Title	Is the Ontological Commitment to "Rules" Really Preferable to Linguistic Science? : A Critical Assessment of the Symbolist-Connectionist Debate on the "Learning of the Past Tenses of English Verbs
Author(s)	KURODA, Kow
Citation	= Papers in linguistic science (1995), 1: 15-29
Issue Date	1995-12
URL	http://dx.doi.org/10.14989/66929
Right	
Type	Departmental Bulletin Paper
Textversion	publisher

Kyoto University

Is the Ontological Commitment to "Rules" Really Preferable to Linguistic Science?
A Critical Assessment of the Symbolist-Connectionist Debate on the "Learning of the Past Tenses of English Verbs"

Kow Kuroda
Kyoto University
E-mail: kuroda@hi.h.kyoto-u.ac.jp

0. Abstract

This paper aims to assess critically the debate on the "learning of the past tenses of English verbs" between those *symbolists* who are proponents of *symbol manipulation* model of the language and those *connectionists* who are proponents of *parallel distributed processing* model of it, thereby suggesting that between such extreme positions there is still a realistic position that linguists can take to blur the debate. It is when linguistics is conceived as a *phenomenology of the language*, as much as chemistry, for example, is a phenomenology of the nature. If the position is adopted, theoretical constructs such as "rules," "schemes," "patterns," can be defended only for a negative reason: without them we could not describe linguistic phenomena correctly and effectively enough to draw any scientifically significant generalizations. To take this position is to take a perspective from which rules are no more than a *heuristic device*. This is, I claim, the most realistic position in which scientific identification of linguistics, or linguists' *looser* talks about the language, can be maintained.

1. What is the Symbolist-Connectionist Debate, and Why?

The debate that I want to critically assess below emerged when Rumelhart and McClelland (1986, p. 267) claimed as follows:

(1) ... We have, we believe, provided a distinct alternative to the view that children learn the rules of English past-tense formation in any explicit sense. We have shown that a reasonable account of the acquisition of past tense can be provided without recourse to the notion of a "rule" as anything more than a *description* of the language. We have shown that, for this case, there is no *induction problem*. The child need not figure out what the rules are, nor even that there are rules.

What they attack here is obviously the celebrated paradigm of *symbol manipulation* that was introduced by Noam Chomsky (1957, 1975) in linguistics under the title of "generative grammar," providing a "revolution" in that field,[1] and then came into fashion in some other branches of psychology such as "cognitive science" (Fodor, 1983; Phylyshyn, 1984), and "artificial intelligence" (Newell and Simon, 1972) as well. The symbol manipulation paradigm, or simply *symbolist* paradigm, has long been cherished since, probably because it embodied one methodological assumption on the nature of human mind: mental behaviors of human being can be described and explained by the

---

[1] For the notion of "(scientific) revolution," see Kuhn (1970). And for detailed information of the revolution, see, for example, Newmeyer (1986), Harris (1991).

system of explicitly specified rules.

With the advent of "parallel distributed processing" (PDP) paradigm, however, the hegemony of the symbolist paradigm began to lose, for radical proponents of the PDP paradigm, or *connectionists*, so to be put, began to challenge symbolists' crucial methodological assumption mentioned just above. It should be noted, to be fair, that connectionism is an approach of a different tradition, which originates in McCulloch and Pitts (1943) style of the brain science, and most of the claims that connectionists explicitly made are relevant to linguistics basically in indirect ways. Some claims, however, really reach across the borders between fields to linguistics. One of them is the empirical status of what we have referred to as rules. Rumelhart and McClelland (*op. cit.*), for example, showed, I believe, that, without any implementation of so-called past tense formation rules such as "change internal vowel *-i-* to *-a-*," PDP models which make use of "pattern association" between two internal states on prepared neural network can simulate the English speaker's *performance* of past tense formation, even the *acquisition* of that formation. They thereby posed a question of what it is that human mental behaviors can be described in terms of rule, and claimed against symbolist, as can be seen in (1), that rules may *describe* mental behaviors such as past tense formation, but never *explain* them.[2]

Symbolists such as Fodor and Phylyshyn (1988), Pinker and Prince (1988), Lachter and Bever (1988) soon accepted this Rumelhart and McClelland's challenge, trying to undermine theoretical thrusts of the PDP model. It is not my own interest in this paper, though, to assess all of those counter-criticisms, for I believe they are virtually the same: they singly insist that rules can be explanation. So, I will take the counter arguments by Pinker and Prince (*op. cit.*) for instance, which I think is the most detailed and sophisticated, and try to assess its defensive power. I will address §2 to the examination of Pinker and Prince's objection to the claim explicit in (1), and reveal an ideological character of the debate, concluding that their defense is not successful on the ground that Pinker and Prince only act as if there were no burden of proof on symbolists' part. In §3, for the purpose of making more clear what is crucial in the debate, I will examine an extended portion of English verb inflection in as theory-neutral a way as possible, ultimately proposing that the symbolist-connectionist debate can be managed properly only if one understands what is an explanation on phenomenological scales. In §4, I will provide additional arguments for the direction of solution that I suggest.

2. Ontological Commitment to "Rules" of Language

2.1 Pinker and Prince's Counter-attacks on the PDP Model

Pinker and Prince (1988, pp. 81-82) enumerate the following twelve points arguing against the claim in (1):

(2) a.   Rumelhart and McClelland's actual explanation of children's stages of regularization of the past tense morpheme is demonstrably incorrect.

b.   Their explanation for one striking type of childhood speech error is also incorrect.

c.   Their other apparent successes in accounting for developmental phenomena either have nothing to do with the model's parallel distributed processing architecture, and can easily

---

[2]For connectionists' specific models and claims, see Rumelhart, *et al.* (1986). McClelland, *et al.* (1986), and Davis, *ed.* (1992). For their goals and related philosophical issues, see Clark (1990). The approach called here *connectionism* is also dubbed *associationism* for its theoretical relevance to the theory of associative memory, which was founded by Quillian (1967), and developed notably by Kohonen (1977).

be duplicated by symbolic models, or involve major confounds and hence do not provide clear support for the model.

d.   The model is incapable of representing certain kinds of words.

e.   It is incapable of explaining patterns of psychological similarity among words.

f.   It easily models many kinds of rules that are not found in any human language.

g.   It fails to capture central generalizations about English sound patterns.

h.   It makes false predictions about derivational morphology, compounding, and novel words.

i.   It cannot handle the elementary problem of homophony.

j.   It makes errors in computing the past tense forms of a large percentage of the words it is tested on.

k.   It fails to generate any past tense form at all for certain words.

l.   It makes incorrect predictions about the reality of the distinction between regular rules and exceptions in children and in languages.

