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BISAZZA, JOHN ANDREW

THE PROCESSING COMPLEXITY OF NOUNS AND VERBS:
PSYCHOLINGUISTIC AND NEUROLINGUISTIC ISSUES

University of Hawaii

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THE PROCESSING COMPLEXITY OF NOUNS AND VERBS:
PSYCHOLINGUISTIC AND NEUROLINGUISTIC ISSUES

A DISSERTATION SUBMITTED TO THE GRADUATE DIVISION OF THE UNIVERSITY OF HAWAII IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY
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DECEMBER 1980

By
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This dissertation attempts to provide an explanation for the psychological processing ease of nouns relative to verbs seen in various performance contexts.

Chapter 1 first describes noun facilitation phenomena in experimental studies with adults, language acquisition and the breakdown of linguistic functions due to brain damage. The second part of Chapter 1 discusses attempts to explain noun facilitation phenomena and inferences made from these phenomena to other research issues. It is argued that, among existing theories, that of Marshall, Newcombe and Holmes (1975) has the greatest potential for eventually explaining all noun facilitation phenomena—despite several inadequacies.

Marshall et al. claim that the basis of noun facilitation is not syntactic class per se. They claim that the number of arguments lexical items can enter into syntactic constructions with ("give" has three: "X gives Y to Z"; "man (noun)" has one: "X is a man") determines processing complexity and susceptibility to impairment. According to Marshall et al., most nouns just so happen to take fewer arguments than most verbs. They claim that a noun like "gift" (three arguments: "X is a gift from Y to Z") should be no easier to process than a verb like "give."

Chapter 2 proposes an alternative to Marshall et al.'s theory ("the full recall hypothesis") which also removes the objections to their theory noted in Chapter 1. This alternative ("the minimum recall hypothesis") claims that a lexical item will be recalled from
long term memory—in tasks involving little or no syntactic computation—with the minimum number of arguments required to use that item in a grammatical sentence. In English and Japanese (the languages discussed in this dissertation) multiple argument (MA) nouns can often be used in single argument (SA) constructions (e.g., "Books make nice gifts"). This is not the case with many verbs (e.g., "put"). Therefore, the minimum recall hypothesis predicts no difference among SA nouns and verbs and MA nouns, which are all predicted to be equally easier to process than MA verbs (provided the latter are like "put" in having obligatory arguments).

Chapter 2 presents the results of tests in Japanese of the full recall hypothesis and the minimum recall hypothesis—as well as of other theories of noun facilitation—using normal adults (tachistoscopic experiment) and a brain-damaged person (reading aloud test). The results support the minimum recall hypothesis.

Chapter 3 presents the results of a similar tachistoscopic experiment in English using normal subjects. In addition, "SA/MA" verbs—which can function as either SA or MA items (e.g., "grow")—were included in the experiment. The minimum recall hypothesis predicts no difference among the thresholds of SA and MA nouns and verbs like "grow." Again the results mostly support the minimum recall hypothesis, although the results for the SA/MA verbs are ambiguous.

Chapter 4 concludes the dissertation with a summary of the arguments which support the minimum recall hypothesis over the full recall hypothesis—as well as over other hypotheses—as a theory of noun facilitation in general. The ambiguous results for the SA/MA verbs are
also discussed. The relevance of the issues discussed throughout the dissertation for psycholinguistic research on parsing strategies, formal syntax and neuropsychology is taken up. Chapter 4 concludes with an outline of goals for future research on the causal basis of noun facilitation phenomena.
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"But at times the spring-flood of memory sets with force up the dark River of the Nine Bends."
1.0. Introduction

In experimental tasks using visual recognition as a response measure, nouns have been shown to be easier to recognize than verbs. The tachistoscopic design reported in Holmes, Marshall and Newcombe (1971) is a case in point. This difference between nouns and verbs has its analog in the areas of child language and linguistic breakdown due to brain damage. Nouns predominate in the speech of very young children. Some brain-damaged persons retain a greater facility for nouns than for verbs. In this study, I will use the expression "noun facilitation" to refer to behavioral phenomena involving a greater processing ease (in any sensory modality and varying task situations) for nouns versus other syntactic categories in any one or all of these three areas.

The greater facility for the visual recognition of nouns—together with its analog in language acquisition and brain damage—is an exciting finding for psycholinguists since it implicates a purely linguistic variable (i.e., syntactic class) in differential psychological processing. Furthermore, similar phenomena involving nouns have been reported for a variety of languages, including some non-Indo-European ones, and thus suggest a universal of language structure and use. This makes noun facilitation a promising source of information against which to check theories of linguistic competence and performance.
The present study will be concerned with an explanatory theory of noun facilitation and with other linguistic issues related to this phenomenon. In this chapter, I will present first an outline of noun facilitation phenomena in experimental studies, language acquisition and linguistic breakdown due to brain damage. In the second half of this chapter I will discuss some attempted explanations of noun facilitation in the literature and some of the inferences regarding linguistic theory made from this phenomenon.

1.1. Manifestations of Noun Facilitation

1.1.1. Noun Facilitation in Experimental Studies

(All of the studies to be discussed throughout this dissertation deal with English unless otherwise mentioned.)

Riegel and Riegel (1961) used adolescent German subjects to determine the effect of 40 stimulus parameters on the visual recognition thresholds of German words. Their study used a tachistoscope and an ascending method of limits design. A tachistoscope is an instrument for presenting visual stimuli at extremely rapid exposure durations; the kinds of tachistoscopes used in psychological experimentation are more elaborate versions of the machines sometimes used to teach speed reading, usually having more accurate exposure duration controls, chin and forehead rests for the subject, etc. In the ascending method of limits design subjects are presented a printed word starting at a baseline exposure duration measured in milliseconds. The subjects are required to verbally report the word they see—if any—to the experimenter. In Riegel and Riegel's study, the criterion for a correct
performance for each stimulus word was three correct verbal reports in a row on successive exposures. Each time a given stimulus word was presented after the first exposure, the exposure duration was slightly increased. In a series of three correct recognitions at increasing durations of presentation, Riegel and Riegel took the first of the three durations as the word's threshold—that is, as a measure of its difficulty for the subject. This procedure was, of course, repeated for all of the stimulus words. (The ascending method of limits design in tachistoscopic recognition tasks will be discussed in more detail in the "Method" section of Chapter 2.)

Riegel and Riegel found effects for a variety of factors, such as number of letters, whether a word had a prefix or not, frequency, etc. However, their highest correlation with the recognition thresholds was obtained from a classification of their stimulus words into concrete nouns versus all other abstract nouns, adjectives and verbs (ibid.:163). That is, concrete nouns were recognized at significantly lower (faster) thresholds than all other stimulus words. (Riegel and Riegel did not report on the results of abstract nouns versus other stimulus words in their experiment.)

Holmes et al.'s (1971) study already noted above was conducted in English with adult subjects as a check on Riegel and Riegel's study since the latter's "results may have been biased by a difference in the relative usage of concrete nouns by adolescents and adults" (ibid.:418). However, using a basically similar design, Holmes et al. still found the facilitation effect for concrete nouns (no abstract nouns were tested), as well as facilitating effects for word frequency
and increasing word length (between four and seven letters). Of particular interest in Holmes et al.'s study was the finding that concrete nouns were significantly easier to recognize than either adjectives or verbs, and that adjectives were midway in recognition difficulty between concrete nouns and verbs.

Marshall and Holmes (1974) reconfirmed the facilitation effect noted above for concrete nouns versus verbs (as well as frequent versus infrequent words) in a tachistoscopic study using a large number of normal subjects. In addition, in this study they used a hemifield design in which stimuli were selectively presented once each in both the right and left visual fields. Marshall and Holmes were thus able to report on a number of interesting interactions. These included the finding that the facilitation for nouns was predominantly associated with low frequency words, as well as being predominantly associated with the right visual field (left hemisphere). This is what we would expect from the normal lateralization of language to the left hemisphere. (The frequency effect, on the other hand, was found to be primarily associated with the left visual field (right hemisphere).)

I will return to some of the interesting aspects of the above studies when I discuss attempted explanations of, and inferences from, noun facilitation.

1.1.2. The Primacy of Nouns in Language Development

Many authors have remarked that many of the first words a child learns tend to be nouns and that nouns predominate in the speech of young children.
In the pregrammar stage, before a child uses notions of construction, his lexicon usually contains a high proportion of words that can be classed as nouns in the adult model (Bloom 1970:29).

Goodglass (1973:213) writes regarding agrammatic symptoms due to brain damage that

There appears to be a parallel between the nominalizing tendency of the agrammatic and the fact that nouns appear first in the speech of the young child. As in the young child's vocabulary, nouns are disproportionately high in number in comparison to verbs in the speech of our Broca's aphasics.

Furthermore, many of the child's first attempts at syntactic construction consist entirely of nouns, such as "Mommy pigtail" (Bloom 1970:54).

Bloom (ibid.:29) also notes that

Further, the new or different forms--adjectives, verbs, prepositions, and other nouns--that [Kathryn, Eric and Gia] added to their lexicons, were just those forms that could be selected to combine with the noun forms already acquired.

The above statements have been challenged from a viewpoint of Whorfian linguistic relativity. For example, in a "play" in which he tries to characterize the Navaho world view in relation to the Navaho language, Sinclair (1954:31) has a Navaho character say

Our Navaho outlook is focused on actions and events, our language on verbs. The first words our children learn may well be those expressive of actions. (Emphasis added.)

NAVAHO MOTHER


Sinclair does not state--or have his characters say--on what grounds he makes the above predictions. However, this hypothesis should be fairly easy to confirm or disconfirm by simple observation, although I am not aware of any such study.
A hypothesis that might be put forth regarding the question raised above with respect to Navaho is that at the very beginning of language acquisition any child must first acquire the labelling function of language, which in turn presupposes at least a minimum of perceptual constancy. Continuing under this view, it could be suggested that the child will first perceive with consistency and learn to label those items in his/her world which are most unchanging, and that such labels can hardly avoid being concrete nouns. (In this connection, Stemmer (1973:41) claims that learning labels through paired association—one of the child's first learning strategies—is not as easy for verbs as for nouns.) Such speculations may ultimately turn out to have some validity, but some qualifications seem immediately necessary. For one, some actions and states (e.g., diaper wetness) could have perceptual constancy for the child. Second, some kinds of change in perceived stimuli—e.g., movement—could function to attract the child's attention and thus increase the likelihood of the stimuli's being labelled by the child. Several researchers claim that the child's earliest words are often those that refer to moving objects (see Bloom and Lahey 1978:75 and the references cited there). Of course, here we need to distinguish among the different kinds of changes that perceived stimuli can undergo. Movement, for example, might not affect the internal structure of an object as perceived by the child in the same way as a change in color, say, might do so.

There are various weaker alternatives to the above hypothesis regarding Navaho which could be imagined. For example, one could claim that once the labelling function is acquired for nouns and at least
some verbs, Navaho children make greater use of verbs than, say, English-speaking children, as a strategy for constructing complex utterances. This kind of claim leads to more specifically linguistic questions regarding the posited primacy of nouns in children's speech. Are there, for example, purely syntactic reasons why young children should have a greater facility with nouns beyond the initial labelling problems discussed above (such as those suggested in Bloom 1970:29-30 noted on p. 5 above—see also pp. 59-60 below in this connection), and to what extent are these factors universal? For the moment, this appears to be an open question, about which I will make some suggestions later in this study.

So far, I have not attempted to provide a definition of the parts of speech—noun, verb, adjective, etc.—I have been discussing. Clearly, however, such a step is crucial to the kinds of questions raised with respect to Navaho above. In the two languages to which I will devote most of my discussion in this study—Japanese and English—such classifications are widely accepted. However, the question of a definition of parts of speech can be turned around as a hypothesis about specific languages or even the universal characteristics of languages, as for example in Chomsky (1965:28ff.). From such a starting point, one would postulate a set of semantic and syntactic criteria which hypothetically could be used to separate out lexical categories in a language or languages. The search for such criteria and the investigation of differential psychological performance according to the parts of speech thereby delimited can thus be seen as aspects of the same general problem, to which I will also return later
There are other studies which touch on the role of nouns in children's speech and which necessitate some qualification to what has been said so far in this section.

In a study of the vocabulary of 18 children at the 50-word stage (roughly 20 months), Nelson (1973:18) found that 66% of the children's vocabularies were made up of general and specific adult nominals. In contrast, "action words" made up only 13%. Various kinds of "modifiers" made up 9%. Other words (which included some adult verbs) made up 12%. Furthermore, she notes (ibid.:19) that not even all of the "action words" in her corpus are verbs in adult language (e.g., "up"). She also observes that

Action words are not as numerous as might be expected according to a theory of the salience of action in the life of the young child, but neither are they so absent that they can be considered a "secondary acquisition" (loc. cit.).

With regards to this last point, Nelson found that the children she studied seemed to fit into two general types in terms of their language acquisition strategies: "referential" types, who had a high proportion (i.e., 50+%) of adult nouns (including pronouns) among their first 50 words; and "expressive" (or "social interaction") types who had a lower proportion (i.e., < 50%) of adult nouns among their first 50 words. However, the children who fell into Nelson's "expressive" category still had fewer verbs (according to the adult lexicon) than nouns among their first 50 words (ibid.:124-7). In any case, recall that Nelson's basis for the "referential"-"expressive" split is the percentage of nouns and pronouns versus all other words, including many
adult non-verbs. The figure of 66% nominals in the vocabulary of 18 children quoted above includes ten of Nelson's "referential" types and eight "expressive" types.

Note that children at the age of 20 months--like those discussed in Nelson (1973)--usually do not combine two or more words into single utterances. Thus, terms such as "modifier," "preposition," etc. must be taken with a grain of salt when applied to children's vocabulary at this age. Such terms are used to classify children's vocabulary partly based on adult usage and partly on the investigator's personal judgment about the context in which the vocabulary is used by the child. Thus, the possibility arises that the child's words labelled "modifiers," for example, by Nelson actually serve a simple labelling function.

Menn (1978:42ff.) discusses a child (Jacob) whose first ten words consisted largely of "action words" (in contrast to Nelson's (ibid.:20) findings for the ten-word stage) and whose first 50 words consisted of roughly 50% "action words." However, like Nelson, Menn counts as "action words" adult prepositions and other adult non-verbs on the basis of the context in which the child uses such words and their apparent referents. Thus, listed as "action words" at the 50-word stage for Jacob are "there," "here," "Jacob," an onomatopoeic word and several prepositions (ibid.:45). At the 50-word stage Jacob's vocabulary was still 50% general adult nominals (0% specific nominals) (loc. cit.).

What can be made of the two works cited above with regards to the claims put forth at the beginning of this section? They seem to
indicate some individual variation in terms of whether nouns do or do not predominate in the speech of young children.

First of all, there seem to be no reported cases of adult verbs predominating in the speech of very young children in the same way that adult nouns predominate in the speech of (at least) a large number of children. Menn's Jacob offers no counterevidence to this claim since his "action words" at the ten-word stage include almost no adult verbs, and since at the 50-word stage adult nominals clearly predominate in his speech. Menn's Jacob does, however, appear to be counterevidence to the hypothesis suggested above (p. 6) regarding the labelling function in language acquisition.

More significantly, we should ask why even "expressive" children should use adult non-verbs (e.g., adult adverbs, prepositions and even nouns) to such a large extent when they feel the need to communicate actions, relations, etc. There clearly seems to be something inherently difficult about verbs (and not about nouns) for even "expressive" children. Note that in the case of Jacob and Nelson's "expressive" children the vocabulary items listed as nominals are--for the most part--words that would be considered nouns in the adult lexicon.

Still, one cannot ignore the individual variation revealed in Nelson (1973) and Menn (1978) with respect to noun facilitation in children. I feel that this variation points up the existence of an interaction of effective factors in language acquisition which may converge differently in different children to produce the variation in children's language revealed by these studies. Facilitation for nouns and difficulty for verbs, however, still seem to be a key factor. These
different factors become especially apparent when children start to combine two or more words per utterance, as the following works show.

Yamada (1980) argues that children start to construct phrases incorporating verbs as a result of communicative pressures. Significantly, she notes (ibid.:42) that the Japanese child (Karen) she studied more frequently constructed multiple-word utterances from nouns alone, rather than from nouns plus verbs prior to the age of two years. At about the age of two years, Karen switched to more multiple-word utterances containing verbs. Interestingly, Yamada (ibid.:29-30) also notes that the mother's utterances to Karen ended in verbs 73% of the time—Japanese being an SOV language. This was the case both before and after Karen's shift to phrases incorporating verbs.

In a similar vein, Horgan (1980) discusses Nelson's dichotomy in terms of noun "lovers" and noun "leavers." Noun "lovers" in Horgan's terminology correspond to Nelson's "referential" types, and noun "leavers" correspond to Nelson's "expressives." Horgan argues that "leavers" are children who concentrate their attention on the syntactic "gestalt" of sentences (cf. Peters, 1977), while "lovers" concentrate on the semantic elaboration of individual words, tending to produce complex noun phrases. Horgan suggests that this difference in emphasis between "lovers" and "leavers" may even be reflected in adults, an adult "lover" being a speaker who uses more and longer noun phrases per utterance than adult "leavers." Horgan also notes that much research is needed to determine if and at what ages the different emphases of "lovers" and "leavers" might converge, with "lovers" becoming more interested in predicates and "leavers" becoming more interested in the
semantic elaboration of nouns.

Clearly, then, there are more factors at work in language acquisition than only noun facilitation. A great deal more research is needed on the whole question of nouns in different stages of language acquisition and different types of language learners. Such studies will be complicated by consideration of the socio-economic factors which Nelson and Horgan discuss in relation to different language learning strategies, the nature of the input language to the child, etc. However, the noun facilitation revealed in experimental studies with adults (as well as in cases of brain damage, to be discussed below) encourages the search for noun facilitation phenomena among all types of language learners. Certainly, the study of noun facilitation in adults (including brain-damaged adults) and children can complement each other. For example, I hope that the studies with adults to be discussed in this dissertation will contribute to our idea of what to look for in work on noun facilitation in children. On the other hand, Nelson's and Horgan's studies, for example, have already suggested things to look for in adult noun facilitation. In particular, do both "lovers" and "leavers" grow up to be adults who would show a noun facilitation effect in tachistoscopic experiments such as those discussed in section 1.1.1. above? We will have to wait for the children studied by Nelson, Menn, Horgan and others to grow up!

1.1.3. Noun Facilitation Effects in the Breakdown of Linguistic Functions Due to Brain Damage

As indicated by the quote from Goodglass above (p. 5), some types of brain damage result in a set of symptoms which have as their common
element a relatively spared facility for performance involving nouns as opposed to verbs, adjectives, etc., although the overall linguistic ability may be depressed. These symptoms beg for theoretical interpretation, especially since many of them have a wide cross-linguistic reliability. (Most of the data to be discussed in this section originate from speakers of Indo-European languages, but see Sasanuma (in press) for data on noun facilitation phenomena among brain-damaged Japanese, as well as section 1.2.2.3. below and following chapters of this study.) However, what might at first glance seem a straightforward tendency becomes a looming complexity upon closer examination—giving rise to complex and varied theoretical interpretations. Such complexity arises when one attempts to specify exactly what performance tasks give rise to noun facilitation and in what sensory modalities, what linguistic units are involved and in what way, and what types of brain damage result in what symptoms.

The findings I will outline below suggest a bias in the non-damaged individual towards nominal quality which surfaces in brain damage symptoms in various ways due to the nature of the attendant damage. In addition, brain damage may have the effect of highlighting or masking (perhaps by highlighting other normal performance biases) aspects of the appearance of this bias.

