Cognitive and Social Determinants of Discovery in SLA

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This paper addresses an important part of theory construction, the production of new ideas, that is, the process of discovery, in order to determine what value insights from the philosophy, history, and sociology of science might have for the emerging discipline of SLA. We recognize the current conflict between those who espouse the rationality of science and those who point to social forces and personal motives as causal agents in the process of discovery. We present a case that endorses the role of reasoning in discovery and which accepts the need for social mechanisms appealing to the interests of individual scientists in order to explain how rationality flourishes. We give particular attention to a set of reasoning strategies for generating hypotheses, identified by Darden (1991), and illustrate them with examples from SLA, where possible, and neighboring fields. We proceed to argue that a plausible social explanation for the centrality of reasoning is that the institution of science has evolved in such a way that its interests coincide with the interests of individual scientists, an account which is based primarily on the work of Hull (1988). Again, we indicate possible implications for the new field of second language acquisition.

1. INTRODUCTION

1.1 *Sir John, Sir Peter, Sir Herman on Sir Karl*

Eminent scientists claim to have been influenced by the major philosopher of science, Sir Karl Popper. Sir Peter Medawar (Nobel prize for medicine) considers him the greatest philosopher of science there has ever been. Sir Herman Bondi (distinguished mathematician and astronomer) has declared that there is no more to science than method and that there is no more to method than Popper. Sir John Eccles (Nobel prizewinning neurophysiologist) recalls his conversion to Popper's teachings and attests that he followed them in his own research; he advises other scientists to 'read and meditate upon Popper's writings on the philosophy of science and to adopt them as the basis of operation of one's scientific life' (citation and story from Magee 1973: 9).

But could they really have followed Popper's philosophy? It seems unlikely. A fundamental problem—and here, we rehearse widely disseminated arguments—is that it is notoriously difficult to judge when a theory has been falsified. Negative findings do not point unambiguously to the necessity to abandon a theory or even a component part: perhaps the recalcitrant findings were produced by a faulty test; perhaps the subjects were inappropriate; perhaps the problem data can be shelved just for the moment; perhaps any given ad hoc hypothesis is really a legitimate elaboration of the theory; and so on. Moreover, would scientists judge in the same ways on each of these issues if the theory in question were theirs or that of a rival? Again, it seems not: Mulkay (1991) provides interview data concerning a highly successful biochemist who professes to observe Popperian principles; according to his rivals, he simply refuses to see that a conjecture of his own has been refuted. As Mulkay suggests, there is a gap between the skeleton of logical rules and complex, often confusing practice. The gap provides room for maneuver.

However, the greatest difficulty with observing Popperian principles is that they are prescriptive. They explain how science *should* be done, which is rather like the tail wagging the dog. The phenomenon in need of explanation is how science could have achieved what it has, that is, how science *is* done.[1] Much of philosophy of science for the last 30 years has been addressing this issue and many interesting accounts have emerged.

In view of this, and bearing in mind that we need to make a clear distinction between prescriptivists and those who investigate actual scientific practice, there is good reason to consider the insights offered by the philosophy, history, and sociology of science. And that is the basic issue that this paper addresses: can SLA benefit from serious attempts to understand how science works?

But first the question needs to be refined somewhat by considering a related question that has been treated dichotomously: the 'discovery' versus the 'justification' of theories.

1.2 *Discovery versus justification of theories*

Before we can address the question of the relevance of philosophers' accounts to SLA theory construction, we must first of all clarify which aspects of theory construction we are talking about. Are we interested in generating hypotheses (the context of discovery) or assessing their value (the context of justification) or both? The dichotomy, usually associated with Reichenbach (1938), makes a very broad distinction, one that is too broad for Laudan (1980). Laudan takes justification to be concerned only with the evidence relevant to the finished research report. Fully formed theories are assessed after the data are in. He finds that the allocation of the rest of the enterprise to discovery—'every stage in its history prior to its ultimate ratification' (Laudan 1980: 181)—ascribes to it a very wide scope which he prefers to further divide: he posits a context of 'pursuit' between discovery and justification, restricting discovery to whatever is involved in generating a new idea. There are problems in ignoring the possibility that justificatory principles might be invoked even in the context of discovery, but it seems reasonable to assume, as Laudan does, that much of the overlap within the trichotomy will occur in the context of pursuit. Therefore, henceforth we will respect Laudan's division and construe discovery as a concern limited to the generation of new ideas.

The idea of formulating rules that would lead to discoveries had a long and illustrious history going back to antiquity and flourishing in the works of Bacon,

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Descartes, and Newton, among others, before being abandoned in the nineteenth century (Laudan 1980). This century, the quest for discovery rules has been dismissed by most philosophers, and relegated to the purview of psychologists. Popper's view is typical: 'every discovery contains "an irrational element", or a "creative intuition"' (Popper 1959: 32).

Folklore, too, has settled on romantic tales of flashes of insight following falling apples, overflowing baths, and the like. However, more recent and sustained attention from philosophers and historians of science such as Nickles (1980, 1987), Gutting (1980), and Darden (1991) has revived interest in the issue. In the cognitive sciences, there is a burgeoning literature on discovery heuristics, especially in the field of artificial intelligence (Lenat 1979, Langley, Simon, Bradshaw, and Zytkow 1987, Thagard 1989), but also in psychology (Gigerenzer 1991). From the perspective of SLA at its present stages of theory construction, the particular interest lies in the prospect that an awareness of discovery strategies might increase the rate of theory generation or, more modestly, enhance our understanding of how to approach theory generation. And since issues of justification have been dealt with elsewhere in the L2 literature (Beretta 1991, Crookes 1992, Long this issue), it is appropriate in this paper to focus squarely on discovery.

2. COGNITIVE VERSUS SOCIAL CAUSATION OF DISCOVERY

Whenever the question of discovery is discussed, there is a great deal of controversy regarding the relative importance of cognitive and social factors in shaping the content of theories. The argumentation has been highly polarized with philosophers of science and cognitive scientists on one side and sociologists of scientific knowledge on the other. Since some of the perspectives on both sides appear to rule out, or excessively minimize, the contributions of the other, we give them short shrift before settling on less radical approaches. First, the social angle.

2.1 *Social context and social 'interests'*

Consider the following two claims. One of them is a parody, the other serious, though it may not be immediately clear which is which:

a. Heisenberg would not have argued for quantum indeterminacy had he not been sensitive to the rise of irrationalism in Weimar. (Fuller 1989: 627)

b. Goedel's 'incompleteness' theorem arose from lacunae in the Viennese social order of 1930. (Slezak 1989: 589)

The serious claim (a) is typical of those that have been made by sociologists of scientific knowledge who consider the social context capable of actually *causing* the content of theories. The most radical articulation of this position has come from proponents of the so-called 'strong programme' (see the classic paper by Barnes and Bloor 1982). The approach has been to select a discovery in the history of science and seek to explain it in terms of social conditions prevailing at the time. A large number of putative cases have been identified in this way and the resultant body of knowledge forms the empirical base which is used to ballast the sociological thesis.