Pinker and Prince's objection, however careful in pointing out deficiencies of the alleged model, seems to me just a red herring; since it is not difficult to see that they have at best succeeded in their arguments in showing that the alleged model is not *yet perfect*.

There is no model that has any limitations, but some limitations can be ameliorated with appropriate modifications. Note that those deficiencies enumerated in (2), which of course may not be exhaustive, divide into the three categories: I) deficiencies which validate the PDP model's current imperfectness (i.e., d, f, i, j, k); II) that which is almost irrelevant to the issue (i.e., f),[3] and III) those which validate the PDP model's incorrectness only with certain premises (i.e., a, b, c, e, g, h, l). Putting aside irrelevant ones, all within the category I are *factual*, but all that fall within III are *theoretical*; since each deficiency of the category III is incorrect as far as it is either *explanation*, or *generalization*, or *prediction*, and all of the deficiencies that may not be modifiable are all of the category III.

It should be also noted, however, that Pinker and Prince reject only *eliminative* one of the following three kinds of connectionism: (i) *implementational* connectionism that explores such PDP models that 'they would characterize the elementary information process provided by neural networks that serves as the building blocks or rules or algorithms' (Pinker and Prince, *op. cit.*, pp. 77-78); (ii) *eliminative* connectionism that explores such PDP models that 'they are will replace symbol-processing models as explanations of cognitive process' (*ibid.*); and (iii) *revisionist* connectionism is the intermediate between (i) and (ii), and seek a 'PDP theory [that] could lead to fundamental new discoveries about the character of symbol-processing, rather than implying that there was no such thing.' (*ibid.*)

To sum up, any PDP model may be rejected by symbolists only if such eliminative claims as in (1) accompany with it. Thus, Pinker and Prince's objection is indirect in that it is not directed to Rumelhart and McClelland's conclusion like (1) as one of the implications of their model. Hence, the debate is rather *ideological* than *empirical*.

2.2 What Symbolists Fight for and Connectionists Fight against

The structure of the debate can be simplified like this: Given an open question of why the speaker of English utters *kicked*, for example, as the past form of the verb *kick*. Symbolists, on the one hand, postulate that the question can be answered like this: (1) it is because there is such a rule as

---

[3]This is irrelevant because it is not emprically clear how much human beings *learn* at all.

"add -ed to the base *kick*," which, accidentally, results in such a pair. Connectionists, on the other hand, postulate that the same question can be answered like this: (2) it is because there occurs a pattern association in the brain such that it can be accidentally described by the rule of "adding -ed to the base." Thus, the debate looks like a vicious circle of whether the rule or the performance is the first.

The problem may be enigmatic, since it seems to constitute a vicious circle, but let us pose here another question: Which is more explanatory? The "explanation" in the symbolist paradigm crucially relies on a theoretical construct called "rule," and that in the connectionist paradigm on another construct called "association." Note that an explanation is a relationship of two things: something that is to be explained (call this *explanandum*) and something that is to explain (call this *explanatum*). The phenomenon of past tense formation is their common explanandum; but there are two ways of explanation because it is characterized by means of different *explanata*, rule and association in symbolist and connectionist paradigms respectively. Although there is no single measure to compare two different explanations, I want to single out one measure here. Granted one explanation is more explanatory than another if, other things being equal, one assumes less than another. According to this measure, symbolists' rule-based explanation can be said to be less explanatory than connectionists' association-based one, because the postulation of rules is more assumptive than that of associations. While associations are empirical objects explicitly definable as one of the properties of the neural network, and ultimately of the brain, there is no empirical definition of what rules are, but an implicit intuition of them. It seems to me quite doubtful whether the question can ever be answered without falling into a tautology, of what rules of language are, which is the very question that I believe Rumelhart and McClelland (*op. cit.*) pose. Thus, the most crucial point in the debate is this: It may be possible to define what are rules in terms of pattern association, but not vice versa, which led Rumelhart and McClelland to make such eliminative claim expressed in (1). Accordingly, it is not connectionists but symbolists who have the burden of proof in the debate, for what is really alleged in the debate is, contrary to the distractive arguments of Pinker and Prince (*op. cit.*) and other symbolists mentioned above, not the empirical adequacy of the alleged PDP model, but one *truism* that has been unquestioned: the description of language in terms of rule can be *identified* with the grammar, or the knowledge of language that symbolists simply have assumed to be internalized somewhere in the brain somehow by virtue of the auxiliary construct called "competence" (Chomsky, 1965), of which there is no justification, and may be suffering from the "first order isomorphism fallacy" (Kugler, Turvey and Shaw, 1982).

3. Revisiting the Past Tense Formation, Focusing on the Verbs Ending -*ing* or -*ink*

In this section, I want to investigate one more aspect about the past tense formation that the availability of rules is not sufficient for verbs to be inflected *correctly*, thereby basically arguing for a connectionistic view on the ground that the fact could be explained by the competition among the inflectional patterns available in that framework.

I will try to show that the *grammatical* formation is, at least in the case of verb inflection, no more than the *most acceptable* formation, and the notion of grammaticality should be understood as strictly a relative notion, because, for a verb to be correctly, i.e., *grammatically*, inflected, there must be *selection of one rule as the correct one*. Accordingly, what is really explanatory in the case of verb inflection is not the explanation of the *availability of rules*, which may be provided more easily in the symbolist paradigm than the connectionist paradigm, but the explanation of the *competition among the rules available*, which I doubt that symbolists have any seriously effort to account for, sheltering

under the Chomskyan "competence" umbrella (Chomsky, 1965). I will address the brief discussion in §4 to this most important problem.

3.1 Some Preliminaries

Rumelhart-McClelland (*op. cit.*) modeled only a small portion of English speaker's performance: the pattern association of the *base* and the *past* forms of English verbs. Symbolists criticize this quantitative insufficiency of the alleged model as well. To make this kind of criticisms less relevant, I want to extend the scope of description to the inflectional behavior of the triplet of forms, {*base, past, past participle*}, of English verbs. Such extension, though, sets another kind of limit, since it increases complexity of the data to be handled. So, I will examine in this paper exclusively the inflectional behavior of the English verbs that end either -*ing* or -*ink*, letting other irregular paradigms such as {*fly, flew, flown*}, {*take, took, taken*} out of scope.[4] I believe, however, such limitation on coverage will not harm essential points of the following arguments.

