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THEORY FORMAT AND SLA THEORY

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This paper reviews work in the philosophy of science pertinent to theory formats and relates it to recent second language acquisition (SLA) theories. Recent developments in philosophy of science and science studies have shown that the theory format advocated by philosophy of science for much of this century is unhelpful; developments in theory format in cognitive science, particularly psychology, artificial intelligence, and linguistics, have proceeded on lines independent of the older tradition in theory format. The naturalization of philosophy of science has resulted in improved understandings of what is necessary in a theory if it is to be adequately explanatory. SLA theory development has largely taken place in ignorance of such recent developments, and initial critiques of SLA theory from within the field reflected the earlier conception of theory. However, SLA research has reached the stage where a meta-understanding of theory formats, in terms of the components of a theory and the language in which a theory is couched (its formalism), is badly needed to facilitate theory development.

Within applied linguistics, calls for a theory of second language (SL) learning or second language acquisition (SLA) have become increasingly common since the 1970s, and a number of extended position statements have been produced (e.g., Krashen, 1981, 1982, 1985; Schumann, 1978; Spolsky, 1989; cf. Beretta, 1991; McLaughlin, 1987). We have also begun to see some of these statements 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

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theoretical work can be judged. Most SL theoretical efforts, however, have not yet benefited from a concern for such matters or a knowledge of the relevant literature. Consideration of foundational matters, such as the target of the SL research program and methods to attain it (cf. Chaudron, 1986), are, nevertheless, essential to the development of the field. In this paper, I review conceptions of theory most relevant to SLA with particular reference to their components, such as hypotheses, and how these are arranged (logically related, or otherwise) and to their surface characteristics (whether or not theories are primarily instantiated in logical symbols, computer code, grammatical symbols, thought, and so on). I then relate these ideas to some current SLA theories.

At the outset, I should recognize that there is a definitional problem. I will refer to the components of a theory, their arrangement, and their appearance in toto as *theory format.* What is meant, however, by the term theory? In SLA, theoretical proposals have been variously labeled theory, theoretical model, and conceptual framework, with little concern for the distinctions that can be made among these terms (which are discussed in Giere, 1979; cf. Brodbeck, 1959; Youngquist, 1971). Similarly, across science in general many meanings have been accepted for the single term theory because of the differing orientations of the various domains of science (see, e.g., Kantorovitch, 1988, and for applied linguistics, Stern, 1983), together with the uncomfortable coexistence of persistent older understandings of the term in some disciplines and newer senses established in other disciplines. We have to recognize that "like any historical entity, science evolves over time and its characteristics at any given time are a product of its (contingent) past and of the environment it finds itself in at present" (Cushing, 1989, p. 7). In addition, the tacit understanding of the term theory by scientists has not always agreed with the meaning given to 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. Consequently, a definition of theory will not be presented here, but it will (I hope) emerge in the course of the discussion.

I will initially lay out conceptions of theory format, separating them by historical sequence and to some extent by discipline. In the subsequent section, I will present an overview of theory formats with reference to a key function of theory, explanation. In the final major section of the paper, I will show how this determiner of theory structure should aid assessment of past work in SLA theory and guide future SL theorizing; I also discuss the role formalisms may have in future SL theories.

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 their 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 in philosophy of science ideas about theory format that are applicable to SLA theories.

The *Received View:* The Traditional Philosophy of Science Perspective on Theory Format