This argument is of the *post hoc ergo propter hoc* persuasion and, as such, fallacious: the social conditions are prior but this does not establish that they are causative. There is no warrant for the claim that social context constitutes a sufficient condition for scientific discovery. Indeed, it is not evident that it is even a necessary condition beyond the trivial sense that there must be a social context in which any given discovery occurs. Furthermore, as Slezak (1989) points out, there is a mismatch between general social contexts and the detailed content of theories. Descriptions of alleged social causes are not intricate enough to account for their very precise effects. What elements, for instance, of Lenneberg's social surroundings could have led him to propose the equipotentiality of hemispheres for language until around 24 months of age?

Introducing the notion of 'interest' to mediate between social context and theory content has not, in Slezak's (1989) view, done much to improve the case made by sociologists. Interests may, however, be a fundamental, necessary condition in the far from trivial sense that science could not have been so phenomenally successful as it has been without the development of a very sophisticated social institution of science that is so organized that the self-interest of individual scientists coincides with the interests of the institution. Such a case is elaborated by Hull (1988) at great length. Since his social explanation of the process of science indicates what features of the institution of science need to be in place, it is very suggestive for a young field such as SLA where, conceivably, some important traits have yet to emerge. We will therefore give due attention to Hull's interest model as a complementary element in the discovery process.

2.2 *Cognitive heuristics and strategies*

On the cognitive side, one major line of work on discovery has been carried out by Herbert Simon and his colleagues. They have attempted to demonstrate that component problem solving in scientific discovery has no special properties that distinguish it from other problem solving (Simon, Langley, and Bradshaw 1981). They acknowledge that real scientific discovery is different from ordinary problem solving in that (i) it takes place in a social milieu, and (ii) its goals are often less definite. However, they argue that complex problems are decomposed into simpler, more manageable problems and that solving these component problems is not qualitatively different from other problem solving. Simon and others (see especially Langley *et al.* 1987) have developed a computer program called BACON which is presented with 'the same sorts of empirical data that were available to physicists and chemists during certain critical episodes in the ... histories of those sciences' (Simon *et al.* 1981: 3). They claim that, using this procedure, BACON (in its various manifestations) has 'discovered' Boyle's Law, Kepler's Third Law, Coulomb's Law, and Ohm's Law, among others.

It might be argued that since computer programs autonomously make discoveries, these discoveries are entirely independent of social factors (cf. Slezak 1989). But before concluding that discovery is a purely rational, teleological enterprise, it should be noted that BACON needs 'clean' data which it will summarize, but it cannot determine which data to gather, nor can it formulate explanatory theories or make experimental predictions; it cannot rule out irrelevant variables; indeed it has to be fed the relevant independent variables (Brannigan 1989). This leads Giere to conclude 'it could not be said to be doing anything meaningful, let alone making a scientific discovery' (1989: 641). Doubters, as Langley *et al.* (1987: 339) are well aware, will only be satisfied when a program such as BACON actually makes a novel discovery instead of detecting mathematical regularities in given historical data sets.

From the point of view of SLA, BACON is currently unlikely to have any influence. Heuristics such as 'if the values of one term increase as those of another decrease, then consider their product' (Langley *et al.* 1987: 66) are useful in seeking invariants in data which, once found, may constitute the parsimonious, seemingly timeless, grand summary expressions of the historical discoveries they deal with: for example, the cube of a planet's distance from the sun is proportional to the square of its period (Kepler's third law of planetary motion). But of course, we do not expect such mathematical regularities from the complexities of language acquisition. The kinds of heuristics that might engage our field must be of a different form altogether.

An approach which seems particularly promising comes from an investigation of strategies used in a particular historical episode in the life sciences, the development of the theory of the gene early this century. Darden (1991) examines the published literature of the time and identifies strategies that could have been used by those involved at various stages of the growth of genetic theory. What makes her attempt relevant to SLA is that the strategies are not domain-specific but instead can serve as candidates for use in other contexts (and yet they are not confined to data manipulation). In the following section, we will give particular attention to Darden's account and attempt to relate her strategies to examples of idea generation in fields familiar to SLA researchers.

3. REASONING STRATEGIES

3.1 *Preliminaries*

So far, we have used the terms 'logic', 'heuristics', and 'strategies' without comment. Our use of the latter two terms makes no distinction. They both connote reasoning methods (or principles, or tactics, or rules of thumb) that may yield (a range of) plausible hypotheses. They contrast with 'logic' which implies a prescription that guarantees the production of correct hypotheses. Since a logic of discovery seems remote, we will restrict our discussion in this section to reasoning strategies or heuristics (for a clear appraisal of terminological issues, see Darden 1991, especially pp. 15–21).

One concern with strategies that are not domain-specific is that they might be insipid. The BACON researchers describe their own heuristics as 'weak', meaning that they are common to all kinds of problem solving. However, they are still precise rules. Darden's strategies, being unrelated to the sifting of data vectors, are intended to be rather more general, but could hardly be considered jejune. Truly unprofitable strategies of indisputable generality are those used by extreme inductivists such as some of those engaged in input research in SLA (if Gregg, this issue, is right); strategies such as 'try anything' and 'random search'. Equally hopeless are vague injunctions from great figures in science: 'court serendipity by being eccentric', 'do what makes your heart leap', 'think big', and 'dare to explore where there is no light' (collected in Root-Bernstein 1989).

A final preliminary point is that although we are only dealing with strategies for producing new ideas, Darden, like Laudan, sees value in a tri-partite distinction (in her case, between (i) producing new ideas (ii) assessment and (iii) anomaly resolution). She stresses that the process of theory construction is not linear but involves a complex interplay of different kinds of strategies at all stages in a theory's development. This should be borne in mind in the account that follows.

3.2 *The strategies*

Darden (1991: 243–57) identifies seven strategies for producing new ideas:

1. Use analogy.
2. Invoke a theory type.
3. Use interrelations.
4. Move to another level of organization.
5. Introduce and manipulate a symbolic representation.
6. Introduce a simplifying assumption, then complicate.
7. Begin with a vague idea and successively refine.

The strategies, which can be employed simultaneously, are not mutually exclusive (or, it need hardly be said, cumulatively exhaustive). We will consider each in turn, providing illustrations and a commentary, though for strategies 6 and 7, we keep the discussion somewhat briefer than for the other strategies as they are relatively imprecise. Wherever possible, we use examples from SLA, though at times this proves to be beyond us.[2]

3.2.1 *Use analogy* An analogy, a process of drawing parallels from one domain of knowledge to another, works on the principle that old ideas can be used to construct new ideas. As Clement (1988: 581) puts it,

> by seeming to move *away* from a problem the subject can actually come *closer* to a solution. In order to use an analogy effectively, one must be able to postpone working directly on the original problem and be willing to take an 'investigatory side-trip' with the faith that it may pay off in the end.