3.1.1 Definition of the Verb Inflection Scheme *H-X*

To make comparison between paradigms easier to handle, I will adopt the notational convention of *Verb Inflection Scheme* (VIS) denoted by *H-X*, defined in the following lines for past and past participle formation in English.

Consider, for example, two inflectional paradigms of *sing* and *sink*. The paradigm of *sing* can be designated by the triplet {*sing, sang, sung*}, and that of *sink* by {*sink, sank, sunk*}, each consisting in an array of three forms. Let *H* be a variable which stands for the header chunk of phonemes that is maximally shared among the three forms ($H = s$- in this case). Thus, two inflectional paradigms {*sing, sang, sung*} and {*sink, sank, sunk*} can be designated in simplified formats of H-{-*ing*, -*ang*, -*ung*} and H-{-*ink*, -*ank*, -*unk*}, respectively. Let symbols such as A_1 and A_2 stand for reduced arrays {-*ing*, -*ang*, -*ung*} and {-*ink*, -*ank*, -*unk*}, respectively. Thus, their paradigms are denoted in simplified formats, H-A_1 and H-A_2 ($H = s$-), respectively. Since *H* in *H-X* is a variable ranging over such sequences as s-, st-, str-, and *X* in *H-X* is a variable ranging over predefined symbols such as A_1 = {-*ing*, -*ang*, -*ung*}, A_2 = {-*ink*, -*ank*, -*unk*}, this convention facilitates exhaustive description of certain portion of English verb inflection.

3.1.2 Description of Relevant Patterns

Among the inflections of English verbs that end either -*ing* or -*ink*, we can identify the following four patterns, *A, B, C* and *D*, and their eight subpatterns as in (3), which exhaust all the inflectional possibilities of those verbs so ending.

(3)   Pattern *A* consists of the two subpatterns: H-A_1, where A_1 = {-*ing*, -*ang*, -*ung*}, and H-A_2, where A_2 = {-*ink*, -*ank*, -*unk*}.

Pattern *B* consists of the two subpatterns: H-B_1, where B_1 = {-*ing*, -*ung*, -*ung*}, and H-B_2, where B_2 = {-*ink*, -*unk*, -*unk*}.

Pattern *C* consists of the two subpatterns: H-C_1, where C_1 = {-*ing*, -*ought*, -*ought*}, and H-C_2, where C_2 = {-*ink*, -*ought*, -*ought*}.

Pattern *D* (regular) consists of the two subpatterns: H-D_1, where D_1 = {-*ing*, -*inged*, -*inged*}, and H-D_2, where D_2 = {-*ink*, -*inked*, -*inked*}

---

[4] For more detailed classification of past formation in English, see Bybee and Slobin (1982).

The VIS defined above are useful not only for its economy in description but also for its flexibility in extension. Although not purported in this paper, it is easy to incorporate into the VIS other inflectional patterns of those verbs which end either *-in* or *-im* by enlarging its pattern index. Thus, such paradigms as {*beg-in, beg-an, beg-un*} can be designated by $H\text{-}A_3$ ($H = beg\text{-}$; A_3 = {*-in*, *-an*, *-un*}), for example, and {*sw-im, sw-am, sw-um*} by $H\text{-}A_4$ ($H = sw\text{-}$; A_4 = {*-im*, *-am*, *-um*}), each of which is the generalization of the pattern $H\text{-}A$.

For each of the patterns that a VIS designates, symbolists postulate a rule. Given a rule of English grammar that instructs us to "change internal vowel *-i-* to *-a-*" to form its past tense, its application defines the same morphological object as that designated by pattern A_1. Likewise, another rule instructing to "change the internal vowel *-i-* to *-u-*" defines the same object as that described by the pattern B_2. In short, the VIS introduced above denotes the same range of morphological phenomena in different terms. What I intend here is, however, not to replace the rule-based description with the scheme-based one, but to compare as fairly as possible the two claims by symbolists and by connectionists by means of an intermediate description language of VIS.

3.1.3 Universal Rule Scheme and the Notion of "Family of Rules"

It is rather trivial that both of past formation and past participle formation can be generalized in terms of the scheme $X \xrightarrow{r_i} X' \, / \, X = \ldots$ (call this *Universal Rule Scheme* (URS)), in which we interpret the left-hand part describes the association of given two forms, X and X', via a specified rule r_i. In other word, it describes what happens when the rule r_i is applied to X. The right-hand part describes the condition on the application of the rule r_i.

The past formations of *kick* and *sing* require different rules; one is to "add *-ed* to V," and another to "change internal vowel *-i-* to *-a-*." It is obvious that they are rules of a single rule of past formation. Thus, past formation constitutes a *family of rules*. Rules in the URS are so indexed as to indicate the identity within a specified family of rules. We say that a rule is available within URS if, and only if, there is a family of rules which contains the case. Consider this with the case of gerund formation. It is preferable to say that the gerund formation also constitutes a family of rules, though it consists of one and only one rule, "add *-ing* to the base," since we want to hold that the formations of the past, the past participle and the gerund of verbs are grammatical phenomena of a same sort. What is crucial to this point is that there must be an *identity* according to which phenomenologically distinct rules are equally rules of a single rule. It is family of rules that determines such identity of a word formation.

3.1.4 Notes on Past Participle Formation

The rule of past participle formation, "add suffix *-en* to the base," can also be formulated in terms of the URS. The rule alters *take*, for example, to *tak-en*, with truncation of ending vowel *-e* involved. Note however that some verbs have two alternate past participle forms. For example, *sink* alters either to *sunk* or *sunk-en*; *drink* alters either to *drunk* or *drunk-en*; and *get* alters either to *got* or *gott-en*, where the *-en* suffixation has to do with only one of the two. Thus, past participle formation is a composition of two distinct rules in one: one rule is to apply first to alter internal vowel in the base, which is similar to the case of past formation; another is to apply secondly to add suffix *-en* to the base already altered (including the case where there has been no vowel alternation).