Concerning what a theory should look like, according to Giere (1985), "since Euclid there has existed a more or less continuous tradition of representing theoretical knowledge in the form of an axiomatic system" (p. 344). However, until around the turn of the present century, there was little explicit direction as to what form a theory should take. Ideas about theory format became more prescriptive with the development of the philosophical movement of the nineteenth century known as positivism. Positivists (e.g., Mach, 1886/1957, cited in Robinson, 1985) agreed with earlier scientists that theories should consist of statements of "general facts," that is, laws, but required additionally that theories be in no way "metaphysical," but contain only empirically defined observational terms. In the subsequent development of positivism into *logical* positivism, implementing this requirement was seen as possible through the application of the newly developed field of logic, first to physics (e.g., Frege, 1893/1964; for historical review, see, e.g., Suppe, 1974) and then 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, 1985b, p. 53). In this conception, the components of a theory are sentences, which represent a set of empirical laws. Centrally, certain sentences are identified as generalizations, from which hypotheses can be deduced. The theory is therefore *hypothetico-deductive.* The theory should be formalized, taking the form of logical propositions expressed in first-order predicate calculus (one of many possible logics). Casting a theory in this form is axiomatization, and the theory itself has been referred to as an axiomatic theory (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). This position held sway until around the 1960s.

Cognitive Science Practice and Theory Format

Theory formats in cognitive science[1] have rarely fully conformed to received view prescriptions. Consider the work of Hull (e.g., Hull et al., 1940), one of the few in psychology to have direct contact with prominent figures of the logical positivist movement, and apparently the one major psychologist concerned to establish an axiomatic hypothetico-deductive theory. In a systematic comparative analysis of early learning theories (Estes et al., 1954), his efforts with regard to "explicitness of axiomatization" were evaluated highest of the five surveyed. These efforts, however, failed to satisfy the standards of his commentator (Koch, 1954), who observed 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" (p. 59). Hull's attempts to develop learning theory upon logical positivist principles were regarded as thoroughly misguided by members of this school (Addis, 1989; Smith, 1986). In Koch (1959), the other major comparative survey of psychology conducted in this period of received view dominance, theorists clearly had mixed feelings about whether or not axiomatization was desirable. 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) and questioned the utility of theories of any kind (Skinner, 1950), but this antitheoretical 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 the then standard view of scientific theory (because of its earlier association with anthropology) and influenced by its role in attacks on the preceding antitheoretical trend in psychology, mainstream linguistic theory (and because of the influence of linguistics, cognitive science generally) does not represent a smooth development of received view conceptions of theory.

Theory Formats in Linguistics. Modern theoretical linguistics primarily results from the development of Chomsky's ideas about language and has been characterized by the development of a family of formalisms such as 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 among grammars, linguistic theory, and mainstream scientific theory was not clear. Sanders (1974) 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.[2] (p. 3)

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)

> Describing the language [is] attempting to construct a grammar of a particular language, that is, a theory of that language. (Chomsky, 1988, p. 61)

Postal (1964) 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)" (p. 3), a linguistic theory, by distinction, is to be a general account of common features of grammars and must contain the following:

> 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, pp. 3–4)

The position that linguistic theories were formal symbol systems that generated sentences allowed the perception that linguistic theories were closest in format to other theories in cognitive science (e.g., those in artificial intelligence) as opposed to those in the hard sciences. However, in the last decade, theoretical linguists, particularly those associated with generative linguistics (cf. Chomsky, 1982a, 1982b; Gazdar, Klein, Pullum, & Sag, 1985), have investigated specific grammars mainly for the light they throw on general characteristics of human knowledge of language (Universal Grammar), so it may be more accurate to say now that linguistic theories (as opposed to grammars) primarily consist of general statements (or principles) from which, together with specific grammars, can be deduced hypotheses about language competence (indirectly testable via performance data; cf. Chomsky, 1988, p. 61).[3]

Psychology: Computational Theory Formats. Elsewhere in cognitive science, specifically in psychology and artificial intelligence, the position developed that theories of human cognition can desirably be set out as computer programs that were initially conceived of as making use of production system models (Newell, 1967; Newell & Simon, 1963) typified by pairs of If-Then statements.[4] Most generally, the position was 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).