This process involves stages of retrieval (selecting an appropriate analog), elaboration (if necessary, making explicit features relevant to problem situation), mapping (transforming features of the analog to make it applicable to subject area), and justification (assessment). An analogy can be close or distant, from the same field or from another.

Thus, in Mendelian genetics, an explanation for 3:1 ratios for a cross between green and yellow peas can be used, by close analogy, to explain a cross between tall and short peas or, a little more distantly, between red-eyed and white-eyed fruit flies (Darden 1991: 245).

Generation of an analogy from a formal principle, as in this case, seems most likely to lead to specific hypotheses. If a formal principle is not available, can a vaguer postulated similarity be useful? This raises the question of what makes an analogy a good one. According to Gentner (1983) and Clement and Gentner (1991), a preference for systematic correspondence between the analogous domains appears to be a psychologically real constraint on analogy selection. Thus, in SLA, one would be looking for coherent systems of common information or shared properties and relations.

Familiar analogies in L2 inquiry are that it might be like pidginization, or associationist learning, or most obvious of all, like L1. For clear instances of reasoning by analogy in this latter case, see Cook (1988: 174–6) and White (1989: 35–54). White (1989: 35) considers 'whether a parallel case can be made for L2 acquisition [with L1]', and notes that 'it only makes sense to ask if knowledge of language is acquired in a similar fashion if that knowledge has common properties in both contexts.' In both cases, it is assumed, competence is represented in the form of an internalized grammar. In both cases, the learner's interim competence is guided by an abstract rule system. In both cases, there is a mismatch between input and ultimate attainment (argued in terms of underdetermination, degeneracy, and negative evidence). In neither case, it is claimed, do learners make errors that violate UG principles. With recognition of these similarities, the first stage, retrieving an analogy that shares properties and relations, is accomplished. Since no special elaboration is needed, the next step is mapping.

In mapping L1 onto L2, not all features will be the same. Certain transformations are necessary to make the analogy applicable. At this point, areas that do not map appropriately are identified and altered. In L1, acquisition is characterized by complete success, whereas in L2, shortfalls in attainment are the norm and the possibility of native-like competence is in doubt. Other differences are that (i) the L2 learner has already acquired an L1; (ii) the L2 learner is older and there may be a critical period for language acquisition (Long 1990, Newport 1990); and (iii) L2 learners may receive formal input.

In the process of mapping, the changes that are proposed should yield testable hypotheses. For example, knowing that the L2 learner already has an L1 might lead to the conjecture either (i) that access to UG in the L2 is restricted to those aspects available in the L1, or (ii) that L2 learners initially assume L1 values of UG parameters, but are still able to access UG and thus can reset to L2 values. These conjectures may be tested. This leads us to the final stage in the use of analogy: justification, that is, the usual range of assessment strategies (as catalogued by Long, this issue).

The above instance from SLA is reasonably well developed and is now far beyond the original generation of the idea by analogy, being occupied currently by issues of anomaly resolution, justification, and theoretical refinement. However, it is possible to see the same original processes being deployed in the analogy that L2 learning is *not* like child language development but like other forms of adult learning, involving problem-solving and hypothesis-testing. The analogy has been retrieved and mapping is under way (see in particular Bley-Vroman's (1991) Fundamental Difference Hypothesis).

3.2.2 *Invoke a theory type* A theory type is an abstract schema that is formulated from a recurring type of relation or process common in the natural world. Once the abstraction has been constructed, it becomes available for use in theory construction. An example of a recurring process in the natural world is selection. Darden and Cain (1989) have patterned an abstraction of this theory type.[3] Other theory types, discussed by Shapere (1974), are compositional and evolutionary types.

This is, on the face of it, a very promising avenue for discovery-oriented research to take, and a number of philosophers and artificial intelligence researchers have shown an interest (Hanson 1961, Brandon 1980, Schaffner 1980, Kitcher 1981, and Thagard and Holyoak 1985). However, so far, apart from those already mentioned, very few theory types have been identified. The goal would be to produce a catalog of abstract theory types from the history of science, comprising what Darden has called 'compiled hindsight' (1987). Such a catalog would provide a potentially useful set of directions for initial stages of theory development, particularly if the domain items a researcher is interested in indicate the fruitfulness of pursuing a certain type of theory. For instance, one might be concerned in a particular domain with an innate–acquired interaction; the compiled hindsight of the history of science might reveal that a familiar range of theory types have typically been proposed for this problem in a number of disciplines, that a subset of these always led to dead-ends, and that a specific type permitted progress. Armed with an abstract formulation of this successful theory type in a given domain, there would be a powerful reason to adopt it as a first approximation. (For a superb initial investigation of innate–acquired type problems, see Wimsatt 1986.)

Although a taxonomy of theory types has yet to be developed, the strategy is still suggestive. While awaiting the program of research that would provide a catalog of theory types to build on the initiatives of Shapere (1974) and especially of Darden and Cain (1989), we might speculate about the value of developing abstract schemata for recurring themes (if not full theory types) for phenomena relevant to SLA. For instance, in a variety of fields, it has been found again and again that the presence or absence of environmental influences during particular periods in the lifespan of an organism affects the ultimate structure and functioning of that organism. The notion of a critical period to account for this phenomenon, first proposed in embryology early this century, has since been seized upon by Lorenz in his work on imprinting, and by scholars of (among other things) socialization in dogs, sexual development in rhesus monkeys, song learning in passerine birds, and language acquisition in humans.

It seems likely that the borrowing of the critical period concept involved some level of abstraction, as the particulars of the work on embryos by Stockard (the production of 'monsters' by interfering with the embryo at different stages in its development) were quite removed from the details of research into imprinting in greylag geese, and song acquisition in white-crowned sparrows. Certainly, by the time the concept was taken up by SLA researchers, there was a considerable literature to draw upon (as Long's (1990) detailed review and diverse bibliography confirm). In particular, abstract discussions were available. A good example is an article by Bateson and Hinde (1987). From this article, we have a common vocabulary, a definition, details of the essential component ideas pruned of topic-specific content, a methodology, and direction regarding the types of explanation that are to be sought. Thus, for example, available to any SLA researcher investigating the critical period hypothesis are such notions as 'onset', 'asymptote', 'duration', 'offset', and 'reversibility'; and the instruction to seek a proximate cause (neurophysiological) and an ultimate cause (evolutionary, see section 3.2.5). A rough abstraction for the critical period hypothesis might be something like the following:

Preconditions:
1. A class of Ys exists
2. Ys have an innate property P
2.1 which is not completely specified
3. Ys are in an environment E
4. E possesses a 'releaser' R

Interaction:
5. Ys interact with E
6. R releases P
6.1 but only during definite phase X

Effects:
7. P is activated and leads to normal, complete adult functioning
8. Effects are at least partially irreversible (cannot be erased or repeated).