Production tasks of various kinds are much more discussed in the language pathology literature in connection with noun facilitation than comprehension tasks. The basic type of observation regarding production involves the occurrence of nouns in the spontaneous speech of brain-damaged patients. Such observations go beyond a statement
about the high number of nouns in the speech of some patients, as in
the quote from Goodglass above (p. 5). For example, the speech of
the agrammatic patient (with damage to the anterior speech area) often
consists of isolated nouns strung together with wide pausal separa-
tions ("telegraphic speech"), and is thus notably lacking in grammatical
function, or relational, words (i.e., articles, prepositions,
conjunctions and other words whose "grammatical functions [are] more
obvious than their semantic content" (Brown and Bellugi 1970:83)) and
inflectional endings, as well as being impoverished with respect to
adjectives, adverbs and verbs. Goodglass (1973:185) provides a good
example of such speech:

Yes . . . ah . . . Monday . . . ah . . . Dad and Peter
Hogan, and Dad . . . ah . . . Hospital . . . and ah . . .
Wednesday . . . Wednesday, nine o'clock and ah Thursday
. . . ten o'clock ah doctors . . . two . . . two . . . an
doctors and . . . ah . . . teeth . . . yah. And a
doctor . . . an girl . . . and gums, and I. [Upon
questioning about his former employment in a paper mill]
Lower Falls . . . Maine . . . Paper. Four hundred tons a
day! And ah . . . sulphur machines, and ah . . . wood
. . . Two weeks and eight hours. Eight hours . . . no!
Twelve hours, fifteen hours . . . workin . . . workin
. . . workin! Yes, and ah . . . sulphur. Sulphur and
. . . Ah wood. Ah . . . handlin! And ah sick, four years
ago.

In the above passage, the only functional words used are "and" and "a,"
and most of the adjectives used are numerals which can also be nouns.
There are only two "verbs" used, and these in their "-ing" form.

Goodglass (1962) compares the preference for the "-ing" form of
the verb among English-speaking agrammatics to the overuse of the
infinitive among French and German agrammatics. In the English case,
one could wonder whether such patients merely omit the auxiliary of the
present progressive form. However,

Since the German infinitive, unlike the English, has an inflectional ending, this evidence supports the view that the agrammatic indeed is not merely dropping the person and tense markers in English, but rather shifting to a nominalized use of the verb (Goodglass and Geschwind 1976:409).

In general, when verbs are used by patients who show a noun facilitation effect they are often used incorrectly—e.g., in inappropriate positions with respect to other lexical items produced at a given time (Whitaker 1972:69—data obtained in a writing sample); without a correct inflection (Goodglass 1973:186); or without obligatory argument NP's (as in the two verb forms in the passage from Goodglass on p.14 above). All of this suggests that a significant syntactic impairment lies at the base of the noun facilitation effect in patients showing these symptoms. Thus, Jakobson (1973:35) writes that the apparent inability to "propositionize" in agrammatism is only the focal expression of a general tendency to abolish any syntactic rank... Ultimately, speech is reduced to primaries, independent words—nouns and nominal forms of verbs—in holophrastic usage.

In an effort to quantify data on noun facilitation and ultimately control possible influencing variables, specific tasks have been tried with the patients who show this effect. The results largely confirm what we know from an examination of such patients' spontaneous speech, but at the cost of some complexity.

As a step towards quantification, Luria and Tsvetkova (1968) discuss a simple task which they used as a test for what they call "dynamic aphasia." This term covers a complex of symptoms resulting from damage to the anterior speech area of the brain and has as one of its
characteristics a distinct facilitation for nouns as described above. In their task, Luria and Tsvetkova asked Russian patients to name as many objects as they could in a minute with their eyes closed, and then to name as many actions as possible in the same length of time. They found (ibid.:298) that their patients with dynamic aphasia could produce on the average about four times as many nouns as verbs. Patients with a medium degree of impairment could produce 9-10 names of objects in one minute, but hardly any verbs. Patients with good recovery from dynamic aphasia "had no disturbance in naming objects, but marked difficulties in finding names of actions" (figures not reported) (loc. cit.). Of interest is the fact that this task failed to bring out any difference between nouns and verbs in Luria and Tsvetkova's normal control subjects.

Whitaker (1979 LSA summer course on aphasia) has suggested that one of the most useful tests of linguistic ability in brain-damaged patients is that of simple repetition. In connection with noun facilitation, the repetition of auditorily presented stimuli has the advantage of letting the investigator know and control the subject's target words. Goldstein (1948:238) reports a patient whose repetition of auditorily presented nouns was better than that for adjectives or verbs. Halpern (1965) also reports a similar finding with repetition tasks, as well as reading aloud tasks (which also have the advantage of controlling the subject's target words).

Marshall and Newcombe (1966) also report the results of a reading aloud task with a patient who showed a noun facilitation effect, among other symptoms. This patient's spontaneous speech had some loss of
function words, although his speech was coherent, and he also had some comprehension difficulty. These symptoms are consistent with the location of his brain damage which involved the left temporal and parietal lobes, as well as the area of the Sylvian fissure. Upon auditory presentation of stimulus words, the patient could repeat without error, and his speech showed no jargon. His primary difficulty involved reading aloud written words. Here, he made many paralexic errors. In the reading aloud task designed by Marshall and Newcombe, he correctly read 45% of the presented nouns and 16% and 6% of the presented adjectives and verbs, respectively—thus showing a clear facilitation effect for nouns. The patient could also read no prepositions (n = 20), adverbs (n = 20), determiners (n = 18) or WH words (n = 8), and only "I" out of the seven nominative case pronouns in English (ibid.:172). In addition, there was a strong tendency for response errors to be nouns when the stimulus word was a noun or verb (90% of the errors for each) (ibid.:173). For adjectives, 72% of the response errors were adjectives and 24% nouns. There were very few adjective response errors for nouns or verbs, and even fewer verb response errors for nouns, adjectives or verbs. Interestingly, there was not a single verb response error to the verb stimuli. Finally, in Marshall and Newcombe's patient the noun facilitation effect could even override a strong familiarity value, as can be seen in the patient's response "Name . . . in France . . . South of France" for the category ambiguous stimulus "NICE": as Marshall and Newcombe (ibid.:176) point out, "surely the most frequent adjective in the English language."
Marshall and his colleagues have continued working along similar lines since the paper discussed above.

For example, Holmes et al. (1971) (also discussed above) conducted their tachistoscopic study as part of a search for parallels in normals for the noun facilitation effect observed in brain-damaged people. In that study, they also administered their tachistoscopic stimuli to five brain-damaged dyslexics as a reading-aloud task with no time pressure. As in the tachistoscopic results, these subjects had significantly more correct answers with the noun stimuli than with either the adjectives or verbs, which were nearly equal in number of correct readings. However, this last finding is in contrast with their tachistoscopic result of a difficulty for adjectives midway between nouns and verbs. Higher frequency also facilitated performance for these dyslexic subjects, as in the tachistoscopic results, but—again in contrast with the normal subjects—increasing length hindered the performance of these dyslexics.

The problem with respect to adjectives is heightened in Marshall, Newcombe and Marshall (1970). With the same subject discussed in Marshall and Newcombe (1966), Marshall et al. this time found a clear difference between adjectives and verbs, with the former easier to read aloud. In two testing sessions separated by three years, the subject read correctly eight at the first session and 11 at the second session out of the same 20 adjectives, and one at the first session and three at the second session out of the same 28 verbs. However, based on some of the types of error responses reported in their 1966 study (such as "beg" read as "beggar," "entertain" as "entertainment," etc.)
Marshall et al. decided to include a list of derived or otherwise related nominals for both the adjective and verb lists (e.g., "high": "height"; "arrive": "arrival"). The upshot was that the subject read only one at the first session and two at the second session of the "adjective nominals," but seven of the "verb nominals" at both sessions. Therefore, adjective nominals were more difficult than either adjectives or verb nominals and were of approximately equal difficulty with verbs. Verb nominals, on the other hand, were only slightly more difficult than pure adjectives. In addition, Marshall et al. (1970:420-1) report that there was a pronounced tendency to misread an adjective nominal as its corresponding root adjective, and a root verb as a derived, or otherwise related nominal! As Whitaker (1972:66) has written, "this is clearly a puzzling area of aphasic behavior which needs a great deal of study." Importantly, Marshall et al. (ibid.:421) point out that the nominals in their task were in general less frequent and longer in orthographic length than their corresponding root form (cf. the findings regarding length reported by Holmes et al. (1971) and discussed above (p. 18)).

Whitaker (1972) also contains some interesting data on the substitution of derived (or otherwise related) nominals for verbs which appears to be a subcomponent of noun facilitation in some brain-damaged persons. In this paper, Whitaker discusses three patients with differing causes and sites of brain damage.

W. L. suffered a skull fracture and hematoma from a blow to the fronto-temporal region of the head. K. T. suffered a traumatic intercerebral hematoma in the parietal region from a bullet wound. F. W. suffered a generalized CVA from occlusion of the middle cerebral artery (ibid.:66).
K. T. and W. L. showed a noun facilitation effect in Luria and Tsvetkova's test for dynamic aphasia. F. W.'s spontaneous speech was severely impaired, and he could therefore not be given this test; however, Whitaker (ibid.:69) concluded from a writing sample of F. W.'s that he showed a distinct noun facilitation effect. F. W. was given a reading aloud test similar to that described above in connection with Marshall and Newcombe (1966) using noun and verb stimuli. Because of the milder nature of their impairment, K. T. and W. L. were presented the stimuli both orally and visually and asked to use each word in a sentence or explain its meaning. Whitaker (1972:66) reports that most of the nouns "were either used correctly or not at all and approximately half the verb stimuli were used correctly or not at all," and for these three patients the errors in response to the verb stimuli were either an "I don't know" response or a nominalization of the target word. Such nominalizations included the following examples (ibid.:67).

<table>
<thead>
<tr>
<th>(1-1)</th>
<th>patient</th>
<th>target</th>
<th>response</th>
</tr>
</thead>
<tbody>
<tr>
<td>K. T.</td>
<td>engage</td>
<td>My engagement is just about finished.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>admire</td>
<td>Your admirer, your tact.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>obstruct</td>
<td>Well, the obstruction here . . .</td>
<td></td>
</tr>
<tr>
<td>W. L.</td>
<td>conceal</td>
<td>concealment</td>
<td></td>
</tr>
<tr>
<td>F. W.</td>
<td>nominate</td>
<td>nomination</td>
<td></td>
</tr>
<tr>
<td></td>
<td>remember</td>
<td>memory</td>
<td></td>
</tr>
<tr>
<td></td>
<td>remove</td>
<td>movement</td>
<td></td>
</tr>
</tbody>
</table>

Many of the nominalizations in (1-1) result in a longer word, and at least a couple result in a less familiar and/or more abstract word. In addition, many of the error responses made by Whitaker's subjects in response to verb stimuli were derived nominals in "-ing" and gerundive nominals—e.g., "believing" for "believe," etc. (cf. the "verbs" in the
passage quoted from Goodglass on p. 14 above). Regarding associated grammatical problems in the patients discussed above, Whitaker (ibid.: 69) writes that "there is no evidence whatsoever that any of [these] patients . . . had normal control or command of such syntactic processes as question formation, negation, complementation or passivization."

The tasks described above involve the presentation of a single word at a time. There are, however, other tasks which reveal a noun facilitation effect in brain-damaged persons using sentential contexts.

For example, Marshall et al. (1970) also report on several tasks with the subject from their 1966 study designed to test the effects of contextual constraints on the subject's ability to produce nouns and verbs. Basically, Marshall et al. orally presented their patient sentences with blanks--sometimes for nouns, and sometimes for verbs--to be filled in. The sentences varied according to whether the choice for the blank was minimally, moderately or highly constrained (syntactically and semantically) by the context of the sentence. The subject correctly supplied nouns for five, four and two out of five highly, moderately and minimally constrained blanks, respectively. However, he correctly supplied zero, two and two verbs for the same respective categories. Thus, in general the subject was more successful at filling in the blanks when nouns were required than when verbs were called for (11/15 versus 4/15, for nouns and verbs respectively). Interestingly, while contextual constraints appeared to facilitate the subject's selection of nouns, the reverse tended to be true for the blanks requiring verbs, although it is hard to assess the significance of the low totals.
involved here.

Results similar to those discussed above regarding sentence completion with nouns and verbs have been reported for normal subjects. For example, Moore (1972) presented subjects with sentences from which either a noun or a verb had been deleted. Next, a "candidate" word for the blank was presented, and the subject's response latency for determining whether or not the resulting sentence was grammatical was measured. Predictably, the subjects took longer to decide on the grammaticality of a resulting sentence when the candidate word being supplied was a verb than when it was a noun. Of course, many questions come to mind in connection with such a procedure—in particular, the structure and complexity of the sentences involved. This would appear to be especially important for interpreting the results among the verbs in the light of the possibilities to be discussed in section 1.2.2.5 below (see especially the remarks regarding Fodor, Garrett and Bever (1968) on pp. 62-3).

Weigl and Bierwisch (1973) report results from a therapeutic task conducted with German-speaking brain-damaged subjects which goes one step beyond the studies discussed above with respect to the noun facilitation effect in a sentential context. They used the "de-blocking" procedure of Weigl (1961) to help effect a transfer from one brain-damaged patient's intact linguistic performance to the performance domain affected by the brain damage. This patient had a "severe expressive aphasia," alexia and agraphia (Weigl and Bierwisch 1973:24). However, her ability to copy written sentences was intact. By having the patient first copy a sentence several times, Weigl and
Bierwisch were able to have her produce the sentence in other performance modes, such as speaking. When a sentence was thus "deblocked," it was possible to have the patient substitute new nouns for the NP slots in the sentence, much along the lines of the pattern drills with substitutions used in second-language teaching. The authors claim (ibid.:25) that the substitution of nouns occurred "rather freely," although "directly bound to particular . . . semantic fields." On the other hand, "the substitution of verbs was almost impossible and required for the most part the deblocking of the new resulting sentence" (loc. cit.). What is interesting about these findings is that not only was the sentential context provided for the patient, but the words for substitution as well, thus eliminating the necessity of a lexical search on the part of the patient. Yet even under these favorable conditions, a noun facilitation effect was apparent.

So far, I have discussed production tasks with brain-damaged subjects which bear on the noun facilitation effect. Although many authors (e.g., Fodor, Bever and Garrett 1974) have pointed out the relative difficulty of obtaining as good linguistic production data as comprehension data in normals, the situation is somewhat reversed in investigations of language breakdown due to brain damage. In the case of linguistic performance by brain-damaged persons two things happen: First, it is much more difficult to evaluate comprehension because of the possibility of confounding due to the impairment of purely production mechanisms. Second, tasks--like simple repetition--which are usually uninteresting with normals because they are too simple to reveal differential processing (although sophisticated measuring techniques
might reveal such differences) become taxing for the brain-damaged patient, and hence of interest to the investigator. Accordingly, there is less to be said here about comprehension aspects of noun facilitation, although what there is tends to parallel what we know from production tasks. (Of course, some of the results of the production tasks discussed above—such as Goldstein's repetition task to auditorily presented stimuli (p. 16 above)—could reflect elements of comprehension problems as well.)

For example, although agrammatic patients have often been loosely characterized as having a relatively intact comprehension compared to their production ability, Caramazza and Zurif (1976) have shown that when such patients have to depend on purely syntactic cues (i.e., in the absence of any semantic cues) to comprehend auditorily presented sentences they perform at or below the level of chance. In addition, agrammatic patients whose speech shows an impairment of relational words have been shown to be significantly impaired in the comprehension of such words as well (e.g., Smith 1974). Goodglass (1973) discusses the syndrome of "conceptual agrammatism" which he says affects both the expression and the comprehension of the same grammatical discriminations; is "manifested in confusion among function words within the same class and confusion in the meaning of verb tenses" (ibid.:213-4); and which may or may not—according to Goodglass—appear in conjunction with symptoms of agrammatic speech. I know of no tests on the auditory comprehension of singly presented nouns and verbs. Such a test could, for example, use picture-matching response latencies as a performance measure. We might expect to find a noun facilitation effect under such
conditions on the basis of the tachistoscopic results discussed in section 1.1.1. above, since the tachistoscopic design constitutes a type of comprehension task.

From the above description of noun facilitation effects as a result of brain damage it appears that this phenomenon can appear in any of a variety of modalities or combinations of modalities. For example, Marshall and Newcombe's (1966) subject had the noun facilitation effect most apparent in his attempts to read out loud. He had no trouble with repetition tasks and was not particularly agrammatic in his speech. In contrast, I have mentioned Goldstein's (1948) and Halpern's (1965) finding of a noun facilitation effect in repetition tasks, and I have noted the kind of telegraphic speech which often accompanies a noun facilitation effect in brain-damaged people.

From these different manifestations of noun facilitation due to brain damage, it is not surprising to find that a variety of types of brain damage can lead to this effect, although damage to the anterior portion of the language dominant hemisphere appears to be the most often implicated. For example, Marshall and Newcombe's (1966) subject had damage as the result of a gunshot wound primarily in the left temporal and parietal cortex, but the invading bullet also "ploughed up . . . the vessels of the Sylvian fissure" (ibid.:169) which borders the anterior cortex. Whitaker's (1972) patient K. T. (see pp. 19ff. above) had damage in the parietal region and appears to be more of the "fluent" type of patient. Whitaker and Whitaker (1976:263) report that some "Wernicke's aphasics"--with damage to Wernicke's area in the temporal lobe--show a facilitation effect for nouns. A caveat:
Whitaker and Whitaker (ibid.:257) also point out that it is sometimes impossible to rule out damage to areas of the brain other than the site of a space-occupying lesion (such as a tumor) since increased pressure within the skull can result in atrophy and damage to parts of the brain remote from the actual site of the lesion.

Thus, the interaction between performance modalities and noun facilitation and the connection between various types of brain damage—e.g., damage to the anterior speech area—and noun facilitation effects appear to be rich research areas. There are, however, other complications which have some bearing on an interpretation of noun facilitation symptoms due to brain damage.

The complications consist principally of the pathological symptoms we must distinguish from noun facilitation per se. Some kind of word finding difficulty is common to all forms of linguistic breakdown due to brain damage (Whitaker, 1979 LSA summer course on aphasia), and this general category contains a variety of specific symptoms other than noun facilitation.

Chief among these symptoms is anomia, which Critchley (1970:10) defines as "an aphasic difficulty in supplying the appropriate term for a particular object at a given moment in time." Anomia of the pure form is usually associated with lesions in the temporal or temporo-parietal area, although here again intracranial pressure and atrophy can produce anomic symptoms with, for example, tumors outside the language areas of the brain (Goodglass and Geschwind 1976:404). Thus, anomia is often an accompaniment to Wernicke's aphasia, and anomics are also frequently fluent aphasics—articulating effortlessly in
speech devoid of sense. Anomia is evident, for example, in the classic confrontation naming task used clinically. In this kind of task, the patient is presented with an object—such as a comb—and asked to name it. Patients with anomia will often fail to correctly name such objects, even though they may use the required word in their spontaneous speech. As errors, these patients will sometimes produce incorrect names or circumlocutory utterances. Depending on the state of their auditory comprehension, anomic patients may or may not be aware of their naming errors.

It has been claimed that nouns per se present the greatest difficulty for anomias (e.g., Goodglass 1973:214). However, there are grounds for questioning such a claim. In the first place, the apparent exclusive difficulty with nouns in anomia may be partly an artifact of the kind of confrontation naming task often used to assess this problem—i.e., actions are relatively less often presented for naming by brain-damaged patients. Furthermore, it is widely admitted that other content words: verbs, adjectives and adverbs are also impaired in the speech of anomic patients (Goodglass and Geschwind 1976:402). Luria and Tsvetkova (1968:298) report that “patients with temporal (sensory) aphasia had severe difficulties both in naming objects and in naming actions” on a test where they were asked to name as many objects or actions as possible in a minute with their eyes closed. Wepman et al. (1973), in their important study of an anomic patient, discussed two hypotheses concerning the apparent difficulty with nouns in anomia. One hypothesis was that the difficulty is due to nominal quality per se; the other was that nouns are disproportionately
affected in anomia because the class of nouns contains many more low frequency words than the other parts of speech. Thus, "if words of lower frequency were suppressed from speech, the occurrence of nouns would be curtailed more severely than that of other parts of speech" (ibid.:222). Wepman et al. found support for the latter hypothesis, and not for the former, in their study. Along the same lines, Eisenson (1971:1228) states that

> Anomia refers to the patient's difficulty in invoking an appropriate term regardless of its part of speech. The defect is most likely to be evident in the effort to evoke nouns (nominal words) only because nouns constitute the bulk of most vocabularies. (Emphasis in original.)