3.2 Examination of Inflectional Paradigms by Means of Verb Inflection Schemes

In the examination that follows, I will try to demonstrate one thing, though indirectly: A

symbolist theory of grammar that exclusively relies on "rules and representations" is not really explanatory, for, though it circumscribes *what can happen* in past or past participle formations, it does not circumscribe *what cannot happen* in a significant sense. Specifically, rules-and-representations theory is not a theory of language strong enough to predict what is the past form of such imaginary verbs as *dring*, whereas the PDP model would tell something about it. It is obvious that the word *dring* will inflect, given it is a verb, according to one of the rules available, but the theory would not tell which rule is most likely to apply. In that theory, *dring* may inflect to form *drang* (like *drink*) or *drought* (like *bring*), and, what is worse, there would be nothing that prevents it to inflect to form *drew* (like *fly*). Furthermore, it would not tell in an explanatory way why the verb *dry* does NOT inflect to form *drew* despite the obvious phonological similarity with *fly*. In short, the rules-and-representations theory is not really explanatory in that it would not provide any interesting account for the "prototypicality" in past and past participle formations, nor would it provide any direct account for the "competition" among the paradigms available.

3.2.1 Case of *sing~sink* Pair

Let us begin our examination with the cases (4) and (5), where $H = s\text{-}$. When $H = s\text{-}$ in $H\text{-}X$, the following pattern of acceptabilities can be obtained:

(4)   a. $H \cdot A_1 = \{s\text{-}ing, s\text{-}ang, s\text{-}ung\}$,

       b. $^*H \cdot B_1 = {}^*\{s\text{-}ing, s\text{-}ung, s\text{-}ung\}$,

       c. $^*H \cdot C_1 = {}^*\{s\text{-}ing, s\text{-}ought, s\text{-}ought\}$,

       d. $^*H \cdot D_1 = {}^*\{s\text{-}ing, s\text{-}inged, s\text{-}inged\}$

(5)   a. $H \cdot A_2 = \{s\text{-}ink, s\text{-}ank, s\text{-}unk\}$,

       b. $^*H \cdot B_2 = {}^*\{s\text{-}ink, s\text{-}unk, s\text{-}unk\}$,

       c. $^*H \cdot C_2 = {}^*\{s\text{-}ink, s\text{-}ought, s\text{-}ought\}$,

       d. $^*H \cdot D_2 = {}^*\{s\text{-}ink, s\text{-}inked, s\text{-}inked\}$

where the symbol * attached to the paradigm is to indicate that it is unacceptable as the inflectional paradigm of the word in the lexical system of standard English.

The possibility of inflection above shows that patterns except for A_1 and A_2 are all unacceptable if $H = s\text{-}$ in that there are distinct two and only two verbs so inflected.

As the comparison with $s\text{-}\{\text{-}eek, \text{-}ought, \text{-}ought\}$ makes clear, any two verbs inflecting in $H\text{-}C$ lose lexical distinctions both in past and past participle forms. I suspect that this factor makes $H\text{-}C$ the least available.

3.2.2 Case of *sling~slink* Pair

When $H = sl\text{-}$ in $H\text{-}X$, then the following pattern of acceptabilities will result:

(6)   a. $^*H \cdot A_1 = {}^*\{sl\text{-}ing, sl\text{-}ang, sl\text{-}ung\}$

       b. $H \cdot B_1 = \{sl\text{-}ing, sl\text{-}ung, sl\text{-}ung\}$

       c. $^*H \cdot C_1 = {}^*\{sl\text{-}ing, sl\text{-}ought, sl\text{-}ought\}$

       d. $^*H \cdot D_1 = {}^*\{sl\text{-}ing, sl\text{-}inged, sl\text{-}inged\}$

(7)   a. $^*H \cdot A_2 = {}^*\{sl\text{-}ink, sl\text{-}ank, sl\text{-}unk\}$

b. $H \cdot B_2 = \{sl\text{-}ink, sl\text{-}unk, sl\text{-}unk\}$

c. $^* H \cdot C_2 = ^*\{sl\text{-}ink, sl\text{-}ought, sl\text{-}ought\}$

d. $^* H \cdot D_2 = ^*\{sl\text{-}ink, sl\text{-}inked, sl\text{-}inked\}$.

The distribution of acceptabilities indicates that, if $H = sl\text{-}$, the scheme $H\text{-}B$ is valid in both cases of $H\text{-}ing$ (index 1) and $H\text{-}ink$ (index 2). Compare this pattern of acceptabilities with that of *sing~sing* pair in §3.2.1, where the scheme A is selected for X in $H\text{-}X$ irrespective of their difference in pattern index. In this case, the same symbol B is selected for X of $H\text{-}X$, irrespective of the value of H in $H\text{-}X$. We find there a weak tendency of invariance in the selection of scheme, which can be observed also in the cases to be examined.

3.2.3 Case of *sting~stink* Pair: Identification of the "Superpattern"

When $H = st\text{-}$ in $H\text{-}X$, then the following pattern of acceptabilities will be obtained:

(8)   a. $^* H \cdot A_1 = ^*\{st\text{-}ing, st\text{-}ang, st\text{-}ung\}$

b. $H \cdot B_1 = \{st\text{-}ing, st\text{-}ung, st\text{-}ung\}$

c. $^* H \cdot C_1 = ^*\{st\text{-}ing, st\text{-}ought, st\text{-}ought\}$

d. $^* H \cdot D_1 = ^*\{st\text{-}ing, st\text{-}inged, st\text{-}inged\}$

(9)   a. $H \cdot A_2 = \{st\text{-}ink, st\text{-}ank, st\text{-}unk\}$

b. $H \cdot B_2 = \{st\text{-}ink, st\text{-}unk, st\text{-}unk\}$

c. $^* H \cdot C_2 = ^*\{st\text{-}ink, st\text{-}ought, st\text{-}ought\}$

d. $^* H \cdot D_2 = ^*\{st\text{-}ink, st\text{-}inked, st\text{-}inked\}$

The distribution of acceptabilities indicates that, if $H = st\text{-}$, rule application is ambiguous, for two patterns $H\text{-}A$ and $H\text{-}B$ are both correct with respect to the inflection of *stink*. This means that there is less need to distinguish between patterns $H\text{-}A$ and $H\text{-}B$. The paradigm of *shrink* show this kind of ambiguity, too. For comparison, verbs which solely obey pattern A count: *ring, drink, spring, sing,* and *sink*, whereas those which solely obey pattern B count *hang, cling, fling, sling, string, wring,* and *sting*. Viewing such ambiguity, it is probably insignificant to distinguish exactly availabilities of patterns $H\text{-}A$ and $H\text{-}B$, since, even if it is ever successful, it will generalize little. I would rather think that two patterns $H\text{-}A$ and $H\text{-}B$ are not utterly distinct, but form a *superpattern*.