Themes of such ubiquity and generality across disciplines as the critical period hypothesis commend themselves as candidates for this kind of attention. Although they do not provide complete theories in the way a selection type theory does, they at least offer some opportunity in hypothesis generation. Assuming some familiarity with other disciplines, there is no reason why SLA theorists should not themselves seek to identify other recurring themes of potential applicability to problems in SLA and derive abstractions.

3.2.3 *Use interrelations* This strategy is very similar to 'use analogy'. An important difference is that in postulating specific interrelations, two domains each recognize an ontological relation with the other. Darden (1991: 252) takes the view that 'if neither field is well-developed, then the interrelation may not be very fruitful in hypothesis formation.' Since SLA theories are at preliminary stages of development, and since fields that SLA draws on are not persuaded that SLA is intrinsically relevant to them, there are no examples in this domain of what Darden and Maull (1977) have called an 'interfield theory'.

However, we do not have to look far beyond SLA to find an instructive illustration of using interrelations: the interfield discipline of psycholinguistics. Psycholinguistics has been accused of being a case of a failed interfield discipline. Failed, it is claimed, if one considers a joint enterprise a failure when one of its two domains—psychology in this case—has pulled out (cf. Johnson-Laird's (1977) paper entitled 'psycholinguistics without linguistics'). Certainly Reber (1987) and McCauley (1987) consider psycholinguistics in that light as their titles bear ample testimony: 'The rise and (surprisingly rapid) fall of psycholinguistics', and 'The not so happy story of the marriage of linguistics and psychology'. However, from our perspective, this allegedly failed use of a strategy is at least close to home and just as instructive as a successful one (say, a cross-disciplinary venture that resulted in a separate field with its own departments within universities, the field of biochemistry (Bechtel 1986a)).

A Blumenthal (1987) has argued, the motivation for trying to bring psychology and linguistics together was not that some researchers in each field were looking for an analog but rather that they recognized an intrinsic connection between the two fields. Chomsky's assertion that linguistic competence is properly viewed as a central domain of human psychology (along with his opposition to behaviorism and his articulation of a theory for the study of grammar) gave impetus to the development of a hybrid discipline. Psychology was ready to receive this impetus because, as Mehler (1980) observed, the accumulation of data without theoretical guidance was a source of dissatisfaction to many. Both fields, therefore, were inclined to look to the other, and this appears to be the defining feature of the strategy of using interrelations.

Mutual attraction may be enough to motivate initial use of this strategy but it is not enough to maintain it. It appears that there must be some reciprocal arrangement between the two fields in which the theories and findings of each are taken seriously by the other. McCauley (1987) comments that the modularity thesis, positing a unique cognitive process, effectively isolates linguistic competence and renders it impervious to constraints by any general psychological principles: 'the consequences of an encapsulated language module . . . balkanize psychology, because they insulate theories of the language organ from other psychological research' (McCauley 1987: 344).

Also, as Reber (1987: 13) complains, linguistic theory is in a state of perpetual flux, but 'no theoretical model of language has ever been rejected because it failed to account for the data from a psychological study.' There is no shortage of linguists prepared to reply that data from language processing have no privileged status (for example, Newmeyer 1983: 46). Carlson and Tanenhaus (1982: 58) argue that there is a mismatch in sophistication between linguistic theories and theories of processing, and that only when processing theories are more developed 'can serious connections be made between psychology and linguistics'.

Though he apparently sees no such mismatch in sophistication, McCauley comes to much the same conclusion that a separation makes sense because both fields 'need as much conceptual space as they can get to develop more adequate theories' (McCauley 1987: 351) and 'it is only *after* reasonably stable theories appear in either the psychology of language, linguistics, or both that we should hold out any hope of a psycholinguistics with any staying power' (ibid.: 351).

Bechtel, a philosopher who has focused attention on the integration of scientific disciplines (see especially Bechtel 1986b) strongly disagrees, insisting that there is no reason why both fields cannot 'serve as part of the endeavor of discovery', and thus 'devise theories that are naturally integrated with each other' (Bechtel 1988: 306). Some linguistic theories are thought, in certain quarters, to be more readily reformatted for processing research than Chomskyan generative grammar. For example, Augmented Transition Network grammars (ATN), developed by computational linguists and psychologists (for the origins, see Thorne, Bratley, and Dewar 1968, Woods 1970). In an insightful paper, Abrahamsen (1987: 372) observes, 'with little modification, these could be applied as models of the mental representation of syntax, leaving the psychologists free to focus on the processing part of the model.' As she further comments, Kaplan, an ATN psychologist, and Bresnan, a linguist, collaborated to devise lexical–functional grammar (LFG) (Kaplan and Bresnan 1982) which specifically built in both linguistic and processing constraints.

It is worth mentioning, in passing, that the idea of a process-ready grammar has been found appealing to major acquisition theorists. In L1 acquisition, Pinker (1984) explains why he selected LFG as the structure whose acquisition he was trying to account for, alluding, among other things, to its accompanying theories of parsing, production, semantic interpretation, and its detailed exploration of computation-theoretic properties. In L2 acquisition, Pienemann (1992) appears to have been motivated by much the same concerns in opting for LFG.

Before leaving the strategy of using interrelations, it should be stressed that it is not an issue of kindness by one discipline toward another. If the relation is not one of mutual recognition and exploitation, it seems unlikely it would occur in the first place.

3.2.4 *Move to another level of organization* The idea here is to determine whether the explananda can be framed hierarchically. If so, it may be possible to form new hypotheses at different levels of the hierarchy. It is, however, very difficult to define 'level' in a way that has any generality. We can only give examples. In the units of selection controversy (Dawkins 1976, Sober 1984), 'level' means something like 'size'; the debate about the proper unit of analysis—species, group, individual, gene—is a debate about level.

In attempting to determine where on the hierarchy an entity or process belongs, it is particularly important to ask 'is there a major relevant class of perturbations that disaggregate or disrupt one set of entities but not another?' (Darden 1991: 253). The Chomskyan encapsulated 'mental organ' is proposed in a way that makes it appear monadic, or a lower-order relational property (at the level of the genotype, rather than a phenotype–environment relation; Chomsky 1980: 65).