More recently, an alternative format for computational theories has developed, in which the theory is embodied in computationally implemented models of cognition labeled associationist, connectionist, or parallel distributed processing (PDP). These have burgeoned rapidly since the mid-1980s (Schmidt, 1988) and are seen by some (e.g., Pinker & Prince, 1987) as representing "an intermediate level between symbol manipulation and neural hardware" (Schmidt, 1988, p. 59).[5] 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 nor 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" (Rumelhart & McClelland, 1986, p. 267, cited in Schmidt, 1988; see also Sokolik, 1990).

These two formats are not seen as antithetical by PDP proponents (Hinton, McClelland, & Rumelhart, 1986; Norman, 1987, p. 549; see discussion in Boden, 1988; Levelt, 1989). Indeed, hybrid models currently are being developed that utilize aspects of both parallel (associationist) and sequential (production system) types of computer modeling (cf. references in Klahr, Langley, & Neches, 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.

Though the theory as program position has been widely accepted, some criticisms have been voiced. 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 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 the programmed simulation of vision utilized in the respected work of Kosslyn (1980) was for Luce (1989) "simply one programmer's version of what he believed Kosslyn had in mind as was evolved from discussions and informed by repeated computer printouts" (pp. 126–127). Luce (1989) 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" (p. 127). 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, and Greeno (1984) remark that examples of the degree of fit and

associated argumentation rather than proof are the principal means of support for programs.[6] 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 [*sic*] own architecture and associated language" (Neches, Langley, & Klahr, 1987, p. 18).

Developments in Philosophy of Science Concerning Theory Format

Contemporaneous with the rise of cognitive science, philosophy of science underwent changes in many areas. From about 1950 onward, 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). Though dismissed in philosophy of science, it lived on as a somewhat misunderstood ideal in some areas of social science.

Some criticisms were leveled at the charge that theories should be expressed in first-order predicate calculus: "It is unheard of to find a substantive example of a theory actually worked out as a logical calculus in the writings of most philosophers of science" (Suppes, 1967, p. 56; cf. Bunge, 1973); 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" (Reynolds, 1971, p. 97). Because of the practical problems faced by attempts to use first-order predicate calculus as a tool for representing theories (Suppes, 1957, 1967), the semantic approach, or model-theoretic approach, was developed by Suppe (1972, 1989) among others (cf. Balzer, Moulines, & Sneed, 1987; DaCosta & French, 1990; Sneed, 1971, 1977; Stegmüller, 1973; Westmeyer, 1989). In this approach, theories became seen not as sets of sentences or propositions, but as "extralinguistic entities which may be described or characterized by a number of different linguistic formulations" (Suppe, 1974, p. 221); a theory constitutes a set of mathematical models[7] 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. Most axiomatization of theories that have been initially promulgated nonformally currently utilizes "semantic/model-theoretic methods" (DaCosta & French, 1990, p. 250, and cf. Westmeyer, 1989). This approach is a direct outgrowth of the logical positivist objective for philosophy of science of systematizing the conceptions of scientists.[8]

Other criticisms were more radical. One possible reason why theories were not presented as the received view advocated is 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, & 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).[9] Recent major developments within philosophy of science have responded to these considerations by focusing on the social and, in particular, psychological aspects of scientific theories, resulting in the "naturalization" of philosophy of science (Giere, 1985). Philosophy of science has come 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, 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; Gentner & Stevens, 1983; Harré, 1960; Hesse, 1963; Leary, 1990; Nersessian, 1988). A theory in this tradition takes the form of "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).[10] This perspective on theory format is motivated not by a desire to clean up and clarify the work of scientists but, rather, to express adequately 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 sentential statements of the earlier view (which no longer need be deductively related), we now also have 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 and construction? To answer this question, I move from simply describing possible and actual theory formats in historical sequence to considering a basis for choice of theory format.

EXPLANATION AND THEORY FORMAT

Over time, a number of different aims have been posited as likely to be achieved by the construction of theories, and such aims may be reflected in the forms that theories take or the forms that are advocated. 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, each connected to a kind of theory format and neither particularly appropriate for the needs of SLA.