3.2.4 Cases of *ring~rink* and *wring~?wrink* Pairs: Inflectional Differentiation

When $H = r\text{-}$ in $H\text{-}X$, then the following pattern of acceptabilities will result:

(10)   a. $H \cdot A_1 = \{r\text{-}ing, r\text{-}ang, r\text{-}ung\}$

b. $^* H \cdot B_1 = ^*\{r\text{-}ing, r\text{-}ung, r\text{-}ung\}$

c. $^* H \cdot C_1 = ^*\{r\text{-}ing, r\text{-}ought, r\text{-}ought\}$

d. $^* H \cdot D_1 = ^*\{r\text{-}ing, r\text{-}inged, r\text{-}inged\}$.

(11)   a. $^* H \cdot A_2 = ^*\{r\text{-}ink, r\text{-}ank, r\text{-}unk\}$

b. $^* H \cdot B_2 = ^*\{r\text{-}ink, r\text{-}unk, r\text{-}unk\}$

c. $^* H \cdot C_2 = ^*\{r\text{-}ink, r\text{-}ought, r\text{-}ought\}$

d. $H \cdot D_2 = \{r\text{-}ink, r\text{-}inked, r\text{-}inked\}$

The distribution indicates that the invariance in the selection of X in H-X does not hold; but it is rather because the word *rink* is not a verb in the lexicon of standard English. So, it will not be false to predict that *rink* would have inflected as in H-A_2 if the word was a verb.

Although such irregularity may be accidental, it is still worth of some considerations. Consider the fact that such irregularity tends to occur in cases of unvoiced consonant -*ink*. Given the minimal phonological opposition between H-*ing* and H-*ink*, it is the latter case that tends to be a noun. The tendency exists, I presume, not only because of phonological reasons, but of semantic, or, more explicitly, conceptual ones (e.g., different sound symbolisms in -*g* and -*k*), as Bybee (1985, 1994) typically claims.

Furthermore, when H = *wr*-, then the following patterns of acceptability will be obtained:

(12) a. $^*H \cdot A_1$ = $^*\{wr$-*ing*, *wr*-*ang*, *wr*-*ung*$\}$
      b. $H \cdot B_1$ = $\{wr$-*ing*, *wr*-*ung*, *wr*-*ung*$\}$
      c. $^*H \cdot C_1$ = $^*\{wr$-*ing*, *wr*-*ought*, *wr*-*ought*$\}$
      d. $^*H \cdot D_1$ = $^*\{wr$-*ing*, *wr*-*inged*, *wr*-*inged*$\}$

(13) a. $? H \cdot A_2$ = $?\{wr$-*ink*, *wr*-*ank*, *wr*-*unk*$\}$
      b. $? H \cdot B_2$ = $?\{wr$-*ink*, *wr*-*unk*, *wr*-*unk*$\}$
      c. $? H \cdot C_2$ = $?\{wr$-*ink*, *wr*-*ought*, *wr*-*ought*]$
      d. $? H \cdot D_2$ = $?\{wr$-*ink*, *wr*-*inked*, *wr*-*inked*$\}$.

where the symbol ? is to designate the uncertainty of inflection. It is necessary because the lexicon of standard English does not contain the word *wrink* which forms a minimal phonological pair with *rink*. Thus, (13) is an imaginary paradigm. But I believe that it is still possible to predict what can happen if English has the word in the lexicon. If *wrink* is not a verb, the word will unambiguously inflect in H-D_2, and, otherwise, it is most likely that the word inflects as in H-B_2 keeping the contrast with H-B_1; with H-B_1 less likely, and H-C_2 the least likely. It can be so predicted if we assume that the lexicon of language has a tendency to be well organized with respect to the differentiation of a superpattern mentioned above.

What deserves mention here is the fact that *ring* in (11) and *wring* in (12) inflect differently, though they pronounce the same. I suspect that there is another principle in organizing the lexicon for making the two paradigms as distinct as possible.

3.2.5 Case of ?*dring*~*drink* Pair

When H = *dr*-, then the following patterns can be obtained, though (14) is imaginary:

(14) a. $? H \cdot A_1$ = $?\{dr$-*ing*, *dr*-*ang*, *dr*-*ung*$\}$
      b. $? H \cdot B_1$ = $?\{dr$-*ing*, *dr*-*ung*, *dr*-*ung*$\}$
      c. $? H \cdot C_1$ = $?\{dr$-*ing*, *dr*-*ought*, *dr*-*ought*$\}$
      d. $? H \cdot D_1$ = $?\{dr$-*ing*, *dr*-*inged*, *dr*-*inged*$\}$

(15) a. $H \cdot A_2$ = $\{dr$-*ink*, *dr*-*ank*, *dr*-*unk*$\}$
      b. $^*H \cdot B_2$ = $^*\{dr$-*ink*, *dr*-*unk*, *dr*-*unk*$\}$
      c. $^*H \cdot C_2$ = $^*\{dr$-*ink*, *dr*-*ought*, *dr*-*ought*$\}$
      d. $^*H \cdot D_2$ = $^*\{dr$-*ink*, *dr*-*inked*, *dr*-*inked*$\}$.

As well as the case of *wrink* in (13), the lexicon of standard English does not contain the word *dring*, but it is nevertheless possible to predict that if it is a noun the imaginary word will inflect as in (14d), and otherwise, it will inflect as either in (14a, b, c). I suspect that it is most likely that *dring* inflects as in (14a) if the contrast between (14a) and (15a) can be maximized, but its phonological similarity to *bring* may make it inflect as in (14c), which I will discuss in §3.2.6.

Although there seems to be a tendency that for each pairs of the two words that end *-ing* and *-ink*, that which ends *-ing* is more likely to be a verb, the existing verb in this case is unvoiced, *-ink* ending one. Of all the pairs of irregular verbs which form minimal contrasts between *H-ing/ H-ink*, apparent exceptions are only the following three pairs; **dring/drink*, **shring/shrink* and **thing/think*. Compare them with other pairs: *hang/*hank*, *fling/*flink*, *cling/*clink*, *sling/*slink*, *ring/*rink*, *bring/*brink*, *spring/*sprink*, *string/*strink*, *wring/*wrink*, *swing/*swink*.