Another difference in levels familiar to SLA (mainly via Jacobs 1988, 1990) features psychological and neurobiological accounts. Is movement between these levels possible, findings from each suggesting hypotheses for the other? According to proponents of eliminative reductionism (for example, Stich 1983, Churchland 1986), the movement is in one direction: higher-level psychological theories are reducible to lower-level neurobiological theories. On this account, psychology is dispensable, and no new hypotheses are generated by the movement to another level. On the other hand, those, like Fodor (1975) and Putnam (1975), who advocate an autonomous psychology, are not concerned with its underlying neural substrate (but see Gregg 1989). With no interlevel movement, the strategy 'move to another level of organization' simply does not apply.

There are two arguments for the autonomy of psychology: (i) that the same brain state can occur in different mental states; and (ii) that the same mental state can occur in different brain states. If true, then the mapping of one level onto the other is bound to be many–many (typically thought to be incoherent in classical reductionism; cf. Nagel 1961). One response to this is to try to argue that Nagelian reductionism is still possible with many–many mappings. Another is to accept the autonomy of psychology and seek to show that neurophysiological evidence can be useful in assessing psychological theories and can lead to the generation of new hypotheses. This latter approach is taken by Bechtel (1984, 1988) and is the only course known to us which permits use of the interlevel idea-generating strategy. Without getting into the complexities of the autonomy-reduction argument, it is sufficient for our purposes to cite examples of interlevel activity leading to hypothesis generation.

Two of the examples cited by Bechtel (1988: 81–5) are from other fields. First,

> Boveri and Sutton developed evidence relating Mendelian genes with chromosomes . . . they then focused on points at which claims made about Mendelian genes differed from those made about chromosomes. Given these differences, the information offered at each level suggested investigations at the other level. This resulted in revisions in the theories developed at each level. *Here cross-level identification served as an hypothesis-generator. . . .* (Bechtel 1988: 85, emphases added)

The second example is that interlevel work in statistical mechanics and thermodynamics 'served to guide the development of the theories at both levels' (ibid.).

Back to psychology and neuroscience, Bechtel offers an intriguing example of lower-level information undercutting the higher-level account (ibid.: 81–2): Gall, in the early nineteenth century, postulated functional faculties responsible for behavioral traits and attempted to identify brain centers for each faculty. Flourens (1824) carried out a series of experiments with animals, lesioning the areas to which Gall had assigned specific functions, and demonstrating that the predicted deficits and the actual deficits did not correlate. (According to Harris (1991), Flourens' animal data, mostly from birds, were based on broader functions like 'intelligence', 'sensation', and 'movement' rather than the twenty-seven discrete and, sometimes, peculiarly human, faculties studied by Gall. Flourens did claim to have thereby repudiated Gall's organology, but there is room for dispute.)

A final example comes from aphasia research and will be familiar to many in SLA. This time, the lower-level theory was compromised by higher-level research and new hypotheses were generated. Traditionally, Broca's area was thought to be responsible for language production and Wernicke's area for comprehension. Reinterpretation within the framework of linguistic theory led to the proposal that Broca's area was actually responsible for syntactic analysis (see Bradley, Garrett, and Zurif 1980; and for further elaboration, Grodzinsky, Swinney, and Zurif 1985). As Bechtel (1988: 108) points out: 'here a psycholinguistic perspective figured centrally in revising an account originally developed from more neurally-based aphasia studies'.

These examples indicate that the strategy of moving to another level of organization can be fruitful in producing new ideas. But in psychology and in SLA, it is not likely to be so if reductionist goals hold sway (and Jacobs' suggestions for SLA tend in this direction) or if neurobiological accounts are thought to be irrelevant to psychologically motivated research. (This is not to say that either approach is wrong, merely that the strategy we are here concerned with would have less applicability.)

3.2.5 *Introduce and manipulate a symbolic representation* This strategy refers to any use of a model to produce new ideas. The models could be mathematical equations, diagrams, computer simulations, scale models, anything that represents the system that is being examined. The point about this strategy is that models permit manipulation or tinkering. A well-known example is the use of models in the process of the discovery of DNA. Watson's (1968) personal account of the discovery repeatedly highlights the importance of playing with the models, trying out new configurations:

> I spent the rest of the afternoon cutting accurate representations of the bases out of stiff cardboard . . . [I] began shifting the bases in and out of various other pairing possibilities. Suddenly I became aware . . . Chargaff's rules then suddenly stood out as a consequence of a double-helical structure for DNA . . . (Watson 1968: 114)

> Constantly [Crick] would pop up from his chair, worriedly look at the cardboard models, fiddle with other combinations . . . (ibid.: 116)

> Only one person can easily play with a model, and so Francis [Crick] did not try to check my work until I backed away and said that I thought everything fitted. (ibid.: 117)

There are countless examples of the use of diagrams and flow-charts as symbolic representations in SLA. These diagrams are generally presented in an expository way, simply as an alternative to, or a confirmation of, the accompanying textual account. Although it is not clear that the models have been used in generating hypotheses, they clearly could be 'tweaked' to see if they can better account for what is already known; components could be added, modified, withdrawn, possibly sparking an idea.

A computer simulation of the evolution of the critical period in human language acquisition is offered in a fascinating study by Hurford (1991). Thirty individuals are equipped with a name, a stage (from 1 to 10) in the lifespan, a language (0 to 10), a parent ID, a dominant 'language acquisition program' (+1 for alleles with a facilitating effect on language acquisition, −1 for alleles with an inhibiting effect), and a recessive 'language acquisition program' (the choice of which allele becomes the dominant and which the recessive in the newborn being random). An evolutionary cycle is designed, variable conditions are factored in, and 1,000 generations are run.

Some of the programming choices are open to modification. Hurford notes that the model simplifies and idealizes at various points; for instance, in positing a simple distinction between facilitating and inhibiting alleles. However, in this instance, he notes, 'it would have been too complicated and too speculative to build a model with such properties' (Hurford 1991: 182). Nevertheless, it is not out of the question for later simulations to tinker with properties of greater complexity. In fact, Hurford does fiddle with his model at some points: noticing occasional late-life surges in language acquisition capacity, he manipulates the model to check what transpires (ibid.: 191); also, he designs other simulations with added complications to see what happens if humans undergo frequent language-impairing neural insult without actually dying (ibid.: 193). Thus, and this is really the point about modifying representations, what-if scenarios can proliferate.[4]

3.2.6 *Introduce a simplifying assumption, then complicate* One way to initiate theory construction is to start with the simplest plausible assumption and then to complicate the oversimplification when the means to do so become available. We have already seen examples of a simplifying assumption made by Gall (1819) in his proposal that mental faculties had a one-to-one mapping with neural correlates. By now, with the emergence of more sophisticated testing techniques (magnetic resonance imaging, regional cerebral blood flow, PET scans, Wada testing, electrical stimulation, etc.) the relationship has been substantially complicated (see, for example, Caplan 1987).