However, Goodglass and Geschwind (1976:404) claim that "a close examination of Wepman et al.'s (1973) data indicates that the effect of this word frequency shift is much greater for nouns than other words." The question of whether frequency is the major factor in anomia thus appears undecided, although the evidence--including that to be discussed below--certainly leans against viewing anomia as a problem with nominal quality per se.

It is important to establish whether anomia constitutes an actual case of "noun defacilitation," because this will have a bearing on the type of explanation we posit for noun facilitation effects. Whitaker and Whitaker (1976:269-70) claim that "it is not possible to conclude that NOUN is hierarchically more important or significant than other categories" on the basis of noun facilitation phenomena due to brain damage since--they claim--the selective impairment of nouns exists as well. As an example of the latter they discuss a case reported in
Konorski (1967). Whitaker and Whitaker (op. cit.:270) claim that Konorski's patient's "speech was almost exclusively filled with verb forms and notably lacked noun forms." Konorski (op. cit.:248-249) does discuss such an apparent case of verb facilitation/noun defacilitation (although Whitaker and Whitaker have combined this patient's data with other examples given by Konorski (ibid.:249) which may not be from the same patient). Konorski's patient was an anomic with a "severe auditory-visual ('sensory') aphasia" (ibid.:248) as a result of a thrombosis of a small branch of the middle cerebral artery. His speech was fluent and coherent--although "rather logorrheic" (loc. cit.), but his comprehension was severely impaired. His symptoms included an inability to name objects presented to him, as shown in the following data quoted in full from Konorski (loc. cit.).

<table>
<thead>
<tr>
<th>(1-2) object shown to patient</th>
<th>response</th>
</tr>
</thead>
<tbody>
<tr>
<td>mirror</td>
<td>I know this, this is . . .</td>
</tr>
<tr>
<td>pencil</td>
<td>I know, this is for writing.</td>
</tr>
<tr>
<td>comb</td>
<td>I know this, its name is . . .</td>
</tr>
<tr>
<td>finger</td>
<td>These are my hands, one hand.</td>
</tr>
<tr>
<td>mouth</td>
<td>This is my . . . not hand . . . my body.</td>
</tr>
<tr>
<td>ear</td>
<td>This is my head, my head composed of two parts.</td>
</tr>
</tbody>
</table>

The patient could read and name the letters of the alphabet. In addition, he could name actions performed in front of him, such as "Now you sit down. You walk. You run." (ibid.:249).

There are a number of points which should be made in connection with Whitaker and Whitaker's (1976) conclusions based on Konorski's data. First, many of the patient's responses (i.e., "name," "hand," "body," "head," "parts") in the object naming task were nouns, although
not the correct nouns, a fact not pointed out by Whitaker and Whitaker.
In fact, the absolute number of verbs in (1-2)—counting "is . . . composed" as one—is nine, equal to the number of nouns—not counting pronouns, but counting "writing." Also note that patients who show a noun facilitation effect—such as the one discussed in Marshall and Newcombe (1966) (see p. 17 above)—tend not to produce verbs as error responses to verb stimuli. Second, the actions named by Konorski's patient—apparently with correct inflections, although this is not clear from the translation of the Polish data—were certainly of the highest level of familiarity and thus fit Wepman et al.'s hypothesis regarding anomia (see p. 28 above). Third, the "-ing" forms which occur both in the data from the above patient and in Konorski's other samples (loc. cit.) mentioned in connection with naming difficulties would presumably be verb-derived nominals in Polish (again, if the English translation is accurate). Such forms in Polish are more clearly nominal in nature than gerundive nominals in English (Anatole Lyovin, personal communication), since they must be inflected with the nominal portmanteau suffix for gender, number and case. We know that the "-ing" forms in Konorski's translation of his Polish data must be this type of verb-derived nominal—rather than, say, the infinitive form of the verb (cf. "I like to swim" and "I like swimming" which have parallel Polish constructions)—since in the type of examples Konorski cites (ibid.:248-9) only the nominal form would be grammatical. That is, in the "This is for _______ (e.g., "writing," "cutting," "smoking")" frame which Konorski cites, verb-derived nominals must be used and inflected for neuter gender, singular number and accusative case.
From the above, it would appear reasonable to conclude that it is at least questionable whether patients like Konorski's represent true cases of verb facilitation/noun defacilitation. If, for example, such patients' problem is with nominal quality per se, why should they frequently resort to a nominal form of the verb or an incorrect noun when trying to name an object?

Even granting—for the sake of argument—that cases of verb facilitation/noun defacilitation due to brain damage exist, there is still another consideration which militates against Whitaker and Whitaker's (op. cit.:270) conclusion that "it is not possible to conclude that NOUN is hierarchically more important or significant than other categories." I know of no finding with normal subjects which could be taken as an analog of the postulated verb facilitation which appears as a result of brain damage. The experimental results with normal subjects I have discussed above, and the facts from language acquisition as well, point to a generally greater processing ease for nouns than for verbs as the normal case. Thus, the "hierarchical significance" (whatever it turns out to be) that Whitaker and Whitaker seem hesitant to apply to an explanation of noun facilitation as a result of brain damage appears to be necessary to explain normal processing anyway. The cause of Whitaker and Whitaker's hesitation in this regard seems to be the assumption that verb facilitation would contradict an explanation of noun facilitation as a result of brain damage according to some type of hierarchy since verbs—as the most difficult items in the posited hierarchy—should never be impaired.
before nouns. But this assumption overlooks the possibility of impairment according to different variables and "hierarchies" depending on the nature of the brain damage—if indeed there exist cases of verb facilitation.

There are still other brain damage related symptoms falling under the general heading of word finding difficulties which might complicate a clinical picture of noun facilitation. These include various types of paraphasia and sensory disruptions.

Paraphasia involves "any error of commission modifying the individual word (sound and morpheme substitution) or of word substitution in . . . spoken or written production" (Eisenson 1971:1226). In verbal paraphasia substituted words often bear a relation to the target word of "categorical approximation" (e.g., "shaver" for "razor") (Brown 1977:36). Other forms of paraphasia are commonly delimited (e.g., as in Brown 1977). Neologisms and—in extreme cases—jargon may also characterize a patient's difficulty. Such disturbances are associated, again, with posterior brain damage and tend to affect content words in general.

Sensory disturbances such as the various agnosias can also result in symptoms involving the inappropriate usage of content words. In general, agnosias—as originally defined by Freud (1953)—are "states in which the individual is able to receive sensory impressions but is unable to recognize (appreciate) their significance" (Eisenson 1971:1225). Agnosias may exist in the auditory, visual or tactile modalities. In a visual agnosia, for example, patients presented with objects for recognition may sense they are seeing something but will not
recognize what. Obviously, a patient with such visual agnosia will perform very poorly on confrontation naming tasks, but for a different reason from that underlying a poor naming performance by an anomic. In confrontation naming, agrammatic patients will have relatively less trouble than anomies in finding the word they want (Brown 1972:115). Anomics will have trouble finding the word. Patients with visual agnosia will not know for which word to search. Evidently, some of the symptoms from these underlying etiologies are going to overlap in superficial appearance. Furthermore, agnosias—at least in the visual and auditory modalities—as well as the various disconnections that may occur between sensory input and the language areas (e.g., the case where objects can be recognized visually but cannot be named until picked up and handled (see Goodglass and Geschwind 1976:403)) are the result of posterior brain damage and, hence, are likely to be complicating factors in anomia. Thus, such factors can contribute to an apparent greater difficulty with names of objects in anomia.

Relatively, Konorski (1967:249) himself has offered an explanation of why his patient (discussed above, pp. 29–31) had an apparent difficulty with nouns on the basis of just such syndromes. Konorski also suggests (loc. cit.) that his patient's ability to name actions performed in front of him occurred because "perhaps when seeing these actions he imagined himself doing them, and their naming occurred through [unimpaired] direct body-kinesthetic-word-kinesthetic connections."

Even more peripheral sensory disorders—i.e., those interfering with the actual reception of sense impressions—can contribute to complications regarding the interpretation of word finding difficulties
due to brain damage. For example, Whitaker (1979 LSA summer course on aphasia) has suggested that the reduction in visual field which often accompanies brain damage (again, in the posterior region) can result in pseudo-derivational errors if a patient is--say--unable to visually span at one time an entire word presented to him/her for reading aloud. For further remarks related to this question see p. 59 in section 1.2.2.5. below.

1.2. Attempted Explanations of, and Inferences from, Noun Facilitation Phenomena

1.2.0. Introduction

There is a very large number of linguistic and other cognitive factors--an "embarrassment of variables" according to Marshall et al. (1970:417)--which play a potential role in noun facilitation phenomena. In my review of opposed explanations of noun facilitation in this section I will not be trying to rule out completely the contribution of certain factors while supporting unequivocally the role of others. Rather, I will attempt to show what aspects of noun facilitation phenomena remain unexplained under each theory and after all relevant theories have been taken into account.

I will first discuss the possibility of single-factor explanations of noun facilitation. By "single-factor explanations" I intend those that might attribute the facility for nouns to a factor--such as concreteness--which lexical items or their referents may exhibit. I will then discuss several integrative explanations--i.e., those that try to relate noun facilitation to more general facts of cognitive function.
1.2.1. Single-Factor Approaches

In the sections above I have mentioned the following factors which may appear connected with noun facilitation phenomena, at least under certain circumstances: concreteness, frequency/familiarity, orthographic length, and morphological complexity. I will discuss these and other relevant factors in turn below.

Since the studies cited in section 1.1.1. used mostly concrete nouns as stimuli, this factor readily suggests itself in partial explanation of the noun facilitation effect. This is lent some support by the fact that brain damaged persons often show an impaired ability with regards to abstract words and concepts (Halpern 1965). However, this factor alone clearly will not account for all noun facilitation phenomena. Halpern (ibid.) still found a noun facilitation effect for dyslexics in a reading aloud task with nouns, adjectives and verbs balanced for rated abstractness. Many of the nominalizations reported by Whitaker (1972:67-8)--and noted on p. 20 above--result in more abstract items. There is also some doubt whether concreteness is a facilitating factor in tachistoscopic tasks. Paivio, Yuille and Madigan (1968) have defined a word's "concreteness" as the extent to which its referent can be experienced by the senses. They also define a word's "imageability" as the extent to which it elicits a mental image, and they report that the correlation between these two attributes is very high, +0.83. Paivio and O'Neill (1970) found that when familiarity and verbal associative meaningfulness were controlled, imageability did not have a facilitating effect on the tachistoscopic recognition of words. Paivio and O'Neill (ibid.:273) suggest,
regarding the Riegel and Riegel (1961) study discussed in 1.1.1. above, that

32% of the stimulus items used were from word classes other than nouns. All but one of these items were in the highest [i.e., longer lengths of time for recognition] 50% in terms of threshold values, suggesting that word class per se rather than concreteness may have been the effective variable in their study.

In Chapter 2 I will present the results of an experiment which demonstrate a noun facilitation effect with concreteness and imageability fully controlled. The same experiment also controlled the factor of verbal associative meaningfulness—the role of which in tachistoscopic tasks Paivio and O'Neill regard as uncertain—across noun and verb categories.

Gardner (1973) has shown that "operativity"—a factor closely related to those discussed above—has a facilitating effect when brain-damaged patients are asked to name pictures. "Operative" objects—as defined by Piaget (1970)—are those which can be grasped, manipulated and operated upon. However, with regards to noun facilitation, the remarks made above with respect to concreteness and imageability appear to apply to operativity as well.

Frequency of occurrence (and its corollary, subjective familiarity) --highly effective factors in all linguistic processing—also cannot be used to explain noun facilitation since in many of the studies discussed above this factor was controlled. Moreover, even though Marshall and Holmes (1974)—see p. 4 above—have reported that in tachistoscopic tasks with normals noun facilitation operates primarily among low frequency words, we have seen how a noun facilitation effect
after brain damage can override even high familiarity values for verbs and adjectives (see pp. 17, 19 and 20 above).

There are, however, many elaborations that can be made with respect to the possible role of frequency/familiarity as factors in noun facilitation.

One problematic consideration regarding familiarity effects has to do with a suggestion made by Riegel and Riegel (1961). They claim (ibid.:168) that the familiarity of lexical items is a function of the frequency of a word's occurrence plus the experiential frequency of the word's referent in the real world. Thus, the frequency with which cats are encountered in the real world would somehow summate with the linguistic frequency of the word "cats." Marshall, Newcombe and Holmes (1975:319) correctly note that this brings up an added relation between frequency and concreteness. They (ibid.:319-20) claim that while Riegel and Riegel's idea is "conceivable for concrete nouns, it is, of course, considerably less plausible for many instances of other parts of speech." It was not Marshall et al.'s intention to suggest an explanation for noun facilitation on the basis of frequency with the foregoing remark, but one could be inferred from it. That is, one might hypothesize that the kind of experiential familiarity suggested by Riegel and Riegel (1961) does summate with word frequency in the case of concrete nouns but not in the case of other words. This is unlikely for two reasons. First, there would still be the problem of abstract nouns which would presumably be subject to Marshall et al.'s (1975:319-20) doubt regarding Riegel and Riegel's idea. Many of the nominalizations made by brain-damaged patients are abstract nouns, as we
have seen (p. 20). Second, given our general lack of insight regarding the "language of thought" (e.g., when we walk does the word "walk" occur to us?), I feel that Riegel and Riegel's suggestion is still an open question—even for verbs and abstract nouns.

Another variation on the theme of frequency: Caplan, Holmes and Marshall (1974:334-5) suggest that the frequency of a derived form (such as "nomination") might summate with the frequency of its base form (i.e., "nominate"), but not vice versa. Of course, this suggestion will not help us account for noun facilitation with non-derived nouns, but it could conceivably figure in an explanation of the kinds of nominalizations discussed above (e.g., p. 20).

Another variation: Roydes and Osgood (1972:174) suggest that there is an overall greater number of nouns as compared with verbs in English. One might imagine that this overall greater number accounts for part of the noun facilitation effect. However, although Roydes and Osgood's suggestion may be true for type frequency it is not necessarily true for token frequency. Thus, Wepman et al. (1973:221-3) do not report any great difference between the overall number of occurrences of nouns versus verbs in either their compilations from Thorndike and Lorge's (1944) word counts of published material (2,298 nouns, 2,069 verbs/10,000 words) or in the spontaneous speech of a normal subject (1,404 nouns, 1,516 verbs/10,000 words). In addition, Roydes and Osgood's suggestion seems unlikely to be true in the face of the substitution of much less frequent noun forms for verb forms as already noted.

The problem regarding frequency effects in experimental investigations of noun facilitation is to control stimuli along all the
different lines discussed above. The experiment which I will present in
Chapter 2 effectively does just this with a special set of stimuli
from Japanese.

Orthographic length is not a factor when it comes to explaining
noun facilitation since this too has been well controlled in the studies
discussed above. Furthermore, we have also seen how noun facilitation
can sometimes override the effect of orthographic length. For example,
Holmes et al. (1971) showed that increasing orthographic length caused
a difficulty for their dyslexics in a reading aloud task (see p. 18
above). However, Marshall et al.'s (1970) patient read verb-derived
nominals much better than their related verbs despite a mostly increased
orthographic length. The noun facilitation effect can also override
the effects of articulatory length on performance by brain-damaged
persons. For example, although articulatory difficulty often
accompanies expressive, non-fluent language disorders (usually
associated with anterior brain damage), there are such patients who
still find it easier to produce longer, verb-derived nominals than
corresponding root verbs (see the examples from Whitaker's patient F. W.
noted on p. 20 above).

Another factor related to orthography—and to the auditory
perception of words as well—has to do with word-internal orthographic/
phonological predictability, redundancy, transition probabilities,
etc. For example, in English relatively few words—and infrequent ones
at that—begin with "xy-" in written form, while a multitude begin
with "un-". Accordingly, in German Riegel and Riegel (1961:162) found
that "if a rare syllable appears at the beginning of a word it will
facilitate [tachistoscopic] recognition." Riegel and Riegel's study also discusses many of the other possibilities along these lines. Here, there is not much hope for an explanation of all noun facilitation phenomena. Such considerations do not, for example, help us to explain nominalizations (such as "nomination" for "nominate") by patients who show a noun facilitation effect. However, tachistoscopic studies and tests with brain-damaged subjects have not always explicitly controlled such variables across noun and verb stimuli. Caplan et al. (1974:332) have noted that—together with other variables such as length, frequency, etc.—"controlling for such a large number of variables may prove impossible in ordinary factorial designs." Such a control would, however, appear to be especially important in connection with brain-damaged subjects where the kind of perceptual problems discussed above (p. 34) may be operating. The experiment to be reported in Chapter 2 uses a special set of stimuli from Japanese uniquely suited to controlling word-internal redundancy and other factors across noun and verb stimuli.

This brings me to the role morphological complexity may play in noun facilitation phenomena. Although the evidence with respect to the effect of such complexity on linguistic performance in general is somewhat ambiguous, it is clear from the kinds of substitutions reported in Whitaker (1972) that this factor cannot be used to explain all noun facilitation. For example, Garrett (1978:617) reports that "MacKay [unpublished] found that derivational complexity predicted reaction time performance in a task requiring subjects to produce nominalizations of verb stems (e.g., decide → decision)." However,
this is precisely the type of error we have seen above as a result of noun facilitation in the data quoted from Whitaker (1972), Marshall et al. (1970) and others. Richardson (1975:283) defines a "derived noun" as "a noun which consists of an explicit noun-forming suffix, together with a stem which is identical to, or is a minimally modified form of, an English word in general usage." Using this definition, Richardson further found that there was "no difference between simple and derived nouns in terms of the accuracy of the . . . response" (ibid.:287)—using the same subject reported on in Marshall and Newcombe (1966) and Marshall et al. (1970).

Related to the question of morphological complexity discussed above is the fact that in many languages nouns are relatively uninflected—i.e., undergo few changes to the stem to indicate case, as well as number. In English, for example, nouns are inflected for number only. On the other hand, English verbs are inflected for person, number and tense. It could be imagined that the relative lack of inflections for nouns in some languages lies at the base of the noun facilitation phenomena I have discussed. This does not seem capable of explaining such phenomena in all languages, however, since for Russian—where nouns are inflected via a portmanteau morpheme for gender, number and case—there are also reports of noun facilitation phenomena, such as Luria and Tsvetkova (1968).

Other factors which lexical items and/or their referents exhibit and which have been shown to play a role in normal and/or pathological linguistic processing include age-at-acquisition and a host of semantic features.
Rochford and Williams (1962) have shown that there is a correlation between the acquisition of vocabulary in children and the breakdown of the usage of names in anomia. That is, words acquired early tend to be more resistant to insult. Lyons, Teer and Rubenstein (1978:179) have reported that when frequency was controlled "words judged to be of early acquisition had a significantly lower [tachistoscopic] recognition threshold than words judged to be of later acquisition." Since I have noted in section 1.1.2. above that nouns tend to predominate in the speech of young children, it is reasonable to speculate on the connection that might exist between noun facilitation phenomena and age-at-acquisition. This is not to say that we will be able to adequately explain noun facilitation on the basis of age-at-acquisition alone since--if possible--this would still leave unresolved the cause(s) of an earlier age-at-acquisition for nouns. Rather, it is more interesting to try to determine a set of causal factors which can explain both age-at-acquisition and processing phenomena in terms of general cognitive functions. I will have more to say on this topic in section 1.2.2.5. below and in Chapter 2.