First is deductive-nomological explanation, central to the received view and recognized by logical positivists as the most desirable (Hempel, 1966; Hempel & Oppenheim, 1948). This type of explanation typically explains a fact, for which it requires a law or generalization, along with a statement of conditions (Brodbeck, 1968). The fact in question is generally 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 °C), a law (water freezes at 0 °C), and the event (the liquid water became solid ice) is deduced and thereby explained by causal subsumption. 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 explanation by analysis (Cummins, 1983), of which a major, exemplary subtype is systematic explanation (see also Haugeland, 1978):

> 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. (Garfield, 1988, pp. 26–27)

For example, if the capacity of a computer to process information is explained by a delineation of what its component units do and how they interact, that is a systematic explanation.

The received view saw physical science as paradigmatic, and so expositions of explanation according to the received view tended to depict explanation in the physical sciences as deductive-nomological. This was plausible (though misleading) because physical science theories are often transition theories—they are concerned with the changes in states of systems, which may, of course, manifest as events. Transition theories can be contrasted with property theories, which are concerned with describing the systems themselves and are analyses of static systems—primarily ways of representing dispositions, competences, or bodies of knowledge.

Although cognitive science now has transition theories (cognitive learning theory [e.g., Anderson, 1982], applications of 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 analytic explanations—for example, cognitive information-processing theories, or theories of linguistic competence (cf. Bialystok, 1990; Smith, 1988). At that time and until recently, ideas in philosophy of science concerning theory construction largely referred to and drew on theories in physical science (particularly physics). Regrettably, 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 from that which cognitive scientists were actually trying to utilize.

Obviously, property theories, though important in themselves, are not sufficient for studies of development or acquisition.[11] Though a theory of the development of a system (such as the development of the ability to communicate in a SL) presumably should deal with the system's states, it must primarily explain transitions between states. It is on the grounds of the adequacy of such explanations that the received view position on explanation, which supports the desirability of the hypothetico-deductive theory format, has been criticized. For example, it can be argued 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 orient 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 Newtonian mechanics is that it is primarily descriptive rather than explanatory, because it does not present the *mechanisms* by which, for example, a particular path of motion is achieved by a moving body (Harré, 1985a, p. 170; cf. Giere, 1988).

Philosophy of science investigations of explanation (see, e.g., Achinstein, 1971, 1983; Pitt, 1988; van Fraassen, 1980; cf. Garfinkel, 1981, for a critique) 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, because (according to Cummins, 1983, and Kim, 1973) explanation of particular events by way of their causal subsumption under laws is not the sort of explanation scientists expect from a scientific theory. Considering what *scientists* might expect from an explanation is the sort of naturalistic move that would not have been valued by exponents of the received view but is typical of the newer naturalized philosophy of science, to which we now turn.

It has been suggested that to understand scientific explanations (and thus construct adequately explanatory theories), we should investigate in detail the resources for scientific explanations. According to Giere (1985), these are "sets of well-authenticated models" (p. 105). He observes (Giere, 1988; cf. Woodward, 1979) 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. . . . '[S]cientific' explanations . . . deploy models developed in the sciences" (Giere, 1988, p. 105). Here he is at one with Harré (though neither Harré nor Giere refers to the other's work), who holds that explanations of events are most complete when they occur through the exposition of the mechanism that connects events—that is, through the analysis of the structure of a model[12] partially analogous to the system being researched. Because the basis for this kind of explanation (according to Harré, 1960, 1970) is analogy,[13] we may expect to find analogies (explicit or implicit) in fully explanatory scientific theories. This is because the mechanisms sought as the basis for explanation do not easily reveal themselves and must be inferred. In the process of developing a theory, as data and 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 different states at different times, and a model of this research object. The source of the model will be different from the system being modeled (unlike the case of a model airplane), because the thing to be modeled is at least in part unknown. Harré (1970; cf. Harré, 1985a, 1985b) terms such models paramorphs and identifies three types in which the analogy is not precise and, thus, conceptually productive:

