacity is explained systematically if the thing whose capacity it is is analyzed as a set of interacting components whose individual capacities and interactions together give rise to the capacity being explained...

It is desirable to juxtapose these two types of explanation. The former appear in 'transition theories', the latter occur typically in 'property theories' (Cummins, 1983). Transition theories attempt to explain the changes in states of systems; that is, they take events as the phenomena to be explained, and they do so by subsuming events under laws. Property theories, by contrast, are concerned only with describing the systems themselves and are analyses of static systems—primarily ways of representing competences or bodies of knowledge. They thus have a different function and orientation to the typical "hard science" theory, which is a transition theory.[9]

[9] Garfield goes on to point out the layered nature of an extensive theory in cognitive science, observing that

> explanations may ... become ... deductive-nomological at lower levels, as the capacities or regularities of the interacting components into which the original system is decomposed by the original explanation are themselves explained. (Garfield, 1988, pp. 26-7)

This may explain past confusion concerning the intent and format of both part of linguistic theory (i.e., grammars, as discussed earlier) and also of computational theory formats in

Although cognitive science now has transition theories (cognitive learning theory, e.g., Anderson; learnability theory, e.g., Pinker, 1984; and of course any SLA theory) the early development of cognitive science was characterized by the development of competence theories embodying systematic explanations—for example, cognitive information-processing theories, or theories of linguistic competence (cf. Bialystok, 1990). At that time and until recently, ideas in philosophy of science concerning theory construction almost exclusively referred to and drew on theories in physical science. It may be that theory construction in cognitive science was hindered by the fact that philosophy of science concepts of theory construction have until recently been based almost exclusively on a type of theory and a kind of explanation different to that which cognitive scientists were actually trying to construct.

Obviously, property theories, though important in themselves, are not sufficient for studies of development or acquisition. Though a theory of the development of a system (such as the development of the ability to communicate in a second language) presumably should deal with the system's states, it must primarily explain transitions between states. It is on the grounds of adequacy of such explanations that traditional transition theories, that is, hypothetico-deductive theories of the Received View type, have been criticized. For example, many would argue that a better explanation of the instance of water freezing presented earlier would first state the composition of water (composed of particles) and then refer to how a disordered group of particles can be put in order by aligning them in two and three dimensions, in ranks and files, or in a grid or latticework, and that this is only possible when the particles can stick to each other and are not in motion. In constructing such an explanation appeal is made to our existing knowledge of the physical world (models) and specifically conditions and a mechanism for one model (a mess) to change into another (an arrangement). To again take a more general case, Newtonian mechanics, because of its paradigmatic status mentioned earlier, has been subjected to the sort of criticism implied in this example. The main charge leveled against it is that it is primarily descriptive rather than explanatory, since it does not present the mechanisms by which, for example, a

cognitive science.

particular path of motion is achieved by a moving body (Harré, 1985a, p. 170; cf. also Giere, 1988e).

Transition theories reflect the weaknesses of philosophical investigations of explanation (e.g., Achinstein, 1983; cf. Pitt, 1988, van Fraassen, 1980) which have concentrated on the explanations of particular events. This is another example of the Received View in philosophy of science failing to connect with actual science, since (according to Cummins, 1983, and Kim, 1973) this is not the sort of explanation scientists expect from a scientific theory. Giere (1988, and cf. Woodward, 1979) argues that

> a large part of any cognitive theory of explanation would be an account of how people deploy various sorts of schemata in giving explanations... "scientific" explanations... deploy models developed in the sciences. (Giere, 1988, p. 105)

This brings us to more recent developments in philosophy of science's understanding of both theories and explanations, facilitated by the naturalization of philosophy of science mentioned earlier, and to theory structures more useful for SLA.