There is a variety of evidence in the language pathology literature which indicates that a multitude of semantic categories and features can be selectively impaired/spared as a result of brain damage. Marshall et al.'s (1970) subject (the same subject discussed in Marshall and Newcombe (1966)) found [+ ANIMATE] nouns more difficult than [- ANIMATE] ones, [+ HUMAN] nouns more difficult than [- HUMAN] animate ones, and proper nouns more difficult than common ones. Goodglass, Klein, Carey and Jones (1973) studied 135 brain-damaged
patients "for their proficiency in naming objects, colors, numbers, letters, and actions, and for their auditory comprehension of words in these categories, plus the additional categories of 'geometric forms' and 'body parts'" (ibid.:265). Their study was prompted by the fact that brain-damaged patients sometimes have problems disproportionately restricted to one or more of these categories—such as colors or body parts. Such selective impairments are sometimes a manifestation of the disconnection syndromes mentioned above (p. 33). One interesting finding reported by Goodglass et al. (ibid.:265) was that Broca's aphasics—with anterior brain damage—"showed no significant differences between the various semantic classes in their naming proficiency." The above remarks are intended merely to illustrate the kinds of semantic considerations which can be brought to bear in questions of word finding difficulty. Marshall et al. (1970:417) have written "one might inquire whether features such as Mass, Human, Proper, etc. influence the ability to read nouns." By extension, we might ask whether it might be possible to combine some of the above factors into a unified hypothesis about noun facilitation. But it does not seem likely that such a course will explain all noun facilitation phenomena in the light of the above finding regarding Broca's aphasics, who often show a noun facilitation effect. Some of the nominalizations reported in Whitaker (1972)—see p. 20—(such as "nomination" for "nominate") appear to change very little in the way of semantic features. In Chapter 2 I will present the results of a tachistoscopic experiment in which nouns were significantly easier than verbs with semantic features controlled across the two categories.
This concludes my discussion of single-factor approaches to noun facilitation phenomena. Again, the factors discussed above do have an attested effect in some types of normal and/or pathological linguistic processing, and thus can be expected to influence the appearance of noun facilitation symptoms to some extent, although it is unlikely that any or all of these factors can fully explain noun facilitation per se.

1.2.2. Integrative Approaches to Noun Facilitation

1.2.2.1. Luria and Tsvetkova

Luria and Tsvetkova's (1968) paper was mentioned in section 1.1.3. (pp. 15-6). They use the phrase "dynamic aphasia" to designate a symptom complex (resulting from anterior brain damage) which includes a distinct facilitation for nouns. As the fundamental defect in this disorder they posit a loss of the "predicative function" (ibid.:297) of language, or a defect in "active propositionizing" (ibid.:303). It is very unclear whether Luria and Tsvetkova intend these statements as a simple characterization of the symptoms of the disorder they describe --including the noun facilitation shown by dynamic aphasics-- or an explanation of these symptoms, since the key terms "predicative function" are never defined in either Luria and Tsvetkova (1968), Luria (1970:199-216)--where dynamic aphasia is also discussed, or even in Vygotsky (1962)--the work which forms much of the basis for Luria and Tsvetkova's statements. Thus, to a certain extent, Luria and Tsvetkova (1968) appear to beg the issue when they characterize the dynamic aphasic's problems with verbs as being due to a loss of the "predicative function" of language--this comes perilously close to
saying that verbs are difficult for these patients because their use of verbs is impaired. In addition, without further definition Luria and Tsvetkova's remarks would appear to have very little to say about the predominance of nouns in language acquisition, noun facilitation in normals under certain experimental task situations, or the connection between noun facilitation due to brain damage and accompanying grammatical problems. There are, however, some points which should still be made in connection with Luria and Tsvetkova's (1968) paper.

The view of the human frontal cortex (the area damaged in dynamic aphasia) underlying Luria and Tsvetkova's remarks is in agreement with the general view of this area as the "executive of the brain" (Pribram 1973). "In short, the frontal cortex appears critically involved in implementing executive programs when these are necessary to maintain brain organization in the face of insufficient redundancy in input processing and in the outcomes of behavior" (ibid.:312). However, while I do not disagree with this view of the frontal cortex, we have seen above that noun facilitation phenomena as a result of brain damage might not always be due to frontal lobe lesions, although this is to some extent still an open question as noted above (p. 26).

Luria and Tsvetkova (as well as Luria (1970)) also imply in the course of their discussion that the dynamic aphasic has a preference for words that are somehow "static" in nature (Goodglass 1976:241). In this connection, Luria (1970:209-10) discusses "adynamia of thought" as part of the picture of dynamic aphasia. Patients with "extreme adynamia of thought"
complain that nothing comes into their heads, that it takes them a number of hours or several days to write a simple letter, and that, having begun to write, they are unable to go beyond stereotyped greetings (loc. cit.).

With regards to this aspect of Luria and Tsvetkova's paper, we might ask in what way the nominalizations of verbs (including gerundive nominalizations), which patients who show a noun facilitation effect often produce (see p. 20 above), are more "static" than their associated verb form. Such a difference may exist, since nominalizations of Russian verbs, for example, would lack tense and aspect, although Luria and Tsvetkova have taken no steps towards clarifying what it might be. In any case, the small amount of data relevant to this question suggests that no such difference is a necessary part of the noun facilitation picture. Marshall et al. (1979:419-20) found that their patient showed very little difference between stative and non-stative verbs and adjectives on a reading aloud test, although he at least attempted to read many more of the non-stative words. Marshall et al. (ibid.:420) also suggest that Halpern's result (Halpern, 1965) on the effect of an 'abstract-concrete' dimension [i.e., concrete words were easier for Halpern's patients] may be interpretable in terms of the feature - Stative. Halpern's highly abstract verbs are predominantly Stative and his concrete verbs predominantly Non-stative.

If one had access to the proper type of patient(s) and sufficient time, one way of further testing this (possible) implication regarding the feature (+ STATIVE) of Luria and Tsvetkova's account of noun facilitation would be to use a deblocking therapy--like that of Weigl (1961) (see p. 22 above)--with noun facilitation patients to see if they "deblocked" more easily with stative verbs and adjectives than with
non-stative ones. Of course, in any such studies special care should be paid to controlling extraneous variables, since verbs and adjectives which differ according to the feature [+ STATIVE] may also differ with respect to other features, such as transitivity (for the relevance of this feature see section 1.2.2.5. below). The experiment on noun facilitation to be discussed in Chapter 2 controls such factors as [+ STATIVE] and transitivity across nouns and verbs.

1.2.2.2. Goodglass

Goodglass (1973) offers the "stress-saliency" hypothesis to explain many of the symptoms of agrammatism.

Goodglass (ibid.:204) describes "saliency" as "the psychological resultant of the stress, of the informational significance, of the phonological prominence, and of the affective value of the word."

According to this suggestion, agrammatic patients try to "break into" speech by

substituting salient words in normally unstressed sentence positions or by making one-word sentences in which, as Jakobson [(1973)] points out, all words whether they represent actions or things are reduced to a nominal function (ibid.:213).

These remarks are similar to those put forth in Jakobson (1973).

Goodglass' idea is also partly related to Luria and Tsvetkova's ideas discussed in the previous section in that Goodglass (ibid.:204) sees the agrammatic's need for a salient word to initiate speech as being due to an underlying defect of "an increased threshold for initiating and maintaining the flow of speech."

Although there is much validity to Goodglass' description of the interaction of stress plus other factors and the difficulties of
agrammatic patients, there are problematic points involved with his proposals if we regard them as an attempted explanation of the noun facilitation phenomena—including the nominalizing tendency—often shown by agrammatics. Goodglass is not very clear in this regard, although his implication seems to be that the concept of saliency might help explain noun facilitation—e.g., "stress and nominal quality may be related," or "the agrammatic's need for salient or nominal features . . ." (ibid.:213). Most of the problem points arise from the fact that Goodglass (as in the above quotes) appears to define nominal quality per se as more salient than verbal quality. Thus, to a certain extent, using the concept of saliency to explain noun facilitation appears to beg the issue. In terms of the definitional factors of saliency proposed by Goodglass—i.e., possibility of occurring in stressed position, informational significance, etc.—there would appear to be no reason to regard verbs as any less salient than nouns. As Goodglass (ibid.:204) himself notes, "the patient with [agrammatism] must find the salient point in his intended utterance, which is ordinarily the significant noun or verb."

The concept of saliency as outlined by Goodglass in general also fails to relate noun facilitation as a result of brain damage to the grammatical nature of the problems often shown by agrammatic and other patients with a noun facilitation effect. Furthermore, verbs are often omitted by agrammatics even when they would normally occur in a stressed position (see the example from Goodglas, p.14 above), and Weigl and Bierwisch (1973:25) report that it is more difficult to deblock verbs, even though these seem to occur in a stressed position.
Goodglass (1973:197) does report on the relative facility shown by agrammatics for producing imperatives in sentences with four-five words. Such a finding is open to other interpretations besides those suggested by Goodglass—i.e., having the verb in sentence-initial position in the imperative form gives it added stress and psychological prominence. For example, we might want to view imperatives as automatized syntactic frames with slots which can be selectively spared in cases of brain damage. Such an explanation might also be applied to the retention of function words between stressed elements also noted by Goodglass (1973:213). "Alajouanine (1968) has noted that agrammatism spares automatic or stereotypic expressions" (Brown 1972:110). (I will discuss another possible interpretation of these results with imperatives in section 1.2.2.5. below.) Of course, a combination of factors may be responsible for the facility with imperatives sometimes seen in agrammatic patients.

Goodglass' approach to noun facilitation phenomena as a result of brain damage has little to say about noun facilitation in experimental tasks with normals, such as the tachistoscopic designs discussed in section 1.1.1.—where no context contributes to the saliency of the stimuli used—or the kind of experiment using sentential contexts reported in Moore (1972) (see p. 22 above). Nor, for that matter, does it have much to say about noun facilitation in the kind of repetition task with brain-damaged patients reported by Goldstein (1948:238).

There are possible additions to the saliency theory proposed by Goodglass which might be made to account for these results. For
example, we might imagine that in English one expects to encounter a noun at the beginning of a sentence and that in one-word recognition tasks words are somehow treated as potential sentence initiators (cf. Roydes and Osgood (1972:174) regarding noun facilitation as a result of a "general human set towards the topics of sentences"). However, besides the fact that a great number of English sentences (including imperatives) do not begin with nouns, such a claim would do little to explain the kind of noun facilitation in context reported in Moore (1972). Ultimately, it would be useful to investigate noun facilitation effects in a true verb-initial language, although any support for an explanation of noun facilitation along these lines seems unlikely.

Russian—as a highly inflected language—permits verb-initial constructions to a much greater degree than English and still shows noun facilitation phenomena. Relatedly, it might be claimed that nouns more often occur as one-word utterances—such as in response to WH questions. Although this may be true for English, it is not true in Japanese where noun facilitation phenomena similar to those I have been discussing have been reported (and will also be reported in connection with the experiment to be discussed in Chapter 2). In Japanese, verbs very often appear as one-word answers to yes-no and other questions, in contrast with English. Also, such an attempted explanation of noun facilitation still seems obviated by the existence of the above-mentioned cases of noun facilitation in sentential contexts.

Stress, intonation patterns and other elements of saliency as defined by Goodglass certainly play a role in language acquisition. We might even want to go so far as to claim that some children adopt as an
acquisition strategy the filling in of stressed points of acquired intonation patterns for multi-morphemic utterances in adult speech (cf. Peters 1977). Here again, however, main verbs and adjectives should be among the earlier items acquired on the basis of stress alone--although prepositions, auxiliaries, etc. would certainly fare worse.

1.2.2.3. Kean

Kean's (1978) view of the underlying defect in the syndrome called "Broca's aphasia" (which usually includes agrammatism as a principal feature depending on the severity of the brain damage, the exact location of the damage, and whose terminology is adopted (cf. Brown 1972:109)) is easily stated. Basically, she argues that all of the problems involved with this syndrome--not only the phonemic paraphasias, but the morphological, lexical and syntactic deficits as well--are the result of a uniquely phonological impairment. Thus, she postulates (ibid.:88) that

a Broca's aphasic tends to reduce the structure of a sentence to the minimal string of elements which can be lexically construed as phonological words in his language.

She further states (ibid.:89) that

the sentences of a Broca's aphasis are assumed [i.e., by Kean] to be well formed. The apparent lack of well-formedness arises not from a deficit which is intrinsically syntactic but rather from the reduction of the phonological structure of a sentence.

Function words--according to Kean (ibid.:87-8)--do not constitute phonological words, since she (loc. cit.) defines a "phonological word" as "the domain over which the assignment of stress [takes] place."
Here, I cannot evaluate the validity of Kean's general explanation of the entire syndrome of Broca's aphasia, but I will discuss the implications of her hypotheses for noun facilitation. Of course, my discussion of this aspect of her theory will have some relevance to the validity of her total neurolinguistic theory of Broca's aphasia.

Kean does not discuss noun facilitation per se. Rather, she makes certain predictions regarding processing ease for major, stressed content words in Broca's aphasia solely on the basis of phonological considerations. Taken in connection with her broader claim that agrammatism involves only a phonological deficit, this amounts to a claim about the causation of noun facilitation in cases of agrammatism. (One problem with the applicability of Kean's approach to noun facilitation in general that should be noted before going any further is that—as we have seen—noun facilitation phenomena are not restricted to cases of Broca's aphasia, and probably not even to cases of anterior brain damage.) Basically, Kean posits a tendency to reduce phonological complexity on the part of agrammatics by deleting inflections and other "non-words" which the agrammatic does not "attend to." And it is predicted that inflectional affixes and function words . . . will be more deletable than derivational affixes and that among the derivational affixes, productive affixes (such as the -s plural) will be deleted more often than affixes which are integral to the base form of a lexical item (ibid.:96).

The published data on noun facilitation simply do not support Kean's views on the nature of the underlying defect in agrammatism and its possible role in why nouns should be easier for agrammatics, and certainly cannot be used to account for noun facilitation phenomena in general.
To begin with, "one consistent grammatical deviation attributable to agrammatics is the aphasics' overuse of the present progressive, both correctly and incorrectly" (Goodglass 1973:212; see also Lesser 1978:126).

Not only do Broca's aphasics show a greater number of correct performances with the present progressive than with any other [verb tense] form, but they more frequently use it as an incorrect substitute for other forms (Goodglass 1973:200). (Emphasis added.)

According to Kean (1978:85-6), "-ing" is an inflectional affix and thus should tend to be omitted in favor of a shorter form of the verb according to her hypothesis. We have already seen from data in Whitaker (1972) and Marshall et al. (1970) that phonological length and morphological complexity can be overridden completely by nominal quality in cases of noun facilitation, whether they are classifiable as cases of agrammatism or not. Furthermore, running stretches of the speech of agrammatics often show nouns strung together--but separated by pauses--with a connecting verb deleted (see the example of such speech quoted from Goodglass (1973) on p. 14 above). Why should verbs--as major, stressed content words--be deleted altogether rather than simply stripped of inflections, as Kean's hypothesis suggests? Note that the nouns which are produced in the kind of telegraphic speech quoted on p. 14 above are not restricted to sentence-initial NP's. For example, consider the following subsection of the text quoted on p. 14: "Monday . . . ah . . . Dad and Peter Hogan, and Dad . . . ah . . . Hospital." Aside from the duplication of the word "Dad," the only items needed to make this a grammatical utterance are a verb of motion with a preposition to connect the sequence "Dad and Peter Hogan" with what is
clearly the GOAL NP, "Hospital."

Further evidence against Kean's proposals comes from Japanese. Sasanuma (in press) discusses a tendency among some brain-damaged patients to produce semantically related nominal compounds when asked to read aloud single kanji (Sino-Japanese ideographs). Thus, common Japanese kanji which can be used perfectly well alone such as 手 "te" ("hand") and 眼 "me" ("eye") are read as 手袋 "tebukuro" ("gloves," literally, "hand-sacks") and 眼鏡 "megane" ("eyeglasses," literally, "eye-mirrors"). (Note the similarity between this tendency and Marshall and Newcombe's (1966) "visual completions" (see pp. 58-9 below).) Such compound formations—which increase the morphological and phonological complexity of the target kanji—occur frequently among Japanese patients, including some agrammatics who show a noun facilitation effect.

Kean's phonological hypotheses obviously cannot do much to explain the tachistoscopic results with normals discussed in section 1.1.1., since in these tasks length and morphological complexity were controlled. In studies such as Holmes et al. (1971) the phonologically simplest form of the verb—i.e., its form following the infinitive "to" (e.g., "go") was used, and all of the stimuli used—nouns and verbs—were "major lexical items which carry word stress [and which, according to Kean] are fully attended to [by Broca's aphasics]" (Kean 1978:128). It has been suggested (e.g., Marshall and Newcombe 1973:191) that in normals under tachistoscopic demands there is little or no phonological mediation at any rate. That is, under such conditions words are most probably recognized via a direct
visual-semantic path (with the semantic reading providing the phonological form for the oral response of the subjects), rather than via a visual-phonological-semantic path. I interpret this as further possible evidence that phonology cannot be the causal factor in noun facilitation. At least, Marshall and Newcombe's (1973) suggestion would disallow a purely phonological explanation which can account for both the tachistoscopic results and the brain damage facts, such as the parallel findings reported in Holmes et al. (1971) between their tachistoscopic results with normals and their results using the same stimuli as a reading aloud task with dyslexics (see p. 18 above). In Chapter 2 I will demonstrate a noun facilitation effect using derivationally related noun-verb pairs from Japanese (in a tachistoscopic design and in a brain-damaged patient) which are identical phonologically and morphologically except for a final vowel which determines whether each stimulus word's suffix is nominal or verbal in nature.

In summary, Kean's proposals can neither account for noun facilitation phenomena in general as a result of brain damage (which they were probably not intended to account for) nor for the noun facilitation specifically seen in cases of agrammatism (which they were intended to account for). This, in turn, counts as a partial disconfirmation of her view of the underlying defect in Broca's aphasia.

1.2.2.4. Whitaker

Whitaker (1972) uses data from the three patients discussed above (pp. 19ff.) to say something about the lexicalist-generative semanticist
debate, without going into any speculations on the causation of noun facilitation beyond the assumption that "the aphasic impairment itself ... hierarchizes nouns and verbs in the process of lexical retrieval" (ibid.:70). It should be noted that this paper predates that of Whitaker and Whitaker (1976) discussed above (pp. 28-9) in which it was claimed that "it is not possible to conclude that NOUN is hierarchically more important or significant than other categories" (ibid.:269-70). Presumably, Whitaker would no longer make the kind of claims to be discussed below.

Basically, Whitaker (1972) presents the argument that the facilitation of derived nominals in his three patients is problematic for generative semantic theory since it seems to imply that more transformationally complex forms (i.e., derived nominals) are spared in brain damage.

If we are to maintain that derived nominals are transformationally related to their source verbs, it is clear that such a model would account for the [aphasic noun facilitation] verbal behavior in question only by a complicated and rather suspect set of principles of brain function. In spite of serious impairment in the use of very general features and rules in the grammar, one would have to argue that these patients were able to make use of a transformational rule which, on good theoretical evidence, must keep track of highly idiosyncratic semantic and syntactic properties (ibid.:70).

Of course, Whitaker's arguments above in favor of a lexicalist view of the lexicon do not tell us why derived nominals should be more resistant to insult—-and therefore presumably less complex—-than their source verb.

Whitaker bases the above remarks on the nature of lexical representation in generative semantic theory. He claims (ibid.:63) that
"in the transformationalist model . . . associated nominals destruction and protection [for example], are derived by transformational rule and thus are not listed in the lexicon." This is indeed the view expressed, for example, in Lakoff (1970:58).

However, generative semanticists could reply to Whitaker's arguments by saying that their theory of the lexicon was not intended to be a model of performance. They could further claim that in the model of performance derived nominals are somehow listed independently of their source verb.

However, note that such an approach based on the competence-performance catch leaves us without any explicit connection between the formal theory and the performance model and, in effect, makes the confirmation or disconfirmation of aspects of the formal theory impossible using performance data.

Both generative semantics and the lexicalist view of the lexicon do not specify how performance mechanisms are to make use of the formal aspects of the lexicon.

For example, under the lexicalist view verbs and their derived nominals would both be contained in a single lexical entry. Now, this theory does not make any claim about whether such entries always function as a unit—for example—with respect to brain damage (i.e., if such an entry is affected in brain damage is it always affected in its totality, including both the noun and verb subentries?)—which would be disconfirmed in advance at any rate given the data from noun facilitation involving the spared processing of derived nominals. Or what should we conclude regarding Chomsky's (1972:60) suggestion that "the
transformationalist hypothesis is correct for . . . gerundive nominals" in the light of the facilitation often seen for gerundive nominals in English-speaking persons who show a noun facilitation effect due to brain damage? In Chapters 2, 3, and 4 I will present some data which bear on our view of the role of the formal structure of the lexicon in a performance model and on the specific question of gerundive nominals.