3.2.7 *Begin with a vague idea and successively refine* An example of beginning theory construction with a vague idea and then refining it comes from the theory of continental drift. The original, relatively vague proposal by Wegener in 1915 could account for such phenomena as the formation of mountain ranges and the close fit of the contours of the South American and West African coastlines. What it could not do was explain to anyone's satisfaction, including Wegener's, how the continents could have ploughed their way through the much denser rock of the ocean floor without disintegrating. Work done by paleomagnetists and marine geologists in the 1950s and, clinchingly, in the 1960s provided the long-awaited refinement: the idea of sea-floor spreading caused by material being forced up from the Earth's mantle by rising convection currents was sufficiently refined and well-corroborated to win over reluctant 'fixists'. (Among the many accounts written by philosophers, see, for example, Frankel 1986.)

4. SOCIAL FORCES AND PERSONAL MOTIVES

The reasoning strategies presented above are derived from readings of certain episodes in the history of science and, as such, might be considered potentially useful in SLA theory construction. But cognitive approaches clearly do not tell the whole story. Sociologists of science of the stature of Merton and Latour[5] have forced us to consider the important role that social factors play in the growth of scientific knowledge. For any of us interested in learning how scientific discoveries come about, a social perspective is sure to be instructive, if only because there must be something about the social institution of science that has enabled it to make the kinds of advances that it has. Specifically, it is remarkable that reason (evidence, internally consistent argument) appears to be accorded particular value given that the practitioners of science are presumably as human and fallible as people in any other institution. This requires some explanation.

A mechanism proposed by Hull (1988) is essentially that science as a social institution is so structured that its interests coincide with the self-interest of individual scientists. In effect, rationality is encouraged by the social structure of science. The claim is threefold: that much of science can be explained by (i) the desire of scientists to receive credit for their work in the form of peer citation, particularly acknowledgement that their work is being built on; (ii) the need for mutual support and cooperation; and (iii) checking, in the form of mutual testing of claims. As Hull (1988: 283) puts it, 'whenever the conditions for the operation of this mechanism are met, the result will be science as we know it'.

In this section, we frame the discussion around Hull's 'interest' mechanism nd speculate about features of the social structure of SLA as an emerging field f inquiry that may not yet be in place. It may be that the social conditions of nore established sciences are better evolved than those in more recent fields to natch institutional and individual interests such that discovery is more likely to ourish.

.1 *Credit*

[ull claims that receiving credit, in the form of citation, particularly if it is vident that one's work was recognized as foundational, is the glory that ientists seek. Moreover, that credit must be from one's peers, not from outsid-s; Hull talks of the loss of professional reputation that occurs when a scientist's ame becomes too prominently displayed in the popular media. He cites grous-g of the following sort among peers of a biologist who went public: 'he couldn't

make a name for himself in real science, so he goes on the Johnny Carson show' (ibid.: 306).

Associated with the desire for peer credit is the wish to be recognized as the *first* to make a given discovery. Both Watson (1968) and Crick (1974) make no bones about it: they wanted to be first past the post with the DNA structure. Similarly, priority was the goal that spurred on Banting and Best, the discoverers of insulin, to work around the clock, at times sleeping in the laboratory and preparing meals over the Bunsen burner (Bliss' (1982) account of the story is intriguing, in both senses of the word). The desire to be credited with new discoveries drove two rival nineteenth-century paleontologists, Cope and Marsh to extremes which even reached beyond the grave: in his will, Cope left his skull to science to be measured; this would show conclusively that he was smarter than Marsh.[6]

If this kind of scientific *realpolitik* is the norm, and Hull argues that it is, then it is crucial to science to have the means for the proper attribution of credit. Without that means, on Hull's account, as witnessed by earlier forms of science such as alchemy, there would be no cumulation of knowledge because science would be secret. A means had to evolve to encourage scientists to share their findings.

Historically, the means that has come down to us emerged in the following way (see Hull 1988: 322 ff.). In the seventeenth century, Bacon urged scientists to submerge their personal drive for credit to the common weal and several groups of scientists endeavored to follow this principle, including the French Academy of Science. Having concluded that the appeal to higher motives was not working, the Academy appealed to self-interest, recognizing that it served the institutional interest:

> It having been found by experience that there are disadvantages in the tasks to which the academicians apply themselves in common, each one shall choose a particular object for his studies, and by the account he shall give of it in the meeting, he shall endeavor to enrich the Academy by his discoveries and improve himself at the same time. (cited in Hull 1988: 323)

Presentation at the Academy satisfied the credit requirement, but for science to be cumulative, and to reach a wider audience of scientists, a record was necessary. The final crucial step was taken by the Royal Society of London: swift publication in the *Philosophical Transactions* of the Society. This formula permitted individual assignment of credit but promoted public access and is the formula that, of course, serves us still.

Assuming then, that generally speaking, status accrues from within-discipline credit, it might nevertheless be the case that in regard to a new field like SLA, the coveting of credit *outside* the field is common. It is frequently observed that SLA researchers cite linguistic and psychological research that has been foundational to their own, but that these fields do not reciprocate. There are assertions from within SLA that linguistics ought to revise its theories in the light of SLA findings. For example, if L2 learners violate UG, some insist that such nformation should be used to change UG. Cook (1988: 181) disagrees, ›ointing out that 'evidence that L2 learners apparently breach UG is open to ›ther interpretations; they might have been influenced by a teaching method, ıave used other faculties of the mind, have transferred something from their ›wn language; and so on.' In principle, however, there is no reason why UG/L2 indings should not feed back into UG; problems of interpretation should not be een as a disqualifying factor since they are common to all fields, not just SLA. Iowever, SLA insistence on reciprocity is perhaps somewhat disingenuous iven the preliminary nature of SLA research (contrasted with the relatively :eveloped state of linguistic theory). Assertions requiring other fields to give redit to one's own are perhaps more royalist than the king, but if Hull's analysis ; correct, linguists and others will certainly use SLA findings if they perceive ıat doing so will increase their chances of being *right*, thereby enhancing their wn prestige. As it is, linguists have indicated that SLA is potentially interesting : it develops. An example is in order.

The position taken in Newmeyer and Weinberger's (1988) assessment of the tatus of SLA is that its close ties to pedagogy hold the field back. The goal of LA theory is better theory, not to influence the practical world. The central ıeme of their paper is 'the struggle of the field to free itself from ties to edagogy' (Newmeyer and Weinberger 1988: 41). In their view, continuing oncern with pedagogy prevents SLA from taking its place as a mature scientific iscipline. It is only in a discipline so perceived, we conjecture, that cross-isciplinary credit will accrue. The Newmeyer–Weinberger view is consistent ·ith Peirce's classic statement: 'True science is distinctively the study of useless ıings. For the useful things will get studied without the aid of scientific men' ᵓeirce 1957: 210).