It has been suggested that in order to understand scientific explanations (and thus construct adequately explanatory theories), we should investigate in detail the resources for scientific explanations, which in Giere's view are "sets of well-authenticated models" (1985, p. 105). Here he is at one with Harré (though neither Harré nor Giere refer to each others' work), who holds that real-world explanations of events are most complete when they occur through the exposition of the mechanism which connects events—that is, through the analysis of the structure of a model[10] analogous to the system being researched. Since the basis for this kind of explanation (according to Harré, 1960, 1970) is analogy,[11] we may expect to find analogies (explicit or implicit)

[10] In his sketch of a cognitive theory of science, Giere has argued that a theory is composed of "(1) a population of models, and (2) various hypotheses linking those models with systems in the real world (1988, p. 85)". This use of 'model' refers to a paradigmatic abstraction from a real-world system, in which central concepts of a theory apply, and which in turn can be used as the basis for understanding and solving problems pertaining to related realworld systems. (Cf. also Giere, 1979, p. 69, for his earlier definition (and helpful supporting discussion) of theory as simply "a kind of natural system".

in fully explanatory scientific theories. This is because the mechanisms sought as the basis for explanation do not easily reveal themselves, but must be inferred. In the process of developing a theory, as data then generalizations accumulate, the underlying structure of phenomena and the causal relationships between events must be constructed. Only by building on preexisting knowledge of similar structures to be found elsewhere can this be done—that is, by a process of analogy.

Analogical relations hold between the system under investigation, including its differing states at different times, and a model of this research object. The source of the model will be different from the system being modelled (unlike the case of a model airplane), because the thing to be modelled is at least in part unknown. Harré (1970, and cf. 1985a, b) terms such models 'paramorphs', and identifies three types in which the analogy is not precise, and thus conceptually productive (pp. 44–5):

> I distinguish singly connected, multiply connected and semi-connected paramorphs... the corpuscular theory of gases is singly connected, because the principles of only one science, mechanics [apply]... [An early model of the atom] is multiply connected [since it] draws on the sciences of mechanics and electromagnetism... Freud's

[11] Early proponents of this view (listed and dismissed in Hempel, 1965) were regarded as unorthodox or misguided by the Recieved View, but the philosophical line associated (by Bhaskar, 1975) with Scriven, Hanson, Hesse and Harré, along with recent psychologically-oriented investigations have renewed understanding of metaphor, analogy, and model in scientific thinking (cf. Gentner, 1982; Lashchyk, 1986; Leary, 1990). These closely related concepts need distinguishing:

The relationship of model and metaphor is this: if we use the image of a fluid to explicate the supposed action of the electrical energy, we say that the fluid is functioning as a model for our conception of the nature of electricity. If however, we then go on to speak of the 'rate of flow' of an 'electrical current', we are using metaphorical language based on the fluid model. (Martin & Harré, 1982, p. 100)

The connection between model and mechanism is extremely close. Harré (1970) distinguishes between cases where the application of model to research object is by way of a "causal transform" and where it is by way of a "modal transform". The latter posits a question like "Is gas temperature really only another way of looking at mean kinetic energy of the molecules?"; the former would be "Is gas pressure caused by the impact of molecules?". He notes that under a causal transform the iconic model "can come to be looked at as a hypothetical mechanism" (p. 54).

> 'psychic energy' mind model is semi-connected because [it draws on] some principles of energetics from physics [but] also introduces processes occurring according to principles unknown to energetics, or any other science... Sometimes semi-connected paramorphs are just what give us a new scientific development, by suggesting the idea of a new kind of entity, or process.

In addition

> is by being associated with a paramorphic model... that many laws of nature get their additional strength of connection among the predicates they associate, that distinguishes them from accidental generalization. A scientific explanation of a process or pattern among phenomena is provided by a theory constructed in this way. (Harré, 1970, pp. 46–7)

So the argument is: Good theories are those which provide the fullest explanations, which they do through providing a model of a system and (if they are transition theories) mechanisms depicting the movement of systems from one state to another. Concisely, a theory necessarily has two parts: (1) a pictorial part, or iconic model, and (2) some associated sentences which refer to the regularities it supports.

4. Implications for SLA theory

These recent developments in philosophy of science concerning the internal and external character of a theory naturally have implications for SLA. First, the criticisms of "logicism"[12] eliminate any suggestion that an SLA theory consist solely of propositions or hypotheses connected by deductive logic (the Received View position). Second, the conception of model and associated mechanism as essential parts of a theory (the line associated with Harré) implies that their role in existing conceptual proposals in SLA theoretical work should be investigated. At the same time it is possible to use this

[12] The assertion that scientific investigation proceeds best according to deductive logic, and should result in theories constructed of propositions connected by deductive logic (Kantorovitch, 1988).

understanding of the concept of model, and particularly of the role of mechanism in providing psychologically-satisfying scientific explanations to identify promising aspects (and weaknesses) of current SLA thought. Finally, developments in the axiomatization of theories can speak to the topic of formalism in SLA theory.