3.2.6 Case of *bring~brink* Pair: Disturbing Inflection I

When $H = br$-, then the following pattern of acceptabilities can be obtained:

(16) a. $^*H \cdot A_1 = {}^*\{br\text{-}ing, br\text{-}ang, br\text{-}ung\}$
      b. $^*H \cdot B_1 = {}^*\{br\text{-}ing, br\text{-}ung, br\text{-}ung\}$
      c. $H \cdot C_1 = \{br\text{-}ing, br\text{-}ought, br\text{-}ought\}$
      d. $^*H \cdot D_1 = {}^*\{br\text{-}ing, br\text{-}inged, br\text{-}inged\}$

(17) a. $^*H \cdot A_2 = {}^*\{br\text{-}ink, br\text{-}ank, br\text{-}unk\}$
      b. $^*H \cdot B_2 = {}^*\{br\text{-}ink, br\text{-}unk, br\text{-}unk\}$
      c. $^*H \cdot C_2 = {}^*\{br\text{-}ink, br\text{-}ought, br\text{-}ought\}$
      d. $H \cdot D_2 = \{br\text{-}ink, br\text{-}inked, br\text{-}inked\}$

The word *brink* is a noun in the lexicon of standard English, and its inflection scheme is *H-D*. As the comparison with the pair of *ring* and *rink* in §3.2.4 makes clear, it is quite unpredictable that *bring* inflect as in *H-C₁*. The crucial point is, I think, the *learnability* of the scheme *H-C*.

Historical factors may have helped to the particularity of this pattern, but it is quite implausible to assume that the child has a certain potential to access to such factors. Keeping the pattern *H-C* being distinct from other patterns must cost a lot, since of all the verbs that end either *-ing* or *ink*, only *bring* and *think* (to be discussed §3.2.7) exhibit this pattern (and *seek* with some extension in pattern index). Given the pattern C is marked, I doubt that the degree of markedness can be stated in rules and representations theories.

3.2.7 Case of *thing~think* Pair: Disturbing Case II

When $H = th$-, then the following pattern of acceptabilities can be obtained:

(18) a. $^*H \cdot A_1 = {}^*\{th\text{-}ing, th\text{-}ang, th\text{-}ung\}$
      b. $^*H \cdot B_1 = {}^*\{th\text{-}ing, th\text{-}ung, th\text{-}ung\}$
      c. $^*H \cdot C_1 = {}^*\{th\text{-}ing, th\text{-}ought, th\text{-}ought\}$
      d. $H \cdot D_1 = \{th\text{-}ing, th\text{-}inged, th\text{-}inged\}$

(19) a. $^*H \cdot A_2 = {}^*\{th\text{-}ink, th\text{-}ank, th\text{-}unk\}$
      b. $^*H \cdot B_2 = {}^*\{th\text{-}ink, th\text{-}unk, th\text{-}unk\}$

  c. $H \cdot C_2$ = {*th-ink, th-ought, th-ought*}
  d. **H \cdot D_2* = *{*th-ink, th-inked, th-inked*}

As noted earlier, inflections in pattern C does not preserve lexical distinction between *-ing* and *-ink* in past and past participle forms. To put differently, the patter C is not suitable for the verbs that may have lexical distinction between *-g* and *-k.* Consider the paradigms of those verbs that have not such negative possibility such as *fight.* It inflect as {*fight, fought, fought*} without the possibility of losing its distinction from the word *fighd /fayd/, which is not a possible word in English. I suspect that this is the strongest reason that the pattern C is the least available in the class of inflectional paradigm that has been considered.

3.3 "Chaos" in the Mind: Summary of the Section

It seems quite claimable to me — though wanting for appropriate neuro-biological foundation — that such paradigms as {*bring, brang, brung*} are incorrect in standard English not because, as symbolists such as Chomsky and Halle (1967) postulate, of deterministic factors, but because, as cognitivists such as Bybee (1985, 1994), Lakoff (1982, 1987), and Langacker (1988) postulate, of experiential factors. The stability of the English lexicon is admittedly not so secure, and somehow retained mostly by chances, thereby exhibiting a "chaotic" behavior.

Note that, by the term "chaos," I do not mean that verb inflection constitutes just a disorder making no regularity, though various versions of such view have been frequently stated and quite popular in linguistics. I mean that the contrary should be true. As Bybee and Slobin (1982) demonstrated, there are *classes* of regularity, which in itself calls attention and demands explanation.

It is certain that the frequency in usage plays there a crucial role, as Bybee (1985) claims, but with its appropriate interpretation accompanied. Given that language is an "open system," and so is verb inflection. Frequency in usage thus can be interpreted as the "temperature" of the system, and, according to Prigogine and Stengers (1984), there emerges a chaos, or something that is difficult to predict the exact behavior of, in such an open system whose temperature is not too cold, nor too hot.

Such characterization of the issue is admittedly a sketchy metaphor, and it is by itself problematic. However, according to recent important findings in natural sciences (Gleick, 1987), chaotic phenomena are so common that non-chaotic phenomena are rare and even exceptional. If so, why does not the language have chaotic properties? We can nowadays expect such hardly predictable phenomena to fall within what Nicolis and Prigogine (1989) call "problems of complexity." Such line of exploration of linguistic phenomena will come with successful results in future, and, indeed, PDP modelings of linguistic phenomena have been already guided on such line. So, I think that some of symbolists' rejections to connectionist models rely on certain scientific ignorance.

4. Discussion: Reconciling the Debate in a Realistic Way

Advocates of symbol manipulation paradigm follow classical ontological argument. They ostensibly argue that there *must* be rules of language because most of linguistic phenomena can be described in terms of rule. Such argument can be easily invalidated, however: it is because it is not true that there *are* rules for all that can be described in terms of rule. Thus, the fundamental issue raised in the symbolist-connectionist debate can be simplified as this: Why is it *possible* to describe systematically most of linguistic phenomena such as past or past participle formations in terms of rule? This renewal of question, which I think serves to make clear the most crucial problem in the debate, inevitably forces us to seek for answers to those more fundamental problems of what is a description,

what is a reality, etc. Although it is doubtful there are any answers to the questions, I want to suggest in the following discussion a plausible direction that linguists should take, which seems to reconcile the debate in a realistic way.

4.1 Nature of Linguistic Description

To begin with, I want to make conceptually clear what linguists actually do when they think that they do linguistics is a *phenomenology of the language*, comparing it with what chemists do when they think that they do chemistry.