Such a suggestion is bound to be deeply divisive, but presumably that is the oint of it. If Hull is right, though, the separation of SLA and pedagogical ›ncerns will occur if SLA researchers judge it in their self-interest to pursue ›parate agendas (many, of course, already do); it will not occur (nor be ›strained) by rhetorical fiat. Evidence of such a separation will be determined, ›r instance, by the success of journals like *Second Language Research* whose ated editorial policy has been to solicit papers that link SLA to '*non-applied*' elds (theoretical linguistics, neurolinguistics, etc). (*Studies in Second Language cquisition*, by contrast, though it also states a preference for theoretically-otivated papers, still maintains a policy of considering discussions of edagogical issues.)[7]

2 *Support*

ll scientists are forced to use the work of others. Once a body of knowledge is ·tablished, a theory well corroborated, it provides the platform for further ivances. Everyone is familiar with Newton's statement that if he has seen rther than most, it is because he stood on the shoulders of giants. In this sense, ientists are forced to cooperate with even their closest rivals (i.e., by using eir work).

In order for scientists to support their views, they must cite the work they have used. Researchers working on the critical period for language acquisition did not develop the idea in a vacuum, and so tend to cite their antecedents, notably Lenneberg (1967). Thus, Lenneberg receives the credit, but the scientist who cites him thereby strengthens his or her case. In this way, scientists trade credit for support.

There *is* an alternative that some prefer: do not cite others but instead present oneself as a pioneer, claiming originality. This denies anyone else the credit they desire, appropriating it all for oneself. Seeking all of the glory might work, but it is a risky approach, as one will have sacrificed support. Work claimed to be original may be entirely derivative, and work claimed to be part of a great tradition may actually be quite original. The only issue, then, is how the individual scientist chooses to present that work (Hull 1988: 202ff.). The safer route is to acknowledge the contributions of others, even when those contributions have been slight, and be content with modest acclaim. (Thus, in this paper, looking for all the support we can get, we elect to give credit where it is due, particularly to Darden and Hull, rather than reformat the arguments slightly and pass them off as our own.)

Another feature of support and cooperation involves the formation of research groups and cliques. Many scientists operate in teams to pursue a program of research and disseminate ideas. One useful function of this is that in a team, there is more likely to be a challenge to bias; there will be different biases, since even scholars who are in general agreement can hardly fail to be conceptually heterogeneous. Because all members of a group attach their names to any research they do, each has a vested interest in making sure that the conduct of the inquiry and the nature of the views expressed are not vulnerable to attack on rational grounds. Thus, the order of the day is cooperative mutual criticism. Isolates, by contrast, have no one to challenge their bias before submitting their work to the scrutiny of the field.

Isolates can, of course, circulate drafts of a paper to colleagues, but by that stage, the experiment has already been carried out and it is too late to modify the theoretical orientation, the hypotheses, the tests, and so on.

It does not seem that SLA researchers typically form research groups. Work is done with students quite frequently, but it is less common to see groups of faculty coming together to pursue a particular program of research. Even the well-known ZISA group, which operated in the late 1970s, featured only one professor (Jurgen Meisel); the others were, at the time, students. Contrast this with the norm in, for example, aphasia research. In this field, it does not take any great familiarity with the literature to identify important research groups.

A further benefit of membership of a research group, or of a loose federation is that it confers solidarity such that attack from outsiders is met by an organized response. It is practically impossible to think of an example of this in SLA. One might point to the instance when Clahsen and Muysken (1986) issued a challenge to proponents of UG/L2, arguing that the adult second language learner does not have access to UG; in this case, it might be contended, the response from a group of like-minded scholars was rapid, laying down their 'party' line (du Plessis, Solin, Travis, and White 1987). But this 'group' has (so far as we know) no tradition of working on an SLA research agenda together.

Although there may be good reasons why SLA researchers tend to operate as isolates (there are not many of them and they are widely scattered), it may be that an important element of the social institution of science has not yet emerged in SLA. Awareness of this is the first step in considering whether or not it is a goal the field should work towards.

The fact that scientists must use each other's work has important implications. They have to trust the accuracy of that work. If experiments are incompetently performed or dishonestly reported, it could take years for the error to be uncovered. Scientists committing years of effort to research built upon erratic findings will find out too late that they have wasted their time. Thus, the punishment for sloppiness and fabrication is severe, the severity being dependent on how much harm resulted. Condign punishment for such infractions is necessary to protect the institution. (Theft, as Hull (1988: 311) points out, is less seriously punished because only the individual is harmed, not the institution of science which still benefits from accurate information no matter who gets the credit.)

Often, exposure comes from replication. Repeated failure to replicate can result in dishonor. Campbell (1988: 500) offers a vivid, though tragic, example of how dire the shame of exposure can be: 'in physiological psychology recently, the chronic failure of others to replicate a dramatic finding seems likely to have been a major determination of a fine young scientist's suicide while a tenured full professor at a major university'. In SLA, as many have commented over the years (for example, Lightbown 1985), major findings are rarely replicated. This is another clear area where the social structure of SLA has not yet fully emerged. Where there is little prospect of being proven wrong, there is less disincentive to produce shoddy work.

Scientists, let us say by way of concluding this section, cooperate. They cooperate not out of altruism but because it is in their best interests and it happens to be in the best interests of the institution of science for them to do so. Significant forms of cooperation are not yet characteristic of SLA.

4.3 *Checking*

Just as important as cooperation among scientists is competition. First and foremost, this is crucial for the mutual testing of knowledge claims. The knowledge that rivals will look for weaknesses in one's work ensures that one tries to be meticulous in experimentation and highly cautious in argumentation. Competition, that is to say, provides quality control. In addition, it ensures that scientists work extraordinarily long hours and cram as much work as possible into a couple of productive decades.

But to compete, it seems, requires considerable tenacity. Scientists must be prepared to fight to get their views accepted, to withstand assault, and to secure scarce funding resources. Hull cites surveys showing that scientists are less

aggressive than members of other professions, suggesting that although perhaps by nature mild, success in the institution demands they enter the lists!

There are many vivid illustrations of combative behavior in Hull's account, involving almost every major protagonist in his story of the rivalry between the cladists and the pheneticists. One of these, Ehrlich, was certainly prepared to defend his views vigorously. Having predicted, in the early 1960s that taxonomy would be quite transformed by the end of the decade by the advent of computers, he would brook no objection:

> At the St. Louis meeting, when one taxonomist asked indignantly, 'You mean to tell me that taxonomists can be replaced by computers?', Ehrlich responded, 'No, some of you can be replaced by an abacus.' Thereafter, Ehrlich did not consider the give-and-take after a paper truly successful unless he brought at least one taxonomist to the point of tears. (Hull 1988: 121)

Competition, then, it is claimed, assists science insofar as it promotes rational argument and precise methodology. Even straw-man arguments and 'whistle-blowing' appeals to falsifiability have a point in that they force scientists to commit themselves. And abrasive behavior, however unconscious or intuitive, apparently helps the process along—it is obviously difficult to ignore.