4.1 Models in existing SLA theories

In the present formulation, I take 'model' to refer to the central metaphor or explanatory analogy utilized by a theory. In the case of the second language learning system embodied in human cognition, the model provided is often that of another human attribute, or a central cognitive process known to exist in human cognition but not yet applied to the case of SLA. Models in SLA are typically not explicitly presented and are often not clearly developed. This may be because investigators have not fully recognized their utility and legitimacy. I will simply mention two—one being that of Krashen, as he is the paradigmatic "theorist" of SLA, the other that of Ellis, since he actually uses the term 'model'.

One model underlying Krashen's theory is that of a ladder (cf. also Kellerman's 1984 use of the term). When data are observed to show temporal discontinuities associated with discrete improvements in competence, subsuming them under stages is the first step in theorizing. This says little more than that we presume that progress in this case requires the accumulation (or possibly loss) in a step-by-step fashion of elements which are sequentially arranged in a series of prerequisites. The metaphor is so familiar to us that defining it seems banal, but its explanatory value is seen if the question 'Why can't the speaker say that yet?' is answered by saying 'That is a stage 5 structure and the speaker is at stage 2'. Yet this obviously fails to answer the question of how a learner moves from one stage to the next—something that can only be answered by considering the explanatory mechanisms implied in the theory.

It might be thought that the basic model which Krashen provides to explain how an individual progresses in SL learning is "the learner as sponge"—but in fact the main model provided is that of the child. For

Krashen, it is the learner's continuing ability to access the language acquisition device (LAD) that Chomsky proposed as part of the child's innately-endowed cognitive capacities which enables a learner to acquire a SL without conscious attention to the form of input. However, the extent to which Krashen understands or utilizes this concept in its full complexity is disputable (cf. Gregg, 1988). If he were saying adults have access to the LAD, i.e., that L2 learning is the same as L1 learning, this would be an explanation of a sort. If he were arguing, say, that L2 learning makes some use of a LAD with specified modification, this would be possibly more interesting, and indeed typical of the sort of partial analogy (e.g., Harré's semi-connected paramorph) discussed earlier as a fruitful aspect of scientific theorizing. Regrettably, even his more detailed presentations (e.g., Krashen, 1983) do not provide a clearly worked out position, as Gregg (1988) makes plain.

Ellis names his depiction of SLA the 'Variable Competence Model' (e.g., Ellis, 1985a). The (sketchy) explanatory mechanisms it posits follow from the modelling of the second language learning system on a traditional cognitive information-processing model with short and long-term memory stores, within which skill-learning occurs via automatization through use. In Ellis's model, the learner moves linguistic knowledge, or rules, from one store or condition to another, with changes in their associated accessibility, both through operating on input and through producing output. Although the theory is rather vague (among other problems, cf. Gregg, 1990), it gains plausibility because of its similarity to a system that many SL investigators would accept as fairly well established.. Future work using Ellis's concepts could develop by drawing more heavily on the potentially explanatory metaphors inherent in the relationship that elements of the model have to their apparent sources in the cognitive information-processing model of human cognition and in production system models of human skill learning.

4.2 Mechanisms in existing SLA theories

The function of a mechanism in a theory of language acquisition is to show how the transition from a representational system at ti to a representational system at ti+1 is effected. In order to do that it is necessary to specify some interaction between input, cognitive procedures and a representational system at ti whose product is a representational system at ti+1. "... The cognitive procedures [used so far in research have been]... 'general cognitive strategies' or ad hoc 'acquisition devices'." (McShane, 1987, p. 115)

McShane subsequently gives some more explicit descriptions of mechanisms to be found in early L1 learnability literature (Atkinson, 1982): association, differentiation, generalization, and hypothesis testing constrained by innate principles (see also MacWhinney, 1987: a more recent L1 collection solely on this topic.) However, constructors of SL theory have yet to catch up with these ideas, as shown in the most recent summary work on SL theory (Spolsky, 1989). In traditional terms, this is simply a set of laws—"I use the term theory to mean a hypothesis or set of hypotheses that has been or can be verified empirically" (Spolsky, 1989, p. 2)—and no mechanisms are provided on which to base the understanding of the processes of SLA that his work is presumably intended to provide. Generally, mechanisms in SLA theories "tend to be rather vaguely defined and poorly supported" (Long, 1990, p. 654). An overview of mechanisms utilized in recent SL theories is given in Larsen-Freeman and Long's (1991) review of SL theory, which they divide into three representative categories—nativist, environmentalist, and interactionist. (The following section draws heavily on this review.)