> 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 [because it] draws on the sciences of mechanics and electromagnetism. . . . Freud's "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. (pp. 44–45)

In addition:

> It 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. 46–47)

So the argument is: Good theories are those that 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 a system from one state to another. Concisely, a theory desirably is composed of two parts: (a) a pictorial part, or iconic model, and (b) some associated sentences that refer to the regularities it supports.[14]

IMPLICATIONS FOR SLA THEORY

These recent developments in philosophy of science concerning the character of a theory naturally have implications for SLA. First, the criticisms of logicism[15] 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, this understanding of the concept of model, and particularly of the role of mechanism in providing psychologically satisfying scientific explanations, can be used to identify promising aspects (and weaknesses) of current SLA thought. Finally, developments in the axiomatization of theories (the model–theoretic approach) as well as the increasing use of computational formats can speak to the topic of formalism in SLA theory.

Models in Existing SLA Theories

In the present formulation, I take model to refer to the central explanatory analogy utilized by a theory. In the case of the SL 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 (1985), because he is the paradigmatic "theorist" of SLA, the other that of Ellis (1985), because he actually uses the term model.

One model underlying Krashen's theory is that of a ladder (cf. 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 that 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.

One might think that the basic model that 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), which Chomsky (1980) proposed as part of the child's innately endowed cognitive capacities, that 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, that is, that SL learning is the same as first language learning, this would be an explanation of a sort. If he were arguing, say, that SL 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) and others' representations of them do not provide a clearly worked out position (Gregg, 1988, in press).

Ellis names his depiction of SLA the variable competence model (Ellis, 1985). The explanatory mechanisms it posits appear to follow from the modeling of the SL learning system on a traditional cognitive information-processing model with short- and long-term memory stores, within which skill learning occurs partly 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 explanatory force 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 models of human skill learning.

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 t_i to a representational system at t_{i+1} is effected. In order to do that it is necessary to specify some interaction between input, cognitive procedures and a representational system at t_i whose product is a representational system at t_{i+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 first language learnability literature (Atkinson, 1982): association, differentiation, generalization, and hypothesis testing constrained by innate principles (for a more recent first language collection solely on this topic, see also MacWhinney, 1987). 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). This work contains 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)—but no mechanisms are provided on which to base an understanding of the processes of SLA. Generally, mechanisms in SLA theories "tend to be rather vaguely defined and poorly supported" (Long, 1990, p. 654). Larsen-Freeman and Long (1991) provide an overview of mechanisms utilized in recent SLA theories divided into three representative categories: nativist, environmentalist, and interactionist. The following section draws heavily on their overview.

The first of these groups (nativist) refers to innate characteristics of the part of the human cognitive system that is specialized for language acquisition (the LAD) and includes the work of Krashen (e.g., 1985, monitor theory [MT]). Larsen-Freeman and Long (1991) 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 [Universal Grammar] to explain acquisition" (p. 248). UG-related acquisition theories usually appeal to "learnability theory" (e.g., Wexler & Culicover, 1980), which specifies mechanisms that interact with the LAD (or constitute it). However, neither this line of work nor the mechanisms it suggests are referred to explicitly by Krashen.[16]

Environmentalist theories are exemplified in the work of Schumann (e.g., 1978) and Andersen (1979, 1983). The absence of explanatory mechanisms in Schumann's (e.g., 1978) work that Larsen-Freeman and Long (1991) find may be connected to the fact that Schumann's theory is close to a factor model (van Geert, 1990; cf. Woodward, 1988), in which success in SL learning is attributed to several factors (e.g., aptitude, accommodation, integrative motivation) 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 reflects an awareness of this weakness (cf. Schumann, 1990, 1991) and considers the desirability for explanatory purposes of positing both potential cognitive and neurobiological mechanisms in SLA theories.