The routine imminence of attack, it appears, serves to ensure that research is more carefully motivated and conducted. Relating the competition discussion to SLA would be easy enough, but it is not readily apparent what the implications might be. That is, there are well-known examples (though not many) of relevant critiques in this context, for example, Bley-Vroman and Chaudron's (1990) condemnation of Flynn, and Gregg's (1984) *summa contra* Krashen. However, it is not clear that because (on Hull's account) the natural sciences are disputatious, SLA should somehow grasp the nettle. As in the above discussion of cooperation (section 4.2), awareness is the important first step.

5. DISCUSSION

What we have tried to do in this paper is to address the question of the applicability of philosophers' insights regarding science to the emerging discipline of second language acquisition. We have focused on discovery, the generation of new hypotheses, as an important component (interrelated with others) of theory construction. We looked at the role of reason in discovery, examining several promising strategies in some detail. Similarly, we considered the influence of social factors, giving special attention to an account which stressed the coincidence of interests between individual scientists and the institution of science, an account which is driven by a complex interplay of credit, cooperation, and competition which operate as beneficial agents of scientific discovery.

Thagard (1989: 72) argues that we need well-worked-out theories of both 'hot cognition' (interests) and 'cold cognition' (reasoning). His view is that

> Even if a scientist is driven by personal motivations of success and fame, he or she has to present research to the rest of the scientific community in terms of its experimental

> and theoretical merits ... because of the institutional commitment of science to experimental evidence and explanatory argument, science as a whole is able to transcend the personal goals of its fully human practitioners who acquire the motivation to do good experiments and defend them by rational argument. (Thagard 1989: 80)

We share Thagard's view, and submit that Hull's (1988) account is at least a plausible explanation of how 'interested' people can nevertheless do rational work. Having such a perspective can be useful to an emerging discipline in that it can help us consider whether or not all the important pieces are in place that constrain individuals to serve the process of discovery in the field.

Regarding the role of the reasoning that we have broached, Darden (1991) suggests that there might be several advantages in persisting with the program of research that tries to identify and codify strategies. Perhaps some are better than others; maybe some should be used prior to others; are some related to ultimate failure? or success? We may assume that some scientists (including some SLA researchers) are better than others; attempts at explicit articulation of the good strategies that they use might be beneficial to colleagues and students. They might reduce the time that students spend as apprentices; for instance, with regard to the strategy 'invoke a theory type', it might be helpful to give our students practice in abstracting problem-solving schemata from exemplary, concrete solutions. This kind of attention could serve to give students a sense of the cutting edge (Darden 1991: 279).

Perhaps the most fertile approach to reasoning strategies is to look upon them as forms of 'method' (broadly interpreted). Typically, when we talk of method in SLA, we are concerned with such matters as experimental design and statistics, that is, a set of important but limited routines, the procedures familiar to us all. We might elaborate our concept of method to include reasoning strategies. For example, to our knowledge, no methods courses in SLA and related fields require students to consider the value of idealization as a strategy for theory construction in complex areas even though it is fundamental to the UG/L2 approach. If the purpose of methods is to understand how to do science, then a greater appreciation of strategies used by scientists seems an appropriate focus. It does not seem to us a wild surmise that the history of science can be mined for strategies that have proven their worth, but which (for whatever reasons) have not been codified.[8]

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NOTES

[1] There is another sense of 'prescriptiveness' that pervades much current discussion: the idea that by examining how science is actually done, we might observe that certain

approaches are more successful than others which, in turn, might lead to normative statements about how science *should* be done. In principle, this might be possible, but since the level of understanding that would sanction prescription is so remote, we do not explore the issue here.

[2] After all, if SLA commonly used the seven strategies we examine, our paper would serve no purpose, effectively preaching to the converted.

[3] See Piattelli-Palmarini (1989) for a discussion of selection theories and language acquisition.

[4] Colleagues have, on the whole, been underwhelmed by the impact that SLA flowcharts and models have had on theory construction, one anonymous reviewer adding that the Hurford example is unusual. Although we undertake no such analysis here, a sensible way to proceed would be to consider what it is that makes the Hurford case unusual. For instance, many an SLA flowchart is presented as a *fait accompli* and no manipulation follows. Hurford, by contrast, makes very specific modifications which are immediately tested.

[5] Classic works by these authors are Merton (1973) and Latour and Woolgar (1979).

[6] The bitter rivalry between E. D. Cope and O. C. Marsh is well known, and is, of course, the important point for our purposes. The fact that Cope made provision in his will for his skull to be measured is a matter of record (see, for example, Osborn 1931). The claim that there was a link between the rivalry and the will was made on a Public Broadcasting Service (Detroit, USA) TV program on 28 November 1992, entitled 'Dinosaurs!'

[7] One reviewer was concerned that in the absence of any comment about where a 'theory of practice' might come from, a fair proportion of the *Applied Linguistics* readership might feel rather sidelined. There are two points here: one is journal policy; the other is the relationship between SLA theory and practical issues. To take the latter point first, there is no reason why SLA theorists (or theorists from any other discipline) should be expected to make some kind of provision for practical matters. No one expects a theoretical physicist to attend to engineering nor theorists working on the Human Genome Project to attend to medicine. When it comes to the human sciences, there is greater confusion. Theoretical linguists have long been arguing that their work is carried on at a level of abstraction that idealizes away from the complexities of the 'real' world, but this view has hardly gone unchallenged. In SLA, the same divisions are increasingly apparent. If SLA is to take its cue from the natural sciences (as Chomskyan linguists think linguistics should do), then it cannot be guided, inhibited, or distracted by practical concerns.

If readers of *Applied Linguistics* feel sidelined by this (and this is the second point), they have perfectly reasonable grounds for complaint. If the purpose of SLA theory is better theory, SLA may very well be outside the brief of the journal. The policy of *Applied Linguistics* appears quite clear on this: papers should relate theory and practice. Therefore, theoretical SLA (which does not consider practice) has no place in it.

[8] This would be even more persuasive if Nickles (1989) is correct. He advances the view that method is essentially a set of streamlined devices that have worked well in disciplines involved in 'normal science', Kuhn's (1970) term for the day-to-day work that takes place under the guidance of a dominant theory. In 'revolutionary' science, when a competing theory destabilizes the prevailing orthodoxy, and we might plausibly add, when a discipline has a short history, it is not entirely clear that the best known methods are the best adapted. In such a time, Nickles speculates, awareness of other strategies is acutely important.

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