The first of these groups ("nativist") are those which refer to innate characteristics of the part of the human cognitive system which is specialized for language acquisition (the LAD), and include the work of Krashen (Monitor Theory, or MT). Larsen-Freeman and Long comment that

> MT offers no explanation for the morpheme orders on which many of its claims are based and supposedly tested, nor for any other developmental sequences... [rather, it] appeals to Chomskyan UG to explain acquisition.(p. 248)

Associated with UG, "learnability theory" (e.g., Wexler & Cullicover, 1980; cf. O'Grady, 1987), specifies important mechanisms (mentioned above) which interact with the LAD (or constitute it) —none of them are referred to explicitly by Krashen, however.[13]

[13] His (1983) paper comes the closest, with the "notice the gap" device.

Environmentalist theories are exemplified in the work of Schumann and Andersen. The absence of explanatory mechanisms in Schumann's work that Larsen-Freeman and Long find may be connected with the fact that Schumann's theory is close to a "factor model" (van Geert, 1990), in which success in SL learning is attributed to several factors (e.g., aptitude, accommodation, integrative motivation, etc.) and scores on some measure of SL achievement are related to such factors in terms of variance explained.

The problem with a factor model, however, is that it actually represents an empirical generalization.... the theory does not provide a description of a mechanism in the real world, of real world inputs to that mechanism, and of real world outcomes. There is no mechanism in the world out there that takes as its input a value of three factors, and that produces a specific cognitive achievement level as its output. There is a mechanism, though, as far as cognitive achievement is concerned. It is an information processing mechanism, processing input information on the basis of production rules and representations stored in memory. Any explanatory model of cognitive achievement should consist of a model of this information processing mechanism. It is this explanatory model—and not the hidden descriptive factor model—that should generate our predictions of future cognitive achievement, skills, and knowledge. (van Geert, 1990, p. 194). Schumann's recent work appears to reflect an awareness of this sort of weakness (cf. Schumann, 1990).

In Andersen's (1979, 1983) work, the concept of "Nativization" may imply an explanatory mechanism, however. Nativization

> refers to the learner's tendency to make new input conform to his or her internal norm or mental picture of what the L2 grammar is like. It involves assimilation of new knowledge to old (in the shape of knowledge of the L1 and pragmatics) through hypothesis formation and application of cognitive processing principles like Slobin's (1973) operating principles... Denativization on the other hand guides depidginization and later stages of first and second language acquisition.(p. 265)

Andersen explicitly uses an existing model—the learner as pidgin creator—and applies its associated mechanisms to a new situation, that of L2 learning.

By interactionist, Larsen-Freeman and Long mean theories which utilize the interaction between environment and innate aspects of cognition in explaining SLA, such as that of the ZISA group (e.g., Clahsen, 1984; Pienemann, 1987), which takes the form of a stage theory. The most well-known stage theory in the social sciences is probably Piaget's, concerning child development. Children utilize a series of "schemes": cognitive structures of some sort, which are modified through experience with input data. Developments of this concept (e.g. Case, 1978, and cf. Flavell, 1982) have utilized, for example, known growth in cognitive capacities, specifically the child's short-term memory capacity, to explain stages in cognitive functioning. The mechanism Pienemann and his colleagues have posited to explain the observed sequence of stages in SL learners' acquisition of syntax in a variety of second languages

> consists in the shedding of [processing] strategies, or the gradual removal of the constraints they impose on what is processable ... the complexity of a structure is determined by the type of reordering and rearrangement of constituents necessary to map underlying meaning on to surface form. (Larsen-Freeman & Long, 1991, p. 272)

The model also attempts to connect "contextual" social-psychological factors with cognitive factors, specifically variation in degree of use of simplification and processing strategies. It thus demonstrates the possibility of tying more distal variables (such as those posited by Schumann) to mechanisms which because they are of the same nature as the learning process itself are inherently more explanatory than e.g. social forces. However, unlike other stage theories (e.g., Piagetian theories, or those of social functioning such as Kohlberg's) the model does not require the gradual accumulation of a series of strategies.