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. (Larsen-Freeman & Long, 1991, p. 265)

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

By interactionist, Larsen-Freeman and Long (1991) mean theories that utilize the interaction between environment and innate aspects of cognition in explaining SLA (e.g., Clahsen, 1984; Pienemann, 1987; cf. Klein, 1990, 1991), which take the form of a stage theory. The most well-known stage theory in the social sciences is probably that of Piaget (1983) 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) 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 SLs

> 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, e.g., 1978) to mechanisms that, because they are of the same nature as the learning process itself, are inherently more explanatory than, for example, social forces. However, unlike other stage theories (e.g., Fischer, 1980; Kohlberg, Levine, & Hewer, 1983; Piaget, 1983), the model does not require the gradual accumulation of a series of strategies.

More recent developments in SLA theory are also important to a consideration of mechanisms. Both connectionist and production system strands of computational theory format are beginning to appear in discussions of SLA.[17] For example, Anderson's (1982) 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, rulelike representations (for discussion, see Schmidt, this issue). 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 et al. 1987; cf. VanLehn, 1990), though 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, 1991) 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 noteworthy 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, 1986; Stemberger, 1985).

Formalisms in SLA Theory

Though not all theories may be susceptible of formalization,[18] scientists (e.g., Finke, 1989, p. 142; Hintzman, 1991) and philosophers of science have argued that formalizing a theory, or stating a theory formally, is desirable for the sort of reasons advanced by Suppes (1960):

> 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. (p. 296)

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), who remarks that "formalisms, in short, are Good Things" (p. 30). Gregg accepts Wexler and Culicover's (1980, p. 596, fn. 10) position 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 first of three possible levels to which the concept of formalism can apply for an acquisition theory (given the distinction between competence and performance). The first level of formalism concerns the use of a formal linguistic theory, whose formalism might well be syntactic symbols.[19] Second, the system that 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 because applied linguistics is strewn with attempts to use linguistic theories that 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 that they use for their acquisition theory is model-theoretic. As they state (Wexler & Culicover, 1980, p. 31): "a theory of (first) language acquisition may be looked on as a triple $\langle G,I,\mathrm{LP}\rangle$," 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 (a) partly because a clear (though not complete) analogy existed between an existing system (the interactions of photons and electrons) and the system of interest, and (b) 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 (1989) concludes that

> successful 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 judgment 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, this, almost by definition, requires the use of set theory. Adopting set-theory formalism should facilitate the utilization of analogies to other structures because it is maximally applicable across science (and philosophy of science).[20] At the next level down, at least two major formal languages (connectionist and production-system-based programs) are available for the computational modeling of language acquisition; a number of partial models of language production, at least, use 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 or not the descriptive theory in question lends itself to use in an acquisition context, and should not be associated with a single competence theory.[21]

A general objection to the advocacy or use of formalisms is 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 professionals engaged in cognitive science research 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.

SUMMARY

In developing our understanding of theory construction through the materials of philosophy of science, we have to navigate shoals of miscommunication. The early twentieth-century philosophy of science did not communicate well with scientists, and even after its removal from the scene by more empirically minded investigators of science there has been an inevitable time lag during which constructors of theory in the real world of science have been unable to make use of the newer material and have largely gone their own separate ways. We, in the new field of SLA, not always certain of what we are doing, have looked to our neighbors in theory construction, in linguistics and psychology, for suggestions. Because they have largely theorized, rather than reflected on the intent and form of theorizing, deriving guidelines from these areas is difficult, too.

In this paper, I have reviewed early twentieth-century positions on theory format (arising from philosophy of science) as well as the positions on theory format that have emerged more recently (in the last 30 years). The latter have arisen from the actual practices of cognitive science theorizing as well as from recent philosophy of science considerations of general scientific theorizing. The received view was that theories should take the form of deductively related sentences stated in logic—a position closely related to how this philosophical school characterized explanation. There were two main weaknesses of this view. First, logic seemed to be a less-than-adequate tool; second, the actual conception of theory was invalid, because it failed to relate to that used in science and, in particular, failed to relate well to scientific explanations.