In the case of verb inflection, linguists can be said to do linguistics when they make generalizations from the description of a kind of natural phenomena occurring in the speaker's mind, which will have such a causal structure roughly schematized as the following series of two stages, (20a) and (20b).

(20) a. $X \xrightarrow{f_i \in F} f_i(X)/X = ...$

b. $f_i(X) \xrightarrow{g_i \in G} X_i'$

where (i) X stands for a given resource (e.g., the base form of a verb), (ii) $f_i \in F$ stands for a format of operation specified by the condition $.../X = ...$ among the available formats within the functional identity F (e.g., past tense formation), (iii) $g_i \in G$ is the mental association of the two mental entities, $f_i(X)$ and X_i'. Linguists can say that there is a rule if they can formulate a scheme like (20).

On larger scales, the scheme (20) and, therefore, the system of rules work fairly effectively. But, rules or schemes function only as heuristic devices, for their validity is essentially trivial, because they are powerful enough to describe anything that is describable.

Note that the validity of such heuristic devices tells nothing about the facts on smaller scales, i.e., architecture of the mind. On smaller scales, on the contrary, as Rumelhart and McClelland (1986) suggests, an appropriate model exposed to an appropriate data (i.e., consistent and sufficient amount of the pairs of V and V_i') manifests the property of *automatic abstraction*, and the availability of formats emerges automatically. Thus, the model suggests that human brain's learning potential dispenses the hypothesis of the innateness of linguistic knowledge. In short, what connectionists attack is the dogma of isomorphism according to which symbolists postulate that what is described on larger scales in terms of rules, schemes, or patterns is isomorphic to what actually occurs on smaller scales of the mind.

The following are my specific proposals to blur the symbolist-connectionist debate.

(21) a. The assumption of system of rules as instantiation of the innate knowledge of language is less explanatory than the assumption of human brain's innate ability to perform such automatic abstractions as $V \xrightarrow{f_i} f_i(V) \xrightarrow{g_i} V_i'$ in (20).

b. Given that (a) is correct, it will be simpler, and is probably better, to consider "rules of language," at least for the case of verb inflection, to be *phenomenological* entities.

By stating like (21a), I do not intend to claim that rules are not indispensable for linguistics. The contrary is true. Rules are indispensable to achieve certain explanations in linguistics out of descriptions. But I also bear in mind that what is an explanation in linguistics is not an explanation in ultimate

form, for explanations in linguistics are on phenomenological scales.

4.2   Linguistics as a Phenomenology of the Language

I guess that fanatic proponents of symbol manipulation paradigm may attack my statement that linguistics is a phenomenology of the language, but I believe that they attack it simply because they do not really understand what is meant by the term "phenomenology." Most natural sciences are phenomenologies: biology is a phenomenology of the nature in terms of "life," chemistry is a phenomenology of the nature in terms of "reactions," and even most part of physics is a phenomenology of the nature in term of "motion."

What strikes me the most in the generativist-connectionist debate is, as well as its ideological aspects, symbolists' over-defensive conception of what *is* and *should be* linguistics. It is quite certain, from various historical facts we have, that, in every field of science, there are possibilities of eliminative reduction. Here is an example, and we can learn a lesson from it.

Consider conceptual conflicts between chemistry and physics. Suppose that all elements (primitives in chemistry) are proved to be composed of lower-level entities (e.g., nucleons), and nucleons of further lower-level entities (e.g., quarks). Even if this happened, it would be very unlikely that a branch of science that we now call chemistry then disappear. Imagine a situation in which no one can talk about the nature in terms of chemical reactions, which is not only quite inconvenient but also absurd. This means that it is necessary to relax the notion of *reality* not to exclude the possibility of talking about elements as if they were real. I say so because I believe that the nature will, like a well that never goes dry, continue to nourish our scientific knowledge even on phenomenological scales. Recent discovery of "chaotic" aspects of the nature, which happened to fall within biology and chemistry, is a good example of this (Gleick, 1987). The discovery seems to confirm one ironical fact: the more familiar is a phenomenon, the worse understood it is likely to be. In any event, thanks to essential richness of the "superficial" facts, modern chemistry can be content with the status of a phenomenological science; and, unlike some parts of modern physics aiming to seek out ultimate entities of the nature, there is few motivations for chemists to investigate things deeper and deeper.

Replace *the nature* for chemists and physicians with *the language* for linguists and connectionists, and think again the debate. Then consider again the claim (1) by Rumelhart and McClelland. It does not really matter, I believe, whether to talk about the language in terms of rule (higher-level entity), or in terms of association (lower-level entity) if we have acknowledged that linguistics is a phenomenology of the language. It is quite easy to run into one of the extreme positions of radical symbolist and connectionist, but it would not be very wise. Linguistics does not need to be so strict a science of the language as brain science does, like chemistry does not need to be so strict a science of the nature as quantum mechanics does. I believe that it will be the most reasonable way to give a reply of *why not* to Rumelhart and McClelland's challenge, and continue our *looser* talks about the language in terms of rule, scheme, pattern, class, and the like, but without any ontological commitment to them.

References

Boden, Margaret A., *ed.* (1990). *The Philosophy of Artificial Intelligence.* Oxford: Oxford University Press (Oxford Readings in Philosophy).

Bybee, Joan L. (1985). *Morphology: A Study of the Relation between Meaning and Form.* (Typological Studies in Language 9) John Benjamins.

_____ (1994). "A view of phonology from a cognitive and functional perspective." *Cognitive*

Linguistics, 5 (4), pp. 285-305.

Bybee, Joan L., and Dan I. Slobin (1982). "Rules and schemes in the development and use of English past tense." *Language*, 58, pp. 265-289.

Chomsky, Noam A. (1957). *Syntactic Structures*. The Hague: Mouton and Co.

———— (1965). *Aspects of the Theory of Syntax*. Cambridge, MA: M.I.T. Press.

———— (1975). *The Logical Structure of Linguistic Theory*. Chicago: The University of Chicago Press. [This is the slightly revised version of Chomsky (1955). *Logical Structure of Linguistic Theory*. Mimeographed, M.I.T.]

Chomsky, Noam A., and Morris Halle (1968, 1991). *The Sound Pattern of English*. (First M.I.T. Paperback edition 1991). Cambridge, MA: M.I.T. Press. [First edition was published in 1968 from New York: Harper and Row]

Clark, Andy (1990a). *Microcognition: Philosophy, Cognitive Science, and Parallel Distributed Processing*. Cambridge, MA: M.I.T. Press/Bradford Books.