Both associationist and production system strands of computational theory format are beginning to appear in discussions of SLA. For example, Anderson's ACT* production system model of skill learning is appealed to directly by O'Malley, Chamot, and Walker (1987). Its major explanatory mechanism of learning is "composition"—the collapsing of separate steps of processing as a given production system is used repeatedly. McLaughlin (1990; cf. Lightbown, 1985) feels that an additional mechanism, "restructuring", is needed, which refers to the transition from representations of language as whole units in memory to more abstract, rule-like representations. (See Schmidt, to appear, for discussion.) The generality of the concept allows it to be used to refer to almost any cases of sudden movement of ILs toward (or occasionally away from) the target language. A number of other learning mechanisms are utilized in production system models (e. g., proceduralization, discrimination, generalization—Neches, Langley, & Klahr, 1987; cf. vanLehn, 1990), though no SLA theorists have yet to avail themselves of the large range of possibilities appearing in this area.

Connectionist approaches are discussed in an SLA context by Gasser (1990) and Sokolik (1990). The importance of mechanism and model explains one major attraction of connectionism —its explicit use of models related (albeit distantly, cf. Loritz, to appear) to neural networks. The associated mechanism in this case is the strengthening or weakening of tendencies for simple processing units to stimulate or inhibit others. It is notable that many reports in this paradigm do not merely imply an iconic model, but actually present diagrammatic representations of the network being proposed in the course of the exposition (e.g., Dell, 1989; Stemberger, 1985).

I have cited various authorities on the point that theories, even in the physical sciences, are rarely stated formally, almost never as hypothetico-deductive systems; but on the other hand I have said this may not be too bad since a formally-stated hypothetico-deductive system alone probably does not deliver what we expect of a theory: a satisfying explanation. That, I have alleged (with Long, and others), is the responsibility of the model(s) and/or mechanism(s) associated with the theoretical system in question. In the immediately preceding section, I have supported the critical remarks of Atkinson for L1A and Long for SLA concerning the lack of clarity of mechanisms advanced by theorists of language acquisition. Following Harré, for explanatory purposes a theory must consist of two elements: (a) statements and (b) models or mechanisms. Even though we may recognize that a theory is a cognitive object, if it is to be utilized and communicated it must be embodied (even if variously) in a maximally clear fashion. The question then becomes, how can this clarity be achieved?

4.3 Formalisms in SLA theory

Though not all theories may be susceptible to formalization,[14] many scientists as well as philosophers of science would argue that formalizing a theory, or stating a theory formally, is desirable, for the sort of reasons advanced by Suppes (1960, p. 296):

> The attempt to characterize exactly models of an empirical theory almost inevitably yields a more precise and clearer understanding of the exact character of the theory. The emptiness and shallowness of many classical theories in the social sciences is well brought out by the attempt to formulate in any exact fashion what constitutes a model of the theory. The kind of theory which mainly consists of insightful remarks and heuristic slogans will not be amenable to this treatment. The effort to make it exact will at the same time reveal the weakness of the theory.

Given the paucity of discussions of the methodology of SLA theory construction, it is not surprising that there has been little consideration of this issue. An exception is Gregg (1989, p. 30), who remarks that "formalisms, in short, are Good Things". Gregg accepts Wexler & Cullicover's (1980) assertion that "a sufficiently precise theory" of what is to be learned is "a prerequisite for creating or evaluating" a learning theory, and calls for "a well-articulated formal characterization of the domain" (Gregg, 1989, p. 24). This refers to only the lowest of three possible levels to which the concept of formalism can apply for an acquisition theory (if the distinction between competence and

[14] Scientifically accepted theories exist which are unformalizable, since their central concepts have "contingent features ... not deducible from some set of first principles" (Harré, 1985, p. 181).
For Harré a case in point is the virus theory of disease. and also cites social psychology as a general area unlikely to be axiomatizable; molecular biology (Culp & Kitcher, 1989) has also been labelled thus.

performance is accepted). The lowest level of formalism concerns the use of a formal linguistic theory, whose formalism might well be syntactic symbols[15] Second, the system which acts on the learner's knowledge (the learning procedure) can also be represented in a formalism. For example, Pinker (1987) uses a production system model, which, though designed with the linguistic formalism of lexical-functional grammar in mind, is intended to work with other types of grammar too. (This is obviously desirable since applied linguistics is strewn with attempts to use linguistic theories which have rapidly been discarded by their originators) Finally, the entire structure may be represented formally, as Wexler and Culicover's (1980) theory is. They present their theory as a set-theory predicate; the overall formalism which they use for their acquisition theory is Model-Theoretic. As they state (1980, p. 31): "a theory of (first) language acquisition may be looked on as a triple <G, I, LP>", where G is the grammar (level one just mentioned, I the input data, which can also be specified formally in the same formal language as the grammar), and LP is the learning procedure.

Formalisms are, of course, not sufficient for successful theory construction In his analysis of the successful explanation of phenomena in the interactions of atomic nuclei and neutrons, Cushing (1989) indicates that the investigations proceeded without difficulty partly because (1) a clear (though not complete) analogy existed between an existing system (the interactions of photons and electrons) and the system of interest; and (2) partly because the earlier work provided "a language with which to discuss, organize and interpret more data" (p. 17). Other generally accepted principles were then used to further support the explanations offered:

> A tightly knit interplay among experiment, theory, and general beliefs ... cement[ed] this model in to a stable, accepted configuration. (Cushing, 1989, p. 17)

The language, or formalism, had not been sufficient on its own, however. Cushing concludes that

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15 Some conceptual confusion here may be engendered by theorists who refer to the need for a sequence of theories as a way of describing L1 development (e.g., Atkinson, 1982).

> [s]uccessful theories are made to work; they don't just work on their own or because nature demands it. Once we are inside a formalism... we may feel that its own internal logic seems compelling... within the framework of the present, 'correct' theory. An essential aspect overlooked by such an approach is how one buys into the starting assumptions of the formalism." (p. 18)

This is a suitably balanced judgement to be applied to the question of formalisms in SLA theory and research. If the SLA field is to couch its theories in formal terms at the highest level of abstraction then there is no practical alternative to set theory. Adopting set-theory formalism should facilitate the utilization of analogies to other structures since it is maximally applicable across science (and philosophy of science). At the next level down, at least two major formal languages are available for the computational modeling of language acquisition, and although some authorities see them as conceptually incompatible, a number of partial models of language production, at least, use elements or concepts associated with both. Finally, if the field is to use a formal linguistic theory because an acquisition theory requires an associated 'clearly expounded' descriptive theory, then the choice of (formal) linguistic theory should be informed by considerations such as whether the descriptive theory in question lends itself to use in an acquisition context, and should not be associated with a single competence theory.

A general objection to the advocacy or use of formalisms is often mentioned—that it prevents work from being accessible. It seems however, that within the limitations of human cognitive and social systems for knowledge transmission and processing we are inevitably faced with a trade-off of reliability and validity against accessibility. At the same time, if high school teachers of science do not hesitate to utilize mathematical formalisms to present the results of Newtonian physics to children, perhaps social science professionals should not shrink from acquiring the tools necessary to adequately handle the subject matter they specialize in. In addition, so long as a productive heuristic for science is the borrowing of models and languages from related fields, it will be those who do not have the formalisms who will be hampered.

5. Conclusion

A major concern of this paper is to facilitate the construction and understanding of theories in SLA. If we are going to theorize, we need to be clear why we perform this act, and how it can be done to best achieve our goal. Informed action is crucial—that is to say, we must have understanding of our actions at one level above them: a meta-awareness. Applied linguistics has been criticized by many, and rightly, for always looking inward, and rarely to its neighboring disciplines. It did not occur to those critics that we also need to look up, at the superordinate disciplines which constitute the science of science. In this paper, like an increasing number of other SL investigators, I have attempted to redress this previous weakness, so that, simply put, we can see what we're doing.

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