Cognitive science approaches to theory were briefly sketched. The relationship among language-related subfields concerned with learning and those concerned with competence has not always been clear. Hence, it was considered necessary to distinguish between transition (e.g., acquisition) and property (e.g., competence) theories, as well as the different types of explanation associated with each. Competence theories in linguistics and psychology have developed specialized formats; learning theories are to be built upon these.

Recent work in philosophy of science was touched on in two regards. First, with the demise of the received view in philosophy of science, there was a shift in conceptions of theory. From being seen as a linguistic object, it moved to being seen as extralinguistic, partially iconic, and, most important, *cognitive* in nature. Second, recent work in this area has been helpful concerning what is required within a transition theory to provide us with a satisfactory explanation. That is the responsibility of the model(s) and/or mechanism(s) associated with the theoretical system in question. It is the explication of this concept, in the detailed work of Harré in particular, that is the most important contribution of recent philosophy of science to those constructing theories of SLA. Those analyzing SLA theories have already noted a lack of clarity shown with regard to this concept by our field. Following Harré, for explanatory purposes a theory should consist of two elements: (a) statements and (b) models or mechanisms. Finally, 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 concern for clarity can be used to argue for formal theories; SLA theoreticians may need to come to grips with several different formalisms, including set theory.

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, 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 generally 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 that 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 are doing.

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NOTES

1. Hunt (1989) defines cognitive science as consisting of "psychology, linguistics, anthropology, philosophy, computer science, and the neurosciences" (p. 603). Theories of SLA are primarily located within this

grouping, with the possible exception of those that deal with SL learning as a psychosocial phenomenon (e.g., Schumann, 1978). For helpful general discussion of how philosophy of science concerns operate in this area, see Bechtel (1988).

2. In a footnote, Sanders (1974) 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 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". (p. 3)

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).

3. Linguistic theories are primarily property rather than transition theories—see Explanation and Theory Format, later. An example of what they intend to explain is as follows:

> The most interesting contribution that generative grammar can make to the search for universals of language is to specify formal systems that have putative universals as *consequences* as opposed to merely providing a technical vocabulary in terms of which autonomously stipulated universals can be expressed. . . .
>
> The explanatory task has not even begun when a constraint or generalization is merely stated. Only when it can be shown to be a nontrivial consequence of the definition of the notion "possible grammar" can it be regarded as explained. (Gazdar et al., 1985, pp. 2–3)

> [At] the level of genuine explanation, we attempt to construct a theory of universal grammar. . . . We can then, in effect, deduce particular languages by setting the parameters in one or another way. . . . [W]e can explain why the sentences of these languages have the form and meaning they do by deriving their structured representations from the principles of universal grammar. (Chomsky, 1988, pp. 133–134)

Linguistic theories explain principally 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; cf. Hintikka & Sandu, 1991, p. 5, who state that "GB theorists . . . use an antiquated hypothetico-deductive model of science").

4. This development was stimulated by early work in theoretical linguistics in addition to that of Miller, Galanter, and Pribram (1960). By this time, logical formalisms had slipped in status: "Psychologists who wish to formalize a theory today have two major alternatives: they may construct either a mathematical theory, usually stochastic, or an information processing model in the form of a computer program" (Simon & Gregg, 1967, p. 246). Stochastic and dynamic models continue to be formalisms used in areas of psychology such as psychophysiology.

5. Hunt (1989) draws on Newell (1980) and Pylyshyn (1984) in identifying three distinct levels of psychological theory within a computational view. Information-processing theories attempt to define human mentalese and the machine associated with it. Physical theories explain how the mentalese machine is instantiated by the brain. Representational theories express regularities in the ways that relationships in the external world are captured by mental models. For discussion of "levels" in cognitive theory, see also Clark (1990) and Peacocke (1986).