———— (1990b). "Connectionism, competence, and explanation," in Margaret A. Boden, *ed.* (1990). *The Philosophy of Artificial Intelligence* (pp. 281-308). Oxford: Oxford University Press.

Davis, Steven, *ed.* (1992). *Connectionism: Theory and Practice*. New York/Oxford: Oxford University Press.

Fodor, Jerry A. (1983). *The Modularity of Mind: An Essay on Faculty Psychology*. Cambridge, MA: M.I.T. Press/Bradford Books.

Fodor, Jerry A., and Zenon W. Pylyshyn (1988). "Connectionism and cognitive architecture: A critical analysis," in Goldman, *ed.* (1988, pp. 801-818). [Originally appeared in *Cognition*, 28 (1, 2), pp. 3-71 (1988). Also reprinted in Pinker and Mehler, *eds.* (1988, pp. 3-71)]

Gleick, James (1987). *Chaos: Making a New Science*. New York: Viking.

Goldman, Alvin I., *ed.* (1988). *Readings in Philosophy and Cognitive Science*. Cambridge, MA: M.I.T. Press.

Kasher, Asa, *ed.* (1991). *The Chomskyan Turn*. Oxford, UK: Basil Blackwell.

Kirsh, David (1992). "PDP learnability and innate knowledge of language," in Stephen Davis, *ed.* (1992, pp. 297-322).

Kohonen, Teuvo (1977). *Associative Memory: A System-Theoretical Approach*. Berlin: Springer-Verlag.

Kuhn, Thomas S. (1970). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press. [First edition was published in 1962]

Kugler, P., M. Turvey, and R. Shaw (1982). "Is the cognitive penetrability criterion invalidated by contemporary physics?" *Behavioral Brain Science*, 5, pp. 303-306.

Lakoff, George (1982). "Experiential factors in linguistics," in Thomas W. Simon and Robert J. Scholes, *eds.*, (1982, pp. 145-156). *Language, Mind, and Brain*. Hillsdale, NJ: Lawrence Erlbaum Associates.

———— (1987). *Women, Fire, and Dangerous Things: What Categories Reveal about the Mind*. Chicago: University of Chicago Press.

Langacker, Ronald W. (1988). "A usage-based model," in Brygida Rudzka-Ostyn, *ed.* (1988, pp. 127-161). *Topics in Cognitive Linguistics*. Amsterdam/Philadelphia: John Benjamins.

McClelland, James L., David E. Rumelhart, and The PDP Research Group (1986). *Parallel Distributed Processing: Explorations in the Microstructure of Cognition; Vol. 2: Psychological and Biological Models*. Cambridge, MA: M.I.T. Press.

McCulloch, Warren S., and Walter H. Pitts (1943). "A logical calculus of the ideas immanent in nervous activity." *Bulletin of Mathematical Biophysics*, 5, pp. 115-133. Reprinted in Boden, *ed.* (1990, pp. 22-39).

Newmeyer, Frederick J. (1986). *Linguistic Theory in America: The First Quarter-Century of Transformational Grammar.* (2nd ed.). New York: Academic Press. [First edition was published in 1980.]

_____ (1991). "Rules and principles in the historical development of generative syntax." In Asa Kasher, *ed.*, (1991, pp. 200-230).

Newell, Allen and Herbert Alexander Simon (1972). *Human Problem Solving.* Englewood Cliffs, NJ: Prentice-Hall.

Nicolis, Grégoire, and Ilya Prigogine (1989). *Exploring Complexity: An Introduction.* München: R. Piper GmbH and Co. KG Verlag.

Pinker, Steven (1991). "Rules of language," in Paul Bloom, *ed.* (1991). *Language Acquisition: Core Readings* (pp. 472-484). New York: Harvester Wheatsheaf. [Originally published in *Science*, 253, pp. 530-535, 1991. Also Reprinted in Geirsson and Losonsky, *eds.* (1996, pp. 558-569). *Readings in Language and Mind*]

_____ (1995). "Why the child holded the baby rabbits: A case study in language acquisition," in Lila R. Gleitman and Mark Lieberman, *eds.*, (1995, pp. 107-133). *An Invitation to Cognitive Science, Vol. 1: Language.* Cambridge, MA: Bradford Books/M.I.T. Press.

Pinker, Steven and Alan Prince (1988). "On language and connectionism: Analysis of a parallel distributed processing model of language acquisition," in Pinker and Mehler, *eds.* (1988, pp. 73-193).

Pinker, Steven and Jacques Mehler, *eds.* (1988). *Connections and Symbols.* Cambridge, MA: M.I.T. Press/Elsevier. [Reprinted from *Cognition: International Journal of Cognitive Science* 28 (1, 2)]

Prigogine, Ilya, and Isabelle Stengers (1984). *Order out of Chaos: Man's New Dialogue with Nature.* New York: Bantam Books. [Forward "Science and change" by Alvin Toffler.]

Pylyshyn, Zenon W. (1984). *Computation and Cognition: Toward a Foundation of Cognitive Science.* Cambridge, MA: M.I.T. Press.

Quillian, M. Ross (1967). "Word concepts: A theory and simulation of some basic semantic capabilities." In Ronald J. Brachman, and Hector J. Levesque, *eds.* (1985, pp. 98-118). *Readings in Knowledge Representation..* San Mateo, CA: Morgan Kaufmann Publishers, Inc. [Originally appeared in *Behavioral Science*, 12, 1967, pp. 410-430]

Rumelhart, David E., and James L. McClelland (1986). "On learning the past tenses of English verbs," in J. L. McClelland, D. E. Rumelhart, and the PDP Research Group (1986, pp. 507-526). Repinted in Goldman, *ed.* (1993, pp 535-551).

_____ (1987). "Learning the past tenses of English verbs: Implicit rules or parallel distributed processing?" in Brian J. MacWhinney, *ed.* (1987, pp. 195-248). *Mechanisms of Language Acquisition.* Hillsdale, NJ: Lawrence Erlbaum Associates. [This is the slight modification of Rumelhart and McClelland (1986)]

Rumelhart, David E., James L. McClelland, and The PDP Research Group (1986). *Parallel Distributed Processing: Explorations in the Microstructure of Cognition, Vol. 1: Foundations.* Cambridge, MA: M.I.T. Press.

Seidenberg, Mark S. (1992). "Connectionism without tears," in Stephen Davis, *ed.* (1992: 84-122).