To conclude, whatever noun facilitation phenomena have to say about the lexicalist-generative semanticist debate, there is a sense in which both theories do not provide a basis for an explanation of noun facilitation phenomena. That is, neither theory provides an explicit basis for less performance complexity for nouns than for verbs in general and derived nominals in particular—with generative semantics perhaps (subject to the qualifications above) even implying a greater complexity for derived nominals. In Chapter 2 I will have some suggestions about what this "less complex" aspect of nominals could be.

1.2.2.5. Marshall et al.

The work to be discussed in this section actually covers a span of more than 10 years and includes several papers by John Marshall, Jane Holmes, Freda Newcombe and Mary Marshall.

Marshall and Newcombe (1966) is a historically important discussion of syntactic and semantic errors in paralexia (for details see pp. 16-17 above). With regards to the nominalizing tendency often seen in noun facilitation cases as a result of brain damage (p. 20 above) they claim (ibid.:171-3) that responses such as "gentleman" for "gentle"
are the result of a "visual completion" strategy, with a noun bias. Of course, such a strategy cannot account for all cases of nominalization—such as the response "memory" to the stimulus "remember" in Whitaker's (1972) data above (p. 20). Still, many nominalizations can be described as "completions"—even among those in response to auditorily presented stimuli.

As noted above (p. 34), another factor here is that of a possibly restricted visual field giving rise to errors involving the end of words presented for reading aloud—as in the patient with a right hemianopia discussed in Warrington and Zangwill (1957) who could only read the beginning of words.

Regarding the general noun facilitation in their patient, Marshall and Newcombe (1966:175) cite Chomsky's (1965:115) arguments that grammars be written so as to select verbs and adjectives for insertion into generalized phrase-markers in terms of nouns and not conversely. Marshall and Newcombe (loc. cit.) merely suggest that this "noun priority in the theory of the lexicon" might somehow be involved in the causation of noun facilitation, without saying exactly what the connection could be. They do note (loc. cit.), however, that since they were trying to elicit single-word responses from their subject, it is difficult to see how their task would involve the selection of an item in terms of other items. They conclude that Chomsky's proposal might somehow be "reflected" in the production of single words. In this regard, Bloom's (1970) remarks noted above (p. 5) are relevant. She (ibid.:29-30) suggests that there is a connection between Chomsky's (1965) proposal regarding selectional features and the fact that
"children [add] to their lexicons . . . just those forms that [can] be selected to combine with the noun forms already acquired" (ibid.:29). I will return to this issue below.

Marshall et al. (1970) reconsider the data of Marshall and Newcombe (1966)—together with new data—and suggest that their visual completion hypothesis might be "subject to certain [linguistic] qualifications" (ibid.:420-1). Since they found that adjectival nominals were more difficult for their subject (the one studied in Marshall and Newcombe (1966)) than the corresponding root adjectives (see pp. 18-9) they suggest that such nominals—but not the verb-derived ones—might be transformationally derived. They speculate that verb-derived nominals are lexically related to their source verb (as in Chomsky (1972)). But, of course, the claim that verb-derived nominals and their source verb are lexically related—as opposed to being transformationally related—does not constitute an explanation of the facility of the derived nouns.

Marshall et al. (1975) present their most recent ideas on the noun facilitation effect revealed in their earlier work. According to their current thinking, the noun facilitation effect is not due to syntactic class per se; instead, it is related to the number of logical arguments lexical items can enter into syntactic constructions with. A higher number of arguments would increase the complexity of a lexical item under this view. Following Reichenbach (1947:80ff., 251ff.), they describe words as being characterized by different numbers of "place functions." Thus, "man(noun)" is a one-place function, as are "tall" and "sleep(verb)" since we can say the following.
(1-3) 
   a. X is a man.
   b. X is tall.
   c. X sleeps.

On the other hand, "friend," "taller" and "hit(verb)" are all two-place functions because we can say the following.

(1-4) 
   a. X is a friend of Y.
   b. X is taller than Y.
   c. X hits Y.

Continuing, "gift," "angry" and "put" are three-place functions because we can say the following.

(1-5) 
   a. X is a gift from Y to Z.
   b. X is angry at Y for Z.
   c. X puts Y on Z.

And so on. Although Marshall et al. (1970:320) note that "there are functions of one, two, and three variables in each grammatical category, noun, adjective and verb," they claim that nouns generally have fewer place functions than verbs.

As Reichenbach notes, the functions are 'unequally distributed' among the categories; nouns and adjectives predominate among the one-place functions; verbs predominate among the two- and three-place functions (loc. cit.).

Thus, Marshall et al. (loc. cit.) conclude by proposing that

The possibility thus arises that the notion of 'lexical complexity' is to be explicated in terms of the number of variables which are associated with particular vocabulary items.

I feel this proposal has some strong advantages. Unfortunately it also has some disadvantages. First, the advantages:

Marshall et al.'s (1975) proposal does many of the things I have criticized other theories for not doing.
It provides a link between noun facilitation and the problems with grammatical relations and constructions often exhibited by brain-damaged persons who show a noun facilitation effect. It is a grammatical explanation for what appears to be a grammatical phenomenon. Thus, an agrammatic who has trouble formulating syntactic constructions with two or more arguments would not be expected—under this view—to be able to use, either in isolation or in context, the multiple-argument (MA)—i.e., taking two or more arguments in syntactic constructions, as opposed to single-argument (SA)—words upon which such constructions turn. Recall that in agrammatic speech several arguments which could occur in a sentence are sometimes expressed—with pausal separations—without a verb to tie them together into a grammatical structure. Such conclusions also follow from the difficulty with verbs in sentential contexts found for normals (e.g., Moore 1972) and brain-damaged persons (e.g., Weigl and Bierwisch 1973, Marshall et al. 1970). That is, verbs are more difficult to insert into sentential contexts than nouns because, in general, more syntactic relations among items must be computed when verbs are inserted. "Apparently, the subject and object [NP's of a sentence] contain less information about the potential relations into which they can enter" (Moore 1972: 555). Fodor, Garrett and Bever (1968)—also cited in Marshall et al. (1975)—provide further support for the view that lexical complexity is the result of the number of variables which are associated with particular lexical items. They found that the greater the number of types of grammatical structures a verb permits—"e.g., a verb may be transitive or intransitive and may permit various types of complement
structures" (ibid.:453)--the greater the processing difficulty of sentences containing such verbs.

The agrammatic's telegraphic speech--as has been noted (p. 14)--parallels the "Mommy pigtail" type of utterance found in the speech of young children (cf. Brown and Bellugi 1970:83-4, Bloom 1970:139-41 and 164-5). Again, what is lacking in such utterances is an MA "connector." Marshall et al.'s (1975) proposal has the advantage of being a possible explanation for why nouns in general should predominate in the speech of young children, beyond the initial labelling considerations noted on p. 6 above. Thus, extrapolating from Marshall et al. (1975), we could hypothesize that children's acquisition of vocabulary items with varying numbers of arguments progresses in intimate relation with their progress in constructing/comprehending MA utterances. Berman (1978:22ff.) reports that Hebrew imperatives predominated among the verb forms in the language of a 25-month-old Hebrew-English speaking girl--despite a more complex morphology than other verb forms in Hebrew. This parallels the facility found with imperatives--compared to other verb forms (except "-ing" forms)--reported by Goodglass (1973:197) for English-speaking agrammatics, although as Berman (1978:36, n. 7) points out the CV form of the imperative form picked up by her subject could have been an influencing factor. For the same child, infinitives were the next verb forms to be acquired after the imperatives--again, despite a more complex morphology "than any Hebrew past-tense 'stem' forms" (ibid.:22) and despite a generally difficult phonological form as well. This parallels the findings with French and German agrammatics of a facility
with the infinitive form of verbs noted above (p. 15). The possibility which arises in connection with Marshall et al.'s (1975) proposal and the above facts is that these verb forms in Hebrew are acquired early because they occur in utterances minus one or more of the arguments which would occur in other syntactic constructions. A similar suggestion could be made regarding the facilitation of imperatives and infinitives in brain-damaged persons.

Marshall et al.'s proposal was put forth primarily to account for noun facilitation in tachistoscopic tasks.

It seems that a particular type of lexical organization—cross-classification of items for the structures they take—is necessary in order to provide the integration of stimulus and contextual information required in efficient 'predictive' sentence recognition. Noun facilitation in single-word tasks may result, then, from a form of organization which derives from the requirements of sentence processing (ibid.:320).

The effect of number of place functions on single-word recognition tasks is the most vulnerable part of Marshall et al.'s theory since they are proposing an effect due to the complexity of syntactic constructions outside of a syntactic context. If this aspect of their theory is confirmed we would then have a proposal finally capable of accounting for significant aspects of noun facilitation phenomena in language acquisition, language breakdown due to brain damage, and experimental task situations. However, their proposal has not been tested in a tachistoscopic design. In Chapter 2 I will present the results of such an experiment (along with other data) designed to test their hypothesis.
The disadvantages:

As stated, Marshall et al.'s theory predicts that items like "gift" will be much more difficult than items like "tree(noun)" in visual recognition tasks. Although this is an empirical question (which I will address in Chapter 2), this prediction seems unlikely to be true to me. On the other hand, my intuitions tell me very little about whether to expect verbs like "give" to be more difficult than verbs like, say, "live(verb)." In Chapter 2 I will have much more to say about such pairs.

A more serious problem--since it involves existing counterexamples--for Marshall et al.'s proposal stems from the nature of the nominalizations made by brain-damaged patients who show a noun facilitation effect. As Lesser (1978:134) points out, such nominalizations as those quoted from Whitaker (1972) on p. 20 above often leave the number of potential arguments involved unchanged. Once again, such nominalizations appear to be a crucial test for any explanation of noun facilitation since verbs, rather than being blocked altogether, frequently surface in noun facilitation patients in nominal form (Whitaker 1972:69). Thus, a theory which fails to account for nominalizations under such circumstances is an inadequate theory of noun facilitation. The above considerations also apply to the overuse of nominals in "-ing" by agrammatics, since these can occur with as many arguments as their associated verb forms. I will have more to say on this question in Chapter 2.

Marshall et al.'s (1975) proposal as stated above (p. 61) also runs into problems regarding the status of adjectives in noun facilitation
phenomena. If adjectives in general are one-place functions similar to "house(noun)" we would expect no difference between nouns and adjectives in tachistoscopic tasks or cases of brain damage. And yet, as we have seen (e.g., p. 4), such differences do exist. There are a number of possibilities here. To begin with, although Reichenbach (1947:251) does state that nouns and adjectives predominate among the one-place functions, he does not state that among adjectives one-place functions predominate. That is, while nouns may be predominantly one-place functions and verbs may be predominantly two- or more place functions, adjectives may be a mixed bag. And experiments like Holmes et al. (1971) may have combined one-place adjectives with two- or more place ones, thus causing their difficulty midway between nouns and verbs. On the other hand, one-place adjectives occur in the following constructions.

(1-7)  

a. X is tall.
b. tall X

Both (1-7a) and (1-7b) are uses of "tall" as a one-place function. One-place adjectives thus have at least two possible one-place constructions they can enter into. While (1-7a) and (1-7b) may be considered identical from the viewpoint of symbolic logic, they do represent two distinct grammatical possibilities. This, in turn, opens all kinds of possible lines of research regarding Marshall et al.'s (1975) proposal in connection with adjectives. For example: Is it the number of strictly logical relations or the number of syntactic structures which determines lexical complexity in performance? Still another possibility has to do with the issue of selectional features as discussed above (pp. 59-60). That is, even SA adjectives would under Chomsky's (1965) view have
selectional features not possessed by SA nouns. (MA Nouns would, of course, require selectional features just as verbs and adjectives do, unless we wanted to derive all MA nouns from underlying verbs—an unlikely possibility.)

These remarks bring me to the final "problem" I see with Marshall et al.'s (1975) proposal. (I place "problem" in quotes because in many ways the points I am about to raise are sources of interest rather than inadequacies.) It should be evident from even the brief remarks above that the concept of "place functions" which Marshall et al. briefly discuss is both incomplete in terms of definition and only the first step which can be taken towards characterizing the performative complexity of lexical items according to the fundamental idea of the "features that specify the number and type of syntactic and semantic relationships into which particular lexical items may enter" (ibid.:321).

For example, regarding the formal criteria according to which we can determine the number of place functions a particular lexical item has, we need to distinguish between the following two types of constructions.

(1-8)  
\begin{itemize}
  \item a. X is a gift from Y to Z.
  \item b. X is a man from Y (e.g., New York) in Z (e.g., a black suit).
\end{itemize}

There are several possible approaches we might take here. For one, we can consider the types of underlying sentence(s) examples like (1-8a) and (1-8b) would derive from (under one view of transformational grammar). Thus, while the three arguments in (1-8a) might derive from a single underlying sentence as in (1-9a), (1-8b) might derive from the set of sentences in (1-9b).
Such an approach would be related to one suggested to me by Greg Lee. Under this approach we could consider constructions such as those in (1-10) as a test for the number of arguments associated with a verb.

(1-10) a. *I put the books on the shelf, but Mike did so on the floor.

b. I put the books on the shelf with my hands, but Mike did so with a forklift.

Such a test, along with others suggested in Lakoff (1970), might help indicate which arguments are integral to particular verbs. This approach is related to that discussed above since the arguments determined by this test not to be integral to the main verb in the surface structure of sentences like (1-10b) could be viewed as being derived from higher simplex sentences in the sense of Lakoff (1970). Note that optionality of arguments at the level of surface structure does not always affect the outcome of this test. That is, while both of the sentences in (1-11) are grammatical,

(1-11) a. He is reading.
b. He is reading the book.

the sentence in (1-12)—which is analogous to (1-10a)—is still ungrammatical.

(1-12) *He is reading the book, and she is doing so the magazine.

However, a simpler approach might be to merely ignore possible arguments which fail to distinguish among lexical items which we are interested in looking at in psycholinguistic studies. For example, (1-8a) could take
the arguments of (1-8b) as in (1-13) below,

(1-13) X is a gift from Y to Z in a black box from Liberty House.

but (1-8b) could not take direct and indirect object arguments. To be sure, there are complex technical issues underlying the suggestions made above, and it may be the case that given the current state of linguistic knowledge it is impossible to provide a completely rigorous set of criteria for counting the number of place functions for lexical items. Fortunately, at the level at which I will be discussing Marshall et al.'s (1975) proposal in following chapters—that is, the level at which we need to begin to evaluate this proposal and its significance for linguistic theory—such a rigorous definition will not be necessary.

Psycho- and neurolinguistic considerations, on the other hand, will be primarily with the semantic and syntactic variables which can be shown to have some effect on complexity of processing. It may not be possible to demonstrate such an effect for all variables necessary from a formal point of view to characterize the constructions into which lexical items can enter. This is an empirical question and a matter of delimiting the theoretically interesting variables with respect to which lexical items may differ and then testing for a behavioral effect. The remarks on adjectives above offer such a possibility. Selectional features in the sense of Chomsky (1965:115) offer another. The kind of effects noted for semantic features by Marshall et al. (1970)—see p. 42 above—lend support to such a possibility, with the appropriate assumptions regarding marked and unmarked forms, etc. Such considerations regarding selectional features are also in line with the basic
idea—expressed in Marshall et al. (1975)—that the performative complexity of individual lexical items is a function of the constructions into which they can enter. Selectional features under this view would be a factor which would have to be controlled in tachistoscopic investigations of noun facilitation. To my knowledge, this has not been done previously, although the experiment to be discussed in Chapter 2 does control for this factor.

The optionality of arguments—i.e., Reichenbach (1947:84) notes that some lexical items (such as "writer") can function as either one- or two-place functions—offers another line of inquiry related to Marshall et al.'s proposal, to which I will return in Chapter 2.

The psycholinguistic literature also contains findings which suggest that in addition to counting the number of arguments a lexical item can occur in constructions with at any one time, we should also count the number of different types of arguments which can occur. For example, "meet" and "know" are both two-place verbs, as shown in (1-14) below.

(1-14)  a. I met him.
  b. I knew him.

However, as the direct object complement of "know" we might have a sentential complement, such as "I knew(that) he was coming." Fodor, Garrett and Bever (1968)—see p. 62 above—have shown that complex sentences containing words like "know" are significantly more difficult to process than the same sentences containing words like "meet."

In summary, then, Marshall et al.'s (1975) proposal concerning noun facilitation phenomena offers—despite some problems—both the
most hopeful basis for an eventual explanatory theory to unite all noun facilitation phenomena and a multitude of interesting new lines for future research.

1.3. Concluding Remarks

In line with the remarks made above, for the remainder of this dissertation I will be concerned largely with a test of Marshall et al.'s (1975) proposal and issues related to it. As noted throughout section 1.2., I will also address myself to some other factors which have been suggested as playing a role in noun facilitation. Most importantly, I will 1) try to suggest ways in which some of the above problems noted with respect to Marshall et al.'s (1975) hypothesis can be handled without abandoning the advantages gained with it; and 2) discuss the relevance of the modification to their proposal, which I will suggest, for linguistic theory in general.
CHAPTER 2.
TESTING THE EFFECT OF NUMBER OF ARGUMENTS
ON NOUN FACILITATION IN JAPANESE

2.0. Introduction

This chapter will be devoted to a discussion of aspects of Marshall et al.'s (1975) claim that the number of arguments a lexical item can enter into syntactic constructions with lies at the base of the noun facilitation phenomena seen in normal and brain-damaged persons. Section 2.1. will be concerned with a tachistoscopic test of their hypothesis, as well as an alternative hypothesis which I will propose, using Japanese stimuli and subjects. Sections 2.2. and 2.3. will each present the results of post-test questionnaires administered to the subjects who participated in the tachistoscopic experiment. The questionnaire to be discussed in section 2.2. deals with the subjective familiarity of the tachistoscopic stimuli. The questionnaire in section 2.3. deals with the age-at-acquisition of the stimuli. Section 2.4. will present the results of a reading aloud test--using the tachistoscopic stimuli--with a brain-damaged Japanese subject. In section 2.5. I will discuss the data from the preceding sections in terms of a confirmation or disconfirmation of Marshall et al.'s hypothesis and alternative theories of noun facilitation.

2.1. A Tachistoscopic Investigation of the Role of Number of Arguments in Noun Facilitation Using Japanese Stimuli

2.1.1. Rationale and Experimental Hypotheses

The desirability of testing Marshall et al.'s (1975) claim regarding the role of number of arguments in noun facilitation has been indicated
in the previous chapter. The methodology for an effective preliminary
test of their hypothesis is straightforward: First, construct four
lists of SA nouns and verbs and MA nouns and verbs balanced for
orthographic length, frequency and other relevant factors. Then present
these words as stimuli in a tachistoscopic design along the lines of
Holmes et al. (1971), or as stimuli in a reading aloud task for brain­
damaged subjects along the lines of Marshall and Newcombe (1966) or
Holmes et al. (1971).

However, as we will see in Chapter 3 (which deals with just such
a tachistoscopic design in English), such stimulus lists are quite
difficult to construct in English. This may be part of the reason
that there has been no test of Marshall et al.'s 1975 hypothesis until
now.

In the summer of 1978 I had arrived at a hypothesis regarding
noun facilitation (to be discussed below) in some aspects similar to
Marshall et al.'s and based on independent work on a neuropsychological
model of linguistic competence. At that time I was affiliated with the
Communication Research Section of the Tokyo Metropolitan Institute of
Gerontology and had access to the apparatus necessary for a tachisto­
scopic study and to a pool of both normal and brain-damaged subjects.
When I became aware of Marshall et al.'s (1975) paper and also learned
from Dr. Marshall that no follow-up test had been done on their
hypothesis, I was struck by the relative advantages of using Japanese
stimuli (and, of course, Japanese subjects) as the basis for such a
test. (These advantages will be discussed in section 2.1.2.3.--
"Materials"--below.)
Accordingly, for my first experiment related to Marshall et al.'s hypothesis I designed the tachistoscopic study to be described below using Japanese stimuli.

Again, the stimuli were Japanese nouns and verbs divided into four lists: SA nouns and verbs and MA nouns and verbs. The experimental procedure was an ascending method of limits tachistoscopic design along the lines of that reported in Holmes et al. (1971) (see pp. 3-4 in Chapter 1), and will be described in section 2.1.2.4. below.

Marshall et al.'s (1975) hypothesis served as the basic experimental hypothesis ($H_e$) for this study, although—as we will see—the study was also intended as a test of other hypotheses as well. Recall that their hypothesis states that syntactic features operate in the visual recognition of single words (and, thus, in their retrieval from LTM), although they do not specify exactly how this comes about. Thus, retrieving single words from LTM might be like snagging a single strand in a bowl of spaghetti: Pulling a word from LTM entails dragging along a certain amount of the information, syntactic features, etc. which are "stuck" to it. One way of characterizing this operation would be to view the recognition of single words as involving the construction or recall of a syntactic frame into which they can enter. In the current state of our knowledge regarding these matters, however, such statements are fairly metaphorical.

Marshall et al.'s (1975) hypothesis consists of the following two sub-claims regarding the two factors of syntactic class and number of arguments.
Noun facilitation is not due to syntactic class per se.

The number of arguments a word can enter into syntactic constructions with affects tachistoscopic recognition thresholds (among other types of performance associated with noun facilitation phenomena, such as noun facilitation in dyslexia, although Marshall et al. (1975) are not explicit on the range of phenomena to which their hypothesis is meant to apply).

Marshall et al. claim that noun facilitation results from the fact that it just so happens that nouns tend to have fewer arguments than verbs.

Accordingly, a comparison of the recognition thresholds for the four lists of stimuli mentioned above—i.e., SA and MA nouns and verbs—with each other allowed a test of $H_e$ according to the following predictions. Under its strongest form, $H_e$ predicted that there would be no difference between the thresholds of

(1) SA nouns and SA verbs;
(2) MA nouns and MA verbs; or between
(3) (SA + MA nouns) and (SA + MA verbs).

The strongest version of $H_e$ did predict a significant difference between

(4) SA nouns and MA nouns;
(5) SA nouns and MA verbs;
(6) SA verbs and MA nouns;
(7) SA verbs and MA verbs; and between
(8) (SA nouns + verbs) and (MA nouns + verbs),

with the first part of each comparison above predicted to receive the lower (faster) thresholds (implying greater processing ease). Of course, these predictions assumed the control of factors with a possible effect on recognition thresholds—such as orthographic length, frequency, etc.—across the four categories of stimulus items.
The null hypothesis (H₀) for this study predicted a lack of a significant difference for any of the eight comparisons just noted.

On the other hand, one could imagine an "anti-Hₐ"--also testable by the experiment to be described below--such that the above (p. 75) two parts of Marshall et al.'s (1975) hypothesis are reversed as indicated below.

(2-3) Noun facilitation is due to syntactic class per se (for reasons as yet unidentified).

(2-4) Number of arguments does not affect tachistoscopic recognition thresholds (or the other types of performance associated with noun facilitation phenomena).

Such an "anti-Hₑ" would make predictions as follows with regards to the eight comparisons listed above. No difference between

(4) SA nouns and MA nouns;
(7) SA verbs and MA verbs; or between
(8) (SA nouns + verbs) and (MA nouns + verbs).

The "anti-Hₑ" would predict a significant difference between

(1) SA nouns and SA verbs;
(2) MA nouns and MA verbs;
(3) (SA + MA nouns) and (SA + MA verbs);
(5) SA nouns and MA verbs; and between
(6) MA nouns and SA verbs,

with the first part of each comparison above predicted to receive the lower (faster) thresholds. Note that both Hₑ and "anti-Hₑ" make the same prediction for comparison 5, and both predict a significant difference for comparison 6--but in opposite directions.
Of course, the above predictions made by $H_e$, $H_o$ and "anti-$H_e$ do not exhaust the possible outcomes of an experiment comparing four classes of stimuli defined by two factors (in this case, syntactic class and number of arguments). For example, there may be an interaction between the factors of syntactic class and number of arguments. Such an interaction was posited by my hypothesis regarding noun facilitation which I had formulated before reading Marshall et al.'s (1975) paper. There is, in fact, some reason to expect an interaction between syntactic class and number of arguments, as I will show below.

As noted in section 1.2.2.5 in Chapter 1, when we actually attempt to distinguish among words according to the number of arguments they can take we are faced with certain problems with respect to what to count as an argument and what not to count. For example, it was noted (p. 70) that some lexical items (such as "writer") can function as either one- or two-place functions. Relatedly, consider one example of an MA noun given by Marshall et al. (1975:320): "gift." If my intuition is representative of standard-English speakers as a whole, then common sense seems to suggest that we do not often use this word with all of the three arguments Marshall et al. assign to it on the basis of phrases such as (2-5).

(2-5) John's gift of candy to Mary

Going beyond this observation, it is important to note what seems to be a general characteristic of MA nouns in English. Namely, such nouns can always be used in a single-argument fashion—at least at the level of surface structure—as in (2-6).

(2-6) Books make nice gifts.
The only possible exception to this observation that I know of might be nouns such as "crux" and "gist" which seem to be associated with certain frozen expressions (i.e., multiple argument frames).

The above observation leads to an interesting problem for any test of Marshall et al.'s hypothesis since many MA verbs do not show such an optionality of arguments. This is the case with "put," as the following examples show.

(2-7)  
a. *I put.
b. *I put it.
c. *I put there.
d. I put it there.

The remarks made above for MA nouns and verbs in English with respect to optionality of arguments are generally applicable to MA nouns and verbs in Japanese, for which I will provide examples in section 2.1.2.3.--"Materials"--below.

Here, with regards to the experiment to be discussed below it should be asked where—in the light of the optionality of their arguments—MA nouns will fall in terms of recognition difficulty relative to SA nouns and verbs and MA verbs. Marshall et al.'s hypothesis predicts that these differences between MA nouns and verbs will not affect tachistoscopic recognition. Marshall et al. (1975:321) --in agreement with the earlier work of Marshall and Newcombe (1966) --assume that the process of recognizing and reading a word "involves the retrieval of the full lexical entry associated with the particular visual stimulus." Presumably, this "full retrieval" hypothesis extends to the argument structures of MA nouns as well as verbs. Clearly, this is not the only possibility we can imagine, especially in the light of the differences between MA nouns and verbs which I have been
discussing.

Craik and Lockhart's (1972) influential paper on levels of processing in memory is concerned with the relation between the persistence of a memory trace and "depth of analysis" or "degree of stimulus elaboration" (ibid.:677). In this connection, they summarize the current widely held view that "perception involves the rapid analysis of stimuli at a number of levels or stages" (ibid.:675):

This conception of a series or hierarchy of processing stages is often referred to as "depth of processing" where greater depth implies a greater degree of semantic or cognitive analysis. After the stimulus has been recognized, it may undergo further processing by enrichment or elaboration. For example, after a word is recognized, it may trigger associations, images or stories on the basis of the subject's past experience with the word (loc. cit.). (Emphases added.)

The instances of "may" in the above quote lead to a whole chain of possibilities regarding the types of effects which might be observed for the experiment to be discussed below.

Namely, instead of a "full retrieval" of the syntactic features attached to lexical items in all performance contexts, perhaps a greater or lesser number of such features is retrieved according to the nature of the task situation. For example, a least effort principle might suggest that only those lexical features necessary for an adequate response in a given performance context will be retrieved from LTM. Such a least effort principle is fully consistent with the remarks quoted from Craik and Lockhart above.

Proceeding along the above lines with respect to a tachistoscopic test of Marshall et al.'s hypothesis, we should ask whether the task demands of single-word tachistoscopic recognition (or the reading aloud
of single words by brain-damaged patients) really necessitate the kind of "full retrieval" of the syntactic features of lexical items which Marshall et al. posit. It seems reasonable to assume that the retrieval of single, isolated words from LTM involves a minimum of syntactic computation and/or retrieval. The question with respect to a test of Marshall et al.'s hypothesis then becomes what is the minimum syntactic frame or number of arguments which must be recalled from LTM with a given type of lexical item in order for single-word recognition to take place?

Here, the distinction between optional and obligatory arguments discussed above suggests itself as a likely factor in the determination of an answer to the foregoing question. That is, perhaps optional argument features need not be recalled from LTM in the recognition of single words where a minimum of syntactic computation is involved. This brings me back to the possible interaction between syntactic class and number of arguments (p. 77 above)—overlooked by H, H₀ and "anti-H."—as factors influencing the results of the experiment to be discussed.

Now, the argument features for MA nouns in English (and in Japanese, as we will see in 2.1.2.3.) are optional in a way that the argument features for MA verbs frequently are not. The strongest prediction we could make regarding the interaction of syntactic class and number of arguments in the light of the optionality of the latter in the case of MA nouns would be to claim that only the features relating to obligatory arguments need be recalled from LTM in tasks—such as tachistoscopic recognition—involving a minimum of syntactic
computation. Thus, in the experiment to be discussed below one might expect SA nouns and verbs and MA nouns to all be equal in recognition difficulty and significantly easier than MA verbs, because MA nouns can always be used in grammatical single argument constructions. The foregoing suggestion is, of course, subject to the provision that the MA verbs involved are like "put" in that they have more than one obligatory argument. (The experiment with English stimuli to be discussed in Chapter 3 will deal in part with the question of verbs that can function as either SA or MA items.) To rephrase, the above strong versions of a hypothetical interaction based on optionality of arguments states that number of arguments increases visual recognition difficulty only for verbs, since such arguments can only be obligatory for verbs and are always optional for nouns.

To contrast with Marshall et al.'s "full recall" hypothesis, I will term my hypothesis outlined above the "minimum recall" hypothesis. These two hypotheses are stated below for later reference.

**Full recall hypothesis:** The full lexical entry associated with a given lexical item is recalled from LTM even in processing tasks requiring a minimum of syntactic computation—such as single-word tachistoscopic recognition (see Marshall et al. 1975:321).

**Minimum recall hypothesis:** In processing tasks involving a minimum of syntactic computation—such as single-word tachistoscopic recognition—a given lexical item will be recalled from LTM with only those argument features necessary for its use in a grammatical sentence.

The word "recall" implies that the two foregoing hypotheses are applicable to experimental situations with normal and brain-damaged subjects which involve retrieval from LTM. The corollaries to these
hypotheses in the case of language acquisition—where the term "recall" is not appropriate—will also be discussed in section 2.5. below.

My minimum recall hypothesis does not constitute a complete contradiction of Marshall et al.'s hypothesis in the sense that the factor of number of arguments is still taken to be the critical factor in predicting processing difficulty. The difference between our hypotheses comes about when allowance is made for the optionality of the arguments associated with MA nouns.

Note that there is already some data which supports my minimum recall hypothesis over Marshall et al.'s full recall hypothesis. In Chapter 1 (p. 65) it was pointed out that the most unsatisfactory aspect of Marshall et al.'s theory is that it fails to account for many of the nominalizations made by brain-damaged patients (who show a noun facilitation effect) when attempting to produce verb or adjective targets. Such nominalizations (see p. 65 in Chapter 1) frequently leave the number of potential arguments unchanged, as in "concealment" substituted for "conceal." My alternative to Marshall et al.'s theory can account for the greater ease of such nominalizations for patients who show a noun facilitation effect in a natural way: Such nominalizations have the effect of changing obligatory arguments to optional ones (along the lines of the above discussion) which do not have to be recalled from LTM, in tasks involving a minimum of syntactic computation, under my hypothesis. The minimum recall hypothesis would thus constitute an improvement over Marshall et al.'s theory—subject to the results of the tachistoscopic experiment to be discussed below. At the same time, my alternative to Marshall et al.'s theory does not do
away with any of the advantages of their theory.

There are also other predictions along the lines of a possible interaction between syntactic class and number of arguments which could be imagined. Such predictions might result from consideration of other factors besides those discussed thus far. These possibilities would need to be considered if both Marshall et al.'s hypothesis and my minimum recall hypothesis were to prove incapable of explaining all the noun facilitation data.

Finally, the tachistoscopic experiment and the other data to be discussed below are also relevant tests of many of the other factors of possible relevance to explanations of noun facilitation discussed in Chapter 1, such as various aspects of frequency, morphological complexity, etc. These factors will be mentioned, where relevant, in sections 2.1.2.3. ("Materials") and 2.5. ("General Discussion") below.

2.1.2. Method
2.1.2.1. Subjects

The subjects for this experiment were 20 normal Japanese adults working in the administration of the Tokyo Metropolitan Institute of Gerontology. There were ten men and ten women, all considering themselves right handed. All had normal or corrected (6/6) vision. They ranged in age from 22 to 39 years (mean: 28.3). The education level for the subjects ranged from 14 to 23 years (mean: 16.2).

All of the subjects participated in the experiment during working hours at the Institute of Gerontology—but were otherwise unrewarded—and can be described as well motivated.
2.1.2.2. Apparatus

A three-channel Takei 226DP-6 tachistoscope, belonging to the Institute of Gerontology, was used in the experiment. Exposure duration could be adjusted in increments of 1 msec starting from 1 msec. The exposure fields measured 20 x 20 cm and subtended a horizontal visual angle of 14° at the retina (i.e., 7° on either side of the fixation point). The luminance was 3.7 cd/m² (± 0.4 cd/m²).

The tachistoscope was equipped with controls which permitted the programming which will be described in section 2.1.2.4. ("Procedure") below.

2.1.2.3. Materials

As stated, the stimuli for this experiment consisted of four lists of Japanese words: SA nouns, SA verbs, MA nouns and MA verbs. Each list had 15 items, bringing the total number of stimuli to 60.

The SA nouns and verbs were derivationally related pairs, as were the MA nouns and verbs. Examples are given below in (2-8), along with English glosses. (A complete list of the Japanese stimuli used in this experiment can be found in Appendix 1.)

(2-8) SA pairs

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>SA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>koori &quot;ice&quot;</td>
<td>kooru &quot;freeze&quot; (intransitive)</td>
</tr>
<tr>
<td>hikari &quot;light&quot;</td>
<td>hikaru &quot;shine&quot; (intransitive)</td>
</tr>
<tr>
<td>kumori &quot;cloudiness&quot;</td>
<td>kumoru &quot;get cloudy&quot;</td>
</tr>
</tbody>
</table>

MA pairs

<table>
<thead>
<tr>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>yurushi &quot;permission&quot;</td>
<td>yurusu &quot;permit&quot;</td>
</tr>
<tr>
<td>urami &quot;resentment&quot;</td>
<td>uramu &quot;resent&quot;</td>
</tr>
<tr>
<td>miokuri &quot;seeing off&quot;</td>
<td>miokuru &quot;see off&quot;</td>
</tr>
</tbody>
</table>
The morphological relationship between the members of noun-verb pairs such as those in (2-8), ending in /i/ and /u/, respectively, is an extremely common one in Japanese (see, for example, Alfonso 1974: 474ff.). Note that the morphological structure is exactly the same for both members of such pairs—i.e., stem plus suffix. The only difference between the noun and verb of such pairs (or across pairs, for that matter) in terms of morphology is whether the suffix is an /i/ or /u/, which determines whether an item is a noun or a verb. This eliminates the possibility of other morphological factors (not related to the noun-verb distinction) influencing the threshold results.

These stimuli allowed a test of Marshall et al.'s hypothesis and my minimum recall hypothesis since they made it possible to see the effect of syntactic class on visual recognition time with number of arguments controlled (e.g., by comparing items within derivationally related pairs), and vice versa (e.g., by comparing across pairs, such as SA nouns with MA nouns).

Using derivationally related noun-verb pairs also made possible a check on many of the other factors mentioned in section 1.2. of Chapter 1—such as phonological complexity, concreteness, etc.—since these factors do not differ within the derivationally related noun-verb pairs. These aspects of the present experiment will also be discussed in section 2.5. ("General Discussion").

The Japanese MA stimuli share the characteristics of MA nouns and verbs in English discussed in section 2.1.1. above. For example, (2-9a) below with the MA verb "uramu" ("resent") is parallel to the
phrase in (2-9b) using the MA noun "urami" ("resentment").

(2-9)  a. \{Mary no kyozetsu o John wa
\{Mary's refusal (object particle) John (subject
\{ particle)
\{ urande iru.
\{ resents.
"John resents Mary's refusal."

b. \{Mary no kyozetsu ni taisuru John no urami
\{Mary's refusal concerning John's resentment
"John's resentment of Mary's refusal"

It is probably true that Japanese does not permit the wide range of
multiple argument constructions with MA nouns permitted in English with
its rich system of prepositions. However, enough examples can be cited
for MA nouns in Japanese to ensure that multiple argument constructions
incorporating them are a clear possibility.

The Japanese MA noun stimuli can also be used in single argument
constructions parallel to (2-6) above (p. 77). This is the case with
"urami" as (2-10) shows.

(2-10) Urami wa hito no tsune da.
"Resentment is (part of) human nature."

A problem may arise here because of the wide range of ellipsis
permitted with Japanese verbs in conversational contexts. That is,
all Japanese MA verbs can be used as in (2-11b) in answer to (2-11a).

(2-11)  a. Anata wa Mary no kyozetsu o urande imasuka?
"Do you resent Mary's refusal?"
(2-11)  b. Ee, urande imasu.

"Yes, (I) resent (it)."

In cases like (2-11b), Japanese MA verbs appear to share the characteristics noted above for MA nouns in example (2-10). However, these cases of "zero pronominalization" (Kuroda 1965:104-34) such as (2-11b) depend on the existence of shared information between two speakers for their grammaticality and interpretability, as noted in Kuroda (1965:114-5) and Hinds (in press:15ff.). Thus, it can be claimed that MA verbs used as in (2-11b) still have obligatory arguments which must be present in underlying structure even if realized as zero in surface structure. The experiment to be discussed below is also a relevant test of this claim. (See also Hinds (in press:15ff.) for a discussion relevant to the foregoing claim.)

For the experiment, the stimuli were printed on the tachistoscopic cards, using rub-on letters approximately half an inch high, in the katakana syllabary. Katakana are used in Japanese orthography where one might normally use italics in English: for foreign words, emphasis (as in signs, advertisements, etc.), onomatopoeic words, and so on. The noun and verb of each stimulus pair had the same number of orthographic symbols when written in this syllabary. In addition, orthographic length was matched across the SA and MA noun-verb pairs, resulting in matched orthographic length across the four categories of stimulus items (as for the items in (2-8)).

The stimuli were presented in the katakana syllabary for very definite reasons. For one thing, it would have been very difficult to match kanji (Sino-Japanese ideographs) across SA and MA pairs in
terms of visual complexity, etc. In addition, Sasanuma and Fujimura
(1971:10-11) note that "kanji and hiragana [the non-italic syllabary
of Japanese] transcriptions of the same words do not have the same
frequencies of use." With respect to this observation, it was necessary
to consider for the sake of my experiment whether the derivationally
related nouns and verbs differed in terms of the frequency with which
they appear in the different Japanese orthographies. That is, the
tachistoscopic thresholds might have been affected if, for example,
the stimuli were presented in kanji plus hiragana (for the suffix) and
if this particular type of transcription were more associated on the
subjects' part with verbs. In fact, I conducted an informal survey
of Japanese adults which confirmed just this possibility. I asked
these adults to tell me whether they would expect a word printed in
kanji plus hiragana to be a noun or a verb. The responses indicated
that this type of transcription is paradigmatic for verbs. Importantly,
however, the adults surveyed indicated no clear expectations with
regards to whether a word printed entirely in hiragana would more likely
be a noun or a verb. This was confirmed by comparing the frequency
with which the derivationally related noun and verb stimuli appeared
in either kanji plus hiragana or just hiragana according to the
Japanese National Language Research Institute's (1964) word count of
published material. That is, this word count gives separate
frequencies for words as they appear in kanji and in hiragana, and a
comparison of such frequencies for the stimuli showed no clear tendency
for the noun stimuli to be more frequently represented in hiragana than
the verb stimuli.
From the above it was concluded that either hiragana or katakana could be used for the lettering on the tachistoscopic cards without prejudicing the results of the experiment in either the direction of nouns or verbs. The hiragana and katakana syllabaries are representationally equivalent for the stimuli used in this experiment, much as an italic script and plain lettering are equivalent in English. I chose katakana, rather than hiragana, for use in the experiment as a further check on any possible expectations with regards to syntactic class and a particular orthography on the part of the subjects. That is, katakana are used in Japanese primarily for onomatopoeic words, foreign words, advertising signs, etc., and as such the subjects should not have expected any connection between the syntactic categories represented in my experiment and katakana. This conclusion was also confirmed in my informal survey of Japanese adults. That is, when asked whether they expected Japanese words written entirely in katakana to be nouns or verbs, the adults questioned usually indicated "no idea" or "onomatopoeic words."

The four categories of stimuli--SA nouns and verbs and MA nouns and verbs--were also matched for absolute (i.e., irrespective of type of orthography) frequency in print using the word count of the Japanese National Language Research Institute.

Each stimulus word was printed horizontally on a white tachistoscopic card. The stimuli were printed in the center of the cards, in accordance with the requirements of the experimental procedure to be described below.
2.1.2.4. Procedure

For purposes of comparison, an attempt was made to follow the experimental procedure of other tachistoscopic work on noun facilitation—especially Holmes et al. (1971), the novelty of the present design consisting of the type of stimuli used. The basic outline of the ascending method of limits experimental procedure was as follows.

The stimuli were presented one at a time in the center of the tachistoscopic viewing field. Each stimulus was first presented at an exposure duration—say 5 msec—below the recognition capacity of the subject. (This "baseline" was determined separately for each subject in practice sessions before the experiment.) At successive trials with the same stimulus, the exposure duration was increased by 1 msec increments. On each trial the subject's task was simply to say to the experimenter the word he/she thought he/she saw. When a subject said the correct word the exposure duration for the next trial was held constant. If the subject then responded with the same word on the next trial, that exposure duration—in msec—was taken as the recognition threshold; noted; and the subject was presented a new stimulus word starting again from the baseline exposure duration. If the subject responded with a different word after once saying the correct word, then the exposure duration was again increased by 1 msec for another trial. The criterion for a correct performance on a given stimulus item was thus two correct reports of the word seen in a row.

The specifics of the above procedure were as described below.
The program for successive trials with a stimulus item was as follows. At each trial a subject was first asked to fixate his/her eyes on a horizontal black bar (slightly larger than the longest word among the stimuli) in the center of the visual field. This bar-type fixation marker was chosen rather than an "X," etc. because in pretesting it was determined to have less of a masking effect on selective parts of the printed stimuli. That is, with this type of fixation any masking effect was spread equally over all of the orthographic symbols in each stimulus item.

The fixation marker described above always stayed on for exactly two seconds. The stimulus item then followed immediately--i.e., with no intervening blank card--for the appropriate duration. The stimulus item was then followed immediately by a blank white card, which always stayed on for exactly four seconds after each stimulus presentation. During these four seconds the subject made his/her response (if any) and prepared for the next presentation of the fixation marker which followed immediately at the expiration of the exposure of the blank white card. This program is summarized in Fig. 1.

For the first presentation of the fixation marker for a series of trials with a given stimulus item, the experimenter gave a "ready" signal to the subject. Subjects had a brief break between stimulus items while a new item was readied, and each subject had a one minute break after half of the stimuli had been completed.

The way the visual recognition of a stimulus item actually came about under the conditions described above was as follows.
A = two second presentation of fixation bar
B = four second presentation of blank white card
X = presentation of stimulus item at baseline exposure duration

Fig. 1. Program for the presentation of a stimulus item.

First of all, the exposure duration (<30 msec) for one presentation of a stimulus item was not enough time for the entire recognition process to take place—i.e., for the visual percept to travel to the brain via sensory receptors, neural pathways, etc. and be interpreted. Still less was such a short exposure duration sufficient time for a subject to make a verbal response. All that was accomplished, therefore, during the extremely short time a stimulus item was exposed to the retinas of a subject was the formation of a sensory image.

Therefore, the sensory image—if any had formed—from a given exposure travelled to the brain and was interpreted by the subject (prior to his/her response) after the actual physical stimulus had disappeared from the visual field. Since my subjects were placed under no time pressure with regards to their actual response—and were even allowed to change their minds about a response—the procedure in no sense measured "response" or "reaction" time.
In addition, the actual threshold for any given word for a subject was as much an index of the number of trials needed for recognition as a measure of the absolute length of time required to form a recognizable sensory image as outlined above. (Recall that successive exposure durations were increased by 1 msec until a subject made a correct response.) Thus, on the first exposure of a given word a subject might have recognized its initial letter. On the second exposure the subject might have recognized the last letter or several middle letters, and on the third exposure he/she might have got the entire word by guessing at any letters still unrecognized—based on his/her knowledge of the language. The fourth exposure would then have served to confirm or disconfirm this guess.

All of the 60 stimuli were completely randomized separately for each subject to control for learning and/or fatigue effects.

Each subject was explained the program described above, with the omission of any explanation regarding the increments in exposure duration from trial to trial. They were told that their task was to report the word being shown to them with as few trials as possible and were encouraged to guess. The subjects were told that the criterion for a complete performance with a stimulus item was two correct reports in a row, as in Holmes et al. (1971) (Jane Holmes, personal communication). Subjects were told that they would be shown only real Japanese words and were given ten practice trials with nouns and verbs of the type used in the experiment before beginning with the actual stimuli.

The experiment was conducted in Japanese by Mr. Yō Kobayashi at the Tokyo Metropolitan Institute of Gerontology.
2.1.3 Results

The mean recognition threshold for a single stimulus item in each of the four categories of stimuli for each subject were used in the statistical analysis. The mean recognition time (in msec) for a single item in the total number of 15 stimuli in each category and across all the replications by the 20 subjects can be seen in Table 1.

Table 1

Single-Item Threshold Means for the Japanese Tachistoscopic Experiment

<table>
<thead>
<tr>
<th></th>
<th>SA nouns</th>
<th>SA verbs</th>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>8.52</td>
<td>8.48</td>
<td>9.05</td>
<td>9.33</td>
</tr>
</tbody>
</table>

A two-factor (syntactic class and number of arguments), completely repeated-measures analysis of variance (anova) design was used as the basis of the calculation of the eight comparisons indicated in 2.1.1. (p. 75). The anova was performed with the P2V program from the Biomedical Computer Programs, P-Series (1977), edited by Dixon and Brown. The resulting anova table is shown in Table 2. Table 2 shows three main effects: syntactic class, subjects and number of arguments. As a main effect, number of arguments was significant at the level of p<.001; the effect of syntactic class reached the level of p<.08. No significance level was computed for the main effect of subjects (or for the various interactions with the subject factor), since in the repeated measures statistical design used to analyze the threshold results there is no appropriate error term for the subject factor. Table 2 does show that the interaction for syntactic class by number
Table 2
Anova Results for the Japanese Tachistoscopic Experiment

<table>
<thead>
<tr>
<th>source</th>
<th>sums of squares</th>
<th>df</th>
<th>mean squares</th>
<th>F</th>
<th>one-tail probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>syntactic class</td>
<td>0.31626</td>
<td>1</td>
<td>0.31626</td>
<td>3.63</td>
<td>p&lt;.08</td>
</tr>
<tr>
<td>subjects</td>
<td>1064.15649</td>
<td>19</td>
<td>56.00822</td>
<td></td>
<td></td>
</tr>
<tr>
<td>syntactic class x subjects</td>
<td>1.65326</td>
<td>19</td>
<td>0.08701</td>
<td></td>
<td></td>
</tr>
<tr>
<td>no. of arguments</td>
<td>9.39134</td>
<td>1</td>
<td>9.39134</td>
<td>61.31</td>
<td>p&lt;.001</td>
</tr>
<tr>
<td>no. of arguments x subjects</td>
<td>2.91017</td>
<td>19</td>
<td>0.15317</td>
<td></td>
<td></td>
</tr>
<tr>
<td>syntactic class x no. of arguments</td>
<td>0.48516</td>
<td>1</td>
<td>0.48516</td>
<td>6.43</td>
<td>p&lt;.02</td>
</tr>
<tr>
<td>syntactic class x no. of arguments x subjects</td>
<td>1.43347</td>
<td>19</td>
<td>0.07545</td>
<td></td>
<td></td>
</tr>
<tr>
<td>totals</td>
<td>1080.34615</td>
<td>79</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
of arguments was significant at the level of \( p < .02 \).

The above results—plus an inspection of the direction of the differences in the mean thresholds in Table 1—indicate that MA items (regardless of syntactic class) had much higher (i.e., slower) recognition thresholds than SA items. The significant interaction between syntactic class and number of arguments, on the other hand, indicates that whether a word was a noun or a verb made a difference in the value of the recognition thresholds only in the case of MA items. In other words, nouns were recognized at lower (i.e., faster) thresholds than verbs, but only in the case of MA items. There was no difference in terms of threshold values between nouns and verbs among SA items.

The above conclusions are clearer in the subsequent comparisons—the eight listed in 2.1.1. (p. 75)—based on the anova, and to be discussed next.

With subsequent tests such as those to be reported, the more comparisons made the greater the likelihood of type I errors. To deal with this possibility, the Dunn test (Dunn 1961, Keppel 1973:147–8) was used for this study, since 1) the number of comparisons to be made was a small subset of all the possible comparisons that could have been based on the present data; and 2) the specific comparisons to be made were determined by the logic of the experiment and could reasonably be considered \textit{a priori} comparisons. In the Dunn test, one simply tests the values of \( t \) obtained in the subsequent tests at a more stringent alpha level. That is, the acceptable alpha level for statistical significance is determined by dividing the alpha level for the anova
(usually .05) by the number of comparisons to be made. In this case, since the total number of comparisons to be made was eight, the adjusted alpha level for statistical significance for each comparison was .0063. That is, if the value of p for a given comparison was higher than .0063, any difference found in that comparison was concluded not to be statistically significant.

In Table 3 are listed the results of the subsequent t-tests performed—using the error terms from the anova—to determine the significance of the difference in thresholds found for the eight comparisons listed on p. 75 above. The symbol "<" refers to threshold values.

These results (Table 3) for the eight comparisons can be paraphrased as follows.

Comparison 1: The thresholds for the SA nouns were not significantly different from those for the SA verbs.

Comparison 2: The MA nouns, on the other hand, had significantly lower (faster) thresholds than did the MA verbs.

Comparison 3: On the whole, the thresholds for all nouns (SA and MA combined) were not significantly different from those for all verbs.

Comparison 4: The SA nouns had significantly lower thresholds than the MA nouns.

Comparison 5: The SA nouns had significantly lower thresholds than the MA verbs.

Comparison 6: The SA verbs had significantly lower thresholds than the MA nouns.

Comparison 7: The SA verbs had significantly lower thresholds
Table 3

Eight Comparisons of Thresholds Based on the Four Categories of Stimuli Used in the Japanese Experiment

<table>
<thead>
<tr>
<th>comparison</th>
<th>t value</th>
<th>df</th>
<th>α = .0063</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) SA nouns vs. SA verbs</td>
<td>0.3453</td>
<td>19</td>
<td>***NS</td>
</tr>
<tr>
<td>(2) MA nouns &lt; MA verbs</td>
<td>3.2408</td>
<td>19</td>
<td>*Sig.</td>
</tr>
<tr>
<td>(3) all nouns vs. all verbs</td>
<td>1.9065</td>
<td>19</td>
<td>**NS ¹</td>
</tr>
<tr>
<td>(4) SA nouns &lt; MA nouns</td>
<td>6.0958</td>
<td>19</td>
<td>**Sig.</td>
</tr>
<tr>
<td>(5) SA nouns &lt; MA verbs</td>
<td>9.3366</td>
<td>19</td>
<td>***Sig.</td>
</tr>
<tr>
<td>(6) SA verbs &lt; MA nouns</td>
<td>6.4413</td>
<td>19</td>
<td>**Sig.</td>
</tr>
<tr>
<td>(7) SA verbs &lt; MA verbs</td>
<td>9.682</td>
<td>19</td>
<td>***Sig.</td>
</tr>
<tr>
<td>(8) all SA items &lt; all MA items</td>
<td>7.8302</td>
<td>19</td>
<td>**Sig. ¹</td>
</tr>
</tbody>
</table>

¹ Note that the minimum recall hypothesis does not necessarily make any claim about a significant difference for comparisons 3 and 8, since it predicts that the three categories of SA nouns and verbs and MA nouns should all have equal thresholds. However, the minimum recall hypothesis would predict that if a difference obtained in either of these comparisons in terms of thresholds it would be in the direction of all nouns having the lower thresholds in comparison 3 and all SA items having the lower thresholds in comparison 8.

* supports the minimum recall hypothesis
** supports the full recall hypothesis
*** agrees with both the minimum and the full recall hypotheses
than the MA verbs.

Comparison 8: On the whole, all SA items (nouns and verbs combined) had significantly lower thresholds than all MA items.

It is evident that the above results concerning the eight comparisons set out in 2.1.1. (p. 75) as a basis for a test of Marshall et al.'s hypothesis do not conform entirely to either the prediction made by their hypothesis, $H_0$, "anti-$H_e$" or my minium recall hypothesis. The implications of these results will be discussed in section 2.5. in the light of the additional data to be presented in sections 2.2.-2.4. below.

2.2. Post-Test on the Subjective Familiarity of the Japanese Stimuli

As an added check on the familiarity of the stimuli used in the experiment described above, I had decided to administer a questionnaire to the 20 subjects used in the experiment asking them to rank the stimuli according to this factor. It may be the case that word counts of published material do not accurately reflect day-to-day familiarity. Other tachistoscopic studies (e.g., Paivio and O'Neill 1970:274 (experiment #3)) which have attempted to exercise greater experimental control over familiarity (i.e., than exercised by word counts of published material) have employed similar procedures. In studies such as Paivio and O'Neill (1970) a group of subjects is asked to rank the proposed stimuli for an experiment before the actual experiment, and then a different group of subjects is used for the experiment. (In experiments of this type the two subject groups must be assumed to have been drawn from the same population.) However, in the present study
it was decided to conduct the familiarity questionnaire after the experiment so that the same subjects could be used for the familiarity ratings and the tachistoscopic task. (If the subjects had ranked the stimuli for familiarity before the experiment, their results in the tachistoscopic task might have been biased.) The advantage of having the actual subjects of the experiment rate the stimuli for familiarity was that no assumptions regarding the similarity of experimental subjects and questionnaire subjects would be necessary.

For the familiarity questionnaire, the subjects were asked to rank all of the 60 stimuli in terms of the everyday frequency with which they felt they encountered these words in speech and writing. For this purpose, the 60 stimuli were broken down into quadruplets by matching SA and MA noun-verb pairs in terms of orthographic length and frequency based on the word counts of published material. For example, the SA pair "koori"-"kooru" ("ice"-"freeze") was matched with the MA pair "yurushi"-"yurusu" ("permission"-"permit") in terms of length and frequency in print. The subjects were asked to rank the four items in each of the 15 such quadruplets on a scale of 1 to 4, "1" standing for the most familiar items and "4" for the least familiar. Subjects were told to assign the same number to items within the quadruplets which they felt had the same familiarity.

The quadruplets were arranged on the questionnaire response sheet without any other identification. In addition, the order of the items within each quadruplet on the response sheet was randomized separately for each subject. The 60 stimuli were written on the questionnaire response sheet in the hiragana syllabary for the reasons discussed
above (pp. 87-8).

The subjects were not placed under any time pressure for the questionnaire.

The sums of the rankings for the four categories of stimuli for each subject were used in the statistical analysis of this questionnaire data. The total sum of the rankings for the 15 stimuli in each category for all 20 subjects can be seen in Table 4. Please recall that higher numbers indicate items rated less familiar, and lower numbers indicate items rated more familiar.

Table 4
Total Familiarity Rankings for the Japanese Stimuli

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>SA verbs</th>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>733.5</td>
<td>649</td>
<td>814</td>
<td>806</td>
</tr>
</tbody>
</table>

Wilcoxon matched-pairs signed ranks tests were used to analyze the familiarity rankings by the 20 subjects according to the eight comparisons indicated in 2.1.1. (p. 75) to determine whether any of these showed a statistically significant difference. The results are shown in Table 5, where the symbols "<" and ">" signify "rated less familiar than" and "rated more familiar than," respectively.

Table 5 shows that, for the most part, the SA items were rated more familiar than the MA items, and that there was a tendency for the verbs to be rated more familiar than the nouns within derivational pairs. The results in Table 5 can be paraphrased as follows. (Please refer to the totals in Table 4.)
Table 5
Eight Comparisons of the Familiarity Rankings for the Four Categories of Stimuli Used in the Japanese Experiment

<table>
<thead>
<tr>
<th>familiarity comparison</th>
<th>T value</th>
<th>N showing a difference</th>
<th>two-tailed probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) SA nouns &lt; SA verbs</td>
<td>28.5</td>
<td>18</td>
<td>p&lt;.05</td>
</tr>
<tr>
<td>(2) MA nouns vs. MA verbs</td>
<td>53</td>
<td>16</td>
<td>NS</td>
</tr>
<tr>
<td>(3) (SA + MA nouns) &lt; (SA + MA verbs)</td>
<td>34.5</td>
<td>17</td>
<td>p&lt;.1</td>
</tr>
<tr>
<td>(4) SA nouns &gt; MA nouns</td>
<td>36.5</td>
<td>18</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(5) SA nouns vs. MA verbs</td>
<td>55.5</td>
<td>18</td>
<td>NS</td>
</tr>
<tr>
<td>(6) SA verbs &gt; MA nouns</td>
<td>19</td>
<td>18</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(7) SA verbs &gt; MA verbs</td>
<td>0</td>
<td>18</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(8) (SA nouns + verbs) &gt; (MA nouns + verbs)</td>
<td>1</td>
<td>18</td>
<td>p&lt;.01</td>
</tr>
</tbody>
</table>
Comparison 1: The SA verbs were rated significantly more familiar than the SA nouns, p<.05 with the two-tailed test.

Comparison 2: The MA verbs were rated more familiar than the MA nouns, but this difference did not attain statistical significance. The maximum value of T for significance at the .1 level (two-tailed test) with 16 subjects showing a difference is 35, and the actual T in this case was 53.

Comparison 3: All verbs (SA and MA combined) barely missed being rated significantly more familiar than all nouns, p<.1 for the two-tailed test. The maximum value of T for significance at the .05 level (two-tailed test) with 17 subjects showing a difference is 34, and the actual T was 34.5.

Comparison 4: The SA nouns were rated significantly more familiar than the MA nouns, p<.01 with the two-tailed test.

Comparison 5: The SA nouns were rated more familiar than the MA verbs, but this difference did not attain statistical significance. The maximum value of T for significance at the .1 level (two-tailed test) with 18 subjects showing a difference is 47, and the actual T was 55.5.

Comparison 6: The SA verbs were rated significantly more familiar than the MA nouns, p<.01 with the two-tailed test.

Comparison 7: The SA verbs were rated significantly more familiar than the MA verbs, p<.01 with the two-tailed test.

Comparison 8: All SA items (nouns and verbs combined) were rated significantly more familiar than MA items, p<.01 with the two-tailed test.
The results presented above are at odds with the word count of published material which was used to balance the four categories of stimuli for frequency of occurrence in everyday language. These findings are not altogether implausible since word counts of printed material are not necessarily an accurate reflection of day-to-day frequency of usage in conversation, etc. The above results thus raise the possibility that some of the significant differences reported in 2.1.2.5. with regards to the tachistoscopic results can be explained on the basis of the familiarity differences among the stimuli for the subjects. Other differences, however, will not be susceptible to such an explanation. For example, the difference between the recognition thresholds of the MA nouns and verbs cannot be explained by the familiarity ratings since the MA nouns—which had significantly lower thresholds than the MA verbs—tended to be rated less familiar than the MA verbs.

The familiarity ratings considered in conjunction with the tachistoscopic results also yield another important possibility independent of the issue of the cause of noun facilitation phenomena: It may be overall familiarity (which includes conversational familiarity) rather than solely frequency in print (for which my tachistoscopic stimuli were balanced) which determines tachistoscopic recognition thresholds. It might have been thought that the reverse would be the case since tachistoscopic recognition is a form of reading printed material.

The possibilities noted above and their implications will be discussed together with the other data of this chapter in section 2.5.
2.3. Post-Test on the Age-at-Acquisition of the Japanese Stimuli

As mentioned in Chapter 1 (p. 42), age-at-acquisition ratings can be used to predict relative tachistoscopic recognition thresholds when frequency is controlled. Lyons et al. (1978) asked people to rate a list of words according to the age at which they thought they had acquired them. For this purpose, a scale (proposed by Carroll and White (1973)) from 1–9 corresponding to ages from 2 to 13+, and school grades from prenursery to eighth grade and above was used. Thirty-three undergraduate subjects were asked to "consider when they first learned each test word in either the spoken or written form" (Lyons et al. 1978:180). For use in their tachistoscopic experiment, Lyons et al. were only interested in testing relatively early-acquired words against relatively late-acquired ones. Consequently, for their experiment they used only the words rated at the two extremes of their scale by the subjects. These early- and late-acquired words thus obtained had also been selected to match in terms of frequency according to Thorndike and Lorge (1944). Using an ascending method of limits design similar to that described above and in Holmes et al. (1971), Lyons et al. found that the tachistoscopic recognition thresholds were significantly lower (faster) for the words rated as being acquired earlier.

Naturally, we have to ask to what extent Lyons et al.'s subjects' ratings were actually based on age-at-acquisition (as opposed to general familiarity, for example) and to what extent these ratings were an accurate reflection of the—at least relative—ages at which the words had been acquired by the respondents. Lyons et al. (1978:181–2) make
the following points regarding these questions.

First, they report that the standard deviation of the age-at-acquisition ratings they obtained was small, although they also note that "this just implies that subjects are probably drawing on the same kind of information in making their judgments" (ibid.:181). They also cite Carroll and White's (1973) direct test of the validity of age-at-acquisition ratings. Carroll and White found that age-at-acquisition ratings correlate highly (.85) with the grades in which words occur with high relative frequency in the writings of children.

Lyons et al. (1978) conducted their own test of the validity of age-at-acquisition ratings. They interviewed 40 first graders to determine whether they knew the words slated for use in their tachistoscopic experiment, since children may know words they do not actually use in their writing.

The results of those interviews confirmed [the age-at-acquisition] ratings in that none of the late-acquired words [according to the undergraduates' ratings] were known by any of the [first grade] subjects. The early-acquired words were known to 74% of the first graders interviewed (Lyons et al. 1978:181-2).

The points made above partially confirm that age-at-acquisition ratings are actually based on age-at-acquisition. Questions remain, however.

From the points made by Lyons et al. we do not know to what degree age-at-acquisition ratings might be accurate. Lyons et al.'s own study (discussed above) with children regarding the validity of age-at-acquisition ratings only mentions two categories of ratings ("early" versus "late") and only deals with children of one school grade (first
grade). Are these ratings accurate only in a relative sense or—to some extent—in an absolute sense as well? That is, if a subject rates a word as having been learned in the seventh grade, what are the chances that the word was actually acquired during this time period—rather than such a rating merely indicating that the word so rated was acquired sometime between words rated "sixth grade" (or earlier) and words rated "eighth grade" (or later)?

In addition, we do not really know whether age-at-acquisition ratings are based on actual age-at-acquisition or on feelings of subjective familiarity, since Lyons et al.'s words which were rated differently for age-at-acquisition were only balanced for frequency according to Thorndike and Lorge (1944) and not according to subjective judgments. We should expect the actual age-at-acquisition of words to parallel word frequency/familiarity in some aspect—either for children or adults, actual counts or subjective ratings, etc.—since children should to some extent learn earlier those words they encounter more often. A problem arises when we want to take age-at-acquisition ratings at face value and go on to make or discuss theories regarding the results of such ratings—as I would like to do in this chapter. As long as subjective familiarity correlates with age-at-acquisition there is no problem whether it or actual age-at-acquisition lies at the bottom of age-at-acquisition ratings with regards to discussions of actual age-at-acquisition. Problems would arise when and if the two kinds of ratings should differ.

Despite, and also because of, the questions raised above with regards to the basis of age-at-acquisition ratings and their accuracy,
I decided to have the subjects used in my tachistoscopic experiment rate the tachistoscopic stimuli for age-at-acquisition—both as a further control on the stimuli and as a possible source of additional, relevant data. If nothing else, a comparison of this questionnaire with the familiarity questionnaire will help to clarify to what extent familiarity and age-at-acquisition ratings correlate with each other.

As with the questionnaire on subjective familiarity, I decided to conduct the age-at-acquisition questionnaire after the tachistoscopic experiment so that the same subjects could be used for both the experiment and the age-at-acquisition ratings. The subjects were asked to rank each of the 60 stimuli separately and without reference to any of the other stimuli, including the other member of the derivational pair which each stimulus formed a part of. To this end, the stimuli were completely randomized (again, separately for each subject) for printing on the questionnaire response sheet in the hiragana syllabary, without any ordinal numbers to mark the stimuli. Again, the subjects were allowed to complete the questionnaire without any time pressure.

For their ratings, the subjects were asked to place a number to the right of each word on the response sheet according to the code for the scale in Table 6, which is an adaptation of the scales used in Carroll and White (1973) and Lyons et al. (1978).

The sums of the age-at-acquisition ratings for the four categories of stimuli for each subject were used in the statistical analysis. The total sum of the ratings for the 15 stimuli in each category—i.e., SA nouns and verbs and MA nouns and verbs—for all the 20 subjects can
Table 6

Age-at-Acquisition Scale

<table>
<thead>
<tr>
<th>code</th>
<th>age</th>
<th>school grade</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>2</td>
<td>prenursery</td>
</tr>
<tr>
<td>2</td>
<td>3</td>
<td>nursery</td>
</tr>
<tr>
<td>3</td>
<td>4</td>
<td>nursery</td>
</tr>
<tr>
<td>4</td>
<td>5</td>
<td>nursery</td>
</tr>
<tr>
<td>5</td>
<td>6-7</td>
<td>first grade</td>
</tr>
<tr>
<td>6</td>
<td>7-8</td>
<td>second grade</td>
</tr>
<tr>
<td>7</td>
<td>8-9</td>
<td>third grade</td>
</tr>
<tr>
<td>8</td>
<td>9-10</td>
<td>fourth grade</td>
</tr>
<tr>
<td>9</td>
<td>10-11</td>
<td>fifth grade</td>
</tr>
<tr>
<td>10</td>
<td>11-12</td>
<td>sixth grade</td>
</tr>
<tr>
<td>11</td>
<td>12+</td>
<td>seventh grade and above</td>
</tr>
</tbody>
</table>

be seen in Table 7. Please recall that higher numbers indicate items rated as acquired later, and lower numbers indicate items rated as acquired earlier.

Table 7

Total Age-at-Acquisition Rankings for the Japanese Stimuli

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>SA verbs</th>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>1582</td>
<td>1443</td>
<td>1973.5</td>
<td>1864.5</td>
</tr>
</tbody>
</table>

Wilcoxon matched-pairs signed-ranks tests were used to analyze the age-at-acquisition ratings according to the eight comparisons indicated in 2.1.1. (p. 75) to determine whether any of these showed a statistically significant difference. The results are shown in Table 8.

Table 8 shows that, for the most part, the SA items were rated
Table 8

Eight Comparisons of the Age-at-Acquisition Rankings for the Four Categories of Stimuli Used in the Japanese Experiment

<table>
<thead>
<tr>
<th>age-at-acquisition comparison</th>
<th>T value</th>
<th>N showing a difference</th>
<th>two-tailed probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) SA verbs vs. SA nouns</td>
<td>42.5</td>
<td>20</td>
<td>p&lt;.02</td>
</tr>
<tr>
<td>(2) MA verbs vs. MA nouns</td>
<td>26</td>
<td>19</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(3) (SA + MA verbs) vs. (SA + MA nouns)</td>
<td>22</td>
<td>18</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(4) SA nouns vs. MA nouns</td>
<td>1</td>
<td>20</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(5) SA nouns vs. MA verbs</td>
<td>9.5</td>
<td>20</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(6) SA verbs vs. MA nouns</td>
<td>12</td>
<td>20</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(7) SA verbs vs. MA verbs</td>
<td>16</td>
<td>20</td>
<td>p&lt;.01</td>
</tr>
<tr>
<td>(8) (SA nouns + verbs) vs. (MA nouns + verbs)</td>
<td>6</td>
<td>20</td>
<td>p&lt;.01</td>
</tr>
</tbody>
</table>

1 The left hand member of each comparison was rated as acquired earlier than the right hand member.
as acquired earlier than the MA items, and that the verbs were rated as acquired earlier than the nouns within derivational pairs. The results in Table 8 can be paraphrased as follows. (Please refer to the totals in Table 7.)

Comparison 1: The SA verbs were rated significantly earlier in terms of age-at-acquisition than the SA nouns, \( p < .02 \) with the two-tailed test.

Comparison 2: The MA verbs were rated significantly earlier than the MA nouns, \( p < .01 \), two-tailed test.

Comparison 3: All verbs (SA and MA combined) were rated significantly earlier than all nouns, \( p < .01 \), two-tailed test.

Comparison 4: The SA nouns were rated significantly earlier than the MA nouns, \( p < .01 \), two-tailed test.

Comparison 5: The SA nouns were rated significantly earlier than the MA verbs, \( p < .01 \), two-tailed test.

Comparison 6: The SA verbs were rated significantly earlier than the MA nouns, \( p < .01 \), two-tailed test.

Comparison 7: The SA verbs were rated significantly earlier than the MA verbs, \( p < .01 \), two-tailed test.

Comparison 8: All SA items (nouns and verbs combined) were rated significantly earlier than all MA items, \( p < .01 \), two-tailed test.

The above results largely parallel those of the familiarity questionnaire reported above. The direction of the differences among the four categories of stimuli are the same for the results of the familiarity rankings and the age-at-acquisition ratings—as can be seen by comparing the totals in Table 4 and Table 7, respectively. However,
the differences which did not turn out to be statistically significant in the case of the familiarity rankings (i.e., comparisons 2, 3 and 5 from p. 75) were all significant in the case of the age-at-acquisition ratings. The possibility exists, therefore, that the age-at-acquisition ratings are a more sensitive reflection of the subjective familiarity of the stimuli for the subjects due to differences in the way the questionnaires were set up. This possibility, and the implications of the age-at-acquisition ratings for theories of noun facilitation and for an interpretation of the tachistoscopic results outlined above will be discussed in section 2.5. along with the general discussion of the other data contained in this chapter.

2.4 A Reading Aloud Test with a Brain-Damaged Subject Using the Japanese Tachistoscopic Stimuli

As indicated throughout Chapter 1, a theory of noun facilitation which can explain data from brain-damaged people and from experiments with normals is highly desirable. In search of data relevant to such a theory, Holmes et al. (1971) administered their tachistoscopic stimuli to five brain-damaged dyslexic subjects as a reading aloud test, following the traditional assumption that more performatively complex linguistic functions will be impaired as a result of brain damage before simpler ones. The results from this test paralleled those they obtained in their tachistoscopic experiment in that verbs were more difficult than nouns (see p. 18 above). I likewise decided to use my tachistoscopic stimuli in a reading aloud test with a male patient (OH) at the Tokyo Metropolitan Geriatric Hospital—in order to provide further data relevant to both Marshall et al.'s (1975)
hypothesis regarding noun facilitation and my alternative to their hypothesis put forth in section 2.1.1. above with respect to optionality of arguments.

Below, I will first describe the medical aspects of OH's case and then the linguistic symptoms which led me to believe that OH might show some kind of noun facilitation effect if given my tachistoscopic stimuli as a reading aloud test. Then I will describe the actual test and OH's performance on it.

OH was 53 years old at the time of the onset of his language difficulties due to a cerebrovascular accident (CVA), which occurred on September 23, 1978. On that day he had lost consciousness for the space of about a minute while engaged in minor maintenance work as part of his job as a chauffeur. It was after this brief loss of consciousness that his language problems appeared and he was brought to the hospital for observation. Prior to this date—on March 2, 1976—OH had been operated on for an aneurysm of the left internal carotid anterior communicating artery. However, after this operation, OH showed no language difficulties, and he subsequently returned to his place of employment where he was given non-strenuous work to perform.

OH was admitted to the Tokyo Metropolitan Geriatric Hospital for testing on October 12, 1978. At that time he showed a mild paralysis of the right side of his face, while his extremities were almost normal.

A computerized axial tomography (CT scan) was performed on April 19, 1979—i.e., nearly seven months post onset—at the Tokyo Metropolitan Geriatric Hospital. According to the interpretation of
the radiologist at the Tokyo Metropolitan Geriatric Hospital, OH's CT scan revealed large low density areas in the pre- and post-central sulcus areas of the left hemisphere--including the supramarginal gyrus--and in the left capsulo-putaminal area. The approximate cortical extent of this damage is shown in Fig. 2. Thus, OH suffered

Fig. 2 Approximate cortical extent of OH's brain damage based on the CT scan performed at the Tokyo Metropolitan Geriatric Hospital (April 19, 1979)—nearly seven months post onset.

both frontal and parietal lobe damage. As noted by the radiologist at the Geriatric Hospital, Broca's area (in the frontal lobe) and Wernicke's area (in the temporal lobe) appear to have been spared.

However, in addition to the above damage, OH's CT scan revealed an enlargement of the left anterior and posterior horns, with a
frontal-parietal porencephaly—i.e., a hole connecting OH's cortical and capsulo-putaminal damage with the left anterior horn. These conditions suggest a possible atrophy of the entire left hemisphere, making it difficult to claim a complete sparing of any part of OH's left hemisphere.

The type of brain damage described above can be expected to provide a variety of linguistic and other symptoms. Thus, a patient with parietal lobe damage in the dominant hemisphere can be expected to show some dyslexia and dysgraphia as well as visual and other sensory problems. Damage to the dominant frontal lobe in the area of the motor strip could be expected to cause articulatory problems. In addition, OH's linguistic performance could be expected to show a mixture of symptoms due to the proximity of his damage to Broca's and Wernicke's areas and the possible atrophy of areas in his left hemisphere as noted above.

OH's symptoms do in fact conform to the above outline as data obtained at the Tokyo Metropolitan Institute of Gerontology show.

Dr. Sasanuma and her colleagues at the Institute of Gerontology prepared a Z-score profile of OH's scores on a battery of diagnostic tests one month after the onset of his language problems. This Z-score profile can be seen in Fig. 3.

A "Z-score" is a score expressed as a deviation from the mean in standard deviation units. The means of the scores used in Fig. 3—and represented by the "0" line—were obtained from a group of 100 brain-damaged subjects with CVA etiologies. The negative numbers at the top of the chart in Fig. 3 indicate scores below the mean and the
Fig. 3. Z-score profile of OH's scores on a battery of diagnostic tests compiled by Dr. Sasanuma and her colleagues at the Tokyo Metropolitan Institute of Gerontology. An explanation of this chart is contained in the text (pp. 115-17).
positive numbers indicate scores above the mean. Dr. Sasanuma's Z-score profile shown in Fig. 3 is based on scores obtained with the Schuell-Sasanuma Examination of Aphasia (Sasanuma 1977).

From Fig. 3 we can see that OH's auditory comprehension is quite good, except where retention span was tested. Reading comprehension is good except where words in kana (either of the Japanese syllabaries) and commands are involved. This problem with words written in kana—as opposed to words in kanji (Sino-Japanese ideographs)—is also consonant with the picture of Broca's aphasia in Japanese and its frequently attendant apraxia of speech (Sasanuma and Fujimura 1971:13ff.). However, Dr. Sasanuma has also informed me that this differential impairment of kana processing and sparing of kanji can be found in other types of brain-damaged persons too. Likewise, oral reading—that is, reading out loud—is below the mean only for words in kana. OH's oral expression is good for automatized sequences, but his repetition and his description of pictures is depressed. However, OH's naming of objects is right around the mean in the profile shown in Fig. 3. With respect to writing, it is again words in kana which cause OH's performance to be depressed—along with the writing of grammatically complex units such as sentences.

Unfortunately, I have no data on OH's post onset visual capacity. The possibility of a visual problem, such as a right homonymous hemianopia (with impairment in the right visual field of each eye), cannot be ruled out given OH's type of damage, and I will return to this question below.
OH had no organic speech defects or hearing impairment.

Dr. Sasanuma has informed me that OH showed an improvement in his oral production and was speaking in some sentences by April 1979 (six months post onset).

With respect to the remarks made on p. 54 in Chapter 1, it is interesting to note that OH also showed a compound formation tendency when asked to read single kanji (Sino-Japanese ideographs) aloud on a test I designed in 1979. Thus, for example, OH read 汽 "ki" ("steam") as 汽船 "kisen" ("steamship"); 栃 "tochi" ("chestnut") as 栃木 "Tochigi" ("chestnut tree" (place name)); 期 "ki" ("period (of time)"), etc.

The aspects of OH's symptoms which led me to believe that OH might show some kind of noun facilitation effect if given my tachistoscopic stimuli as a reading aloud test were the following. In the therapy sessions which I observed, OH spoke rarely, if at all, and then only in short phrases—usually consisting of one or two words. These utterances most often consisted of nouns, with or without grammatical particles attached, but no verbs. If verbs were present in these short utterances by OH, they were likely to be extremely common ones, such as the copula "desu." When OH was asked to repeat entire sentences read to him by his therapist, he would often begin correctly with the one or two nouns beginning the sentence, with or without the correct grammatical particles. However, in this repetition exercise OH almost never succeeded on the first try in repeating the entire sentence complete with verb; nor did he ever produce the verb of a sentence...
while omitting the nouns. (Verbs in simple Japanese sentences are always sentence final.) Of course, retention span problems may have been involved in the foregoing symptoms.

In addition, OH seemed to frequently use the "te-"--or gerund--form of the Japanese verb when he did have a verb present in his productions. Lacking data from a long period of observation, I cannot state with certainty that OH's use of gerunds constituted an overuse of this form of the verb in the same sense and to the same extent as that among some non-Japanese speaking patients who show a noun facilitation effect (see pp. 14-5 and 20 in Chapter 1). However, the possibility does exist.

Finally, recall that the brain-damaged person studied by Marshall and Newcombe (1966) (see pp. 16-7 above) had parietal lobe damage and showed a noun facilitation effect primarily when reading aloud.

Having decided to conduct a reading aloud test with OH along the lines of Holmes et al. (1971) using the tachistoscopic stimuli, the actual execution of the test was straightforward. The same cards constructed for the tachistoscopic experiment were used to present the stimuli, one at a time, to OH. The stimuli on these cards (see p. 87 above) had the advantage of large printing and high legibility--important factors for testing brain-damaged persons. OH had no difficulty understanding the instructions for the test; he had already been given many reading aloud tests by this time. The instructions were simply to read aloud the printed words.

The test was administered with a relative lack of time pressure. That is, OH was given approximately a half minute to respond, and if
he could not respond correctly in this period the experimenter flipped to the next card. However, if OH gave the appearance of being close to producing a response additional time was allowed. In addition, if OH gave a response very close to the target word—e.g., with a distortion of a single segment—or if the therapist was unsure of the exact segments uttered by OH, he was encouraged to try the word again.

OH was given the reading aloud test—all 60 stimuli—on two separate occasions. For each occasion, the stimuli were completely and differently randomized.

The experimenter for each testing session was Ms. Hata, of the Communication Research Section of the Tokyo Metropolitan Institute of Gerontology, who also transcribed OH's responses.

The first administration of the reading aloud test was on January 18, 1979—that is, approximately four months after the onset (September 23, 1978) of OH’s language problems. On this first administration of the reading aloud test, five of the noun-verb pairs—three SA pairs and two MA pairs—were different from those used in the tachistoscopic experiment. These differences are also noted in Appendix 1. These differences between the stimuli used for the first reading aloud test and the tachistoscopic experiment came about because I was still organizing the stimulus materials for the experiment at the time of the first reading aloud test with OH. After the first reading aloud test, I changed a small number of the stimuli because of frequency, orthographic and other factors.

The number of words in each stimulus category correctly read aloud by OH on the first reading aloud test is shown in Table 9.
Table 9
OH's Results on the First Reading Aloud Test

<table>
<thead>
<tr>
<th></th>
<th>SA nouns</th>
<th>SA verbs</th>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>9</td>
<td>9</td>
<td>4</td>
<td></td>
</tr>
</tbody>
</table>

Recall that there were 15 words in each of the four categories shown in Table 9.

Determining what was a correct response and what was not in order to compile the above figures was not always