6. Naturally, specialists in this area are fighting back, though space does not permit a full discussion here. For the most extreme claims concerning the appropriateness of a computational format, not just for theories of cognition, but for all theories, see Thagard (1988).

7. 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 et al., 1987, p. 2)

See also Suppes (1960).

8. Churchland's (1989) critical comment highlights this orientation:

> I think it strange . . . to embrace an account of theories that has absolutely nothing to do with the question of how real physical systems might embody representations of the world. . . . The semantic approach takes theories even farther . . . away from the buzzing brains that use them, than did the view that a theory is a set of sentences. (p. 157)

9. "If you want to find out anything from the theoretical physicists about the methods they use . . . don't listen to their words, fix your attention on their deeds" (Einstein, 1934, p. 12); Suppe (1974), referring to an important symposium on the structure of scientific theories occurring 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!

10. For a comparison of the way the Harré and the Suppe lines of research use the term model, see DaCosta and French (1990), van Fraassen (1980, p. 44), and Bunge (1973).

11. See Garfield (1988, esp. pp. 26–27) on the need for several levels of explanation in an extensive theory in cognitive science.

12. In his sketch of a cognitive theory of science, Giere (1988) 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" (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 real-world systems. (For his earlier definition and helpful supporting discussion of theory as simply "a kind of natural system," cf. also Giere, 1979, p. 69.)

13. Early proponents of this view (listed and dismissed in Hempel, 1965) were regarded as misguided by the received view, because of the received view's addiction to a concept of explanation based on deductive logic. However, 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; Laschyk, 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 such as "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).

14. This is a complete break, then, not only from the received view, but also from any of its linear descendants, such as the semantic approach of Suppes, newer versions of positivism (e.g., van Fraassen, 1980), and any positions apparently opposed to the received view (e.g., some varieties of realism) that yet maintain that explanation through deduction rather than through the positing of a model or mechanism is adequate. SLA's concern with mechanisms has placed it, albeit unknowingly, firmly in the Harré line.

By knowing what a good theory looks like, we may also be able to say how to get to a good theory: Seek multiply-connected or semi-connected paramorphs that will provide hypothetical mechanisms to explain the systems of interest and their associated events/regularities. Here we impinge on the "context of discovery": means for the discovery of new hypotheses and theories, a topic ruled inadmissible to philosophy of science by Reichenbach (1938) and Popper (1959), but essential to it, and recently reintroduced (Beretta & Crookes, 1992; Nickles, 1980).

15. Logicism is 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).

16. Krashen's (1983) paper provides the most explicit mechanism, the "notice the gap" device.

17. As Levelt (1989) and Klein (1990) have noted in discussions of language research, both connectionism and production systems are formal languages—we are already seeing the development of computational simulations that utilize both (cf. Neches et al., 1987).

18. According to Harré (1985a), scientifically accepted theories exist that are unformalizable, because their central concepts have "contingent features . . . not deducible from some set of first principles" (p. 181). For him a case in point is the virus theory of disease. He also cites social psychology as a general area unlikely to be axiomatizable; molecular biology (Culp & Kitcher, 1989) has also been labeled thus.

19. Some conceptual confusion here may be engendered by theorists who refer to the need for a *sequence* of theories as a way of describing first language acquisition (e.g., Atkinson, 1982).

20. It is noteworthy that Anderson's (1982) ACT* (a production-system-based theory) has been formalized in model-theoretic terms (Heise & Westermann, 1989). It is claimed that the model-theoretic version eliminates "terminological and conceptual ambiguities" as well as "specifying the likenesses to other theories in cognitive psychology [and] artificial intelligence" (Heise & Westermann, 1989, p. 103).

21. Cf. Pinker (1987). Because his learnability theory is not tied to a given L1 competence theory, he found that flaws in his earlier L1 learnability work were repairable by applying a productive analogical system (AI models of the development of abilities in areas such as vision ["relaxation" models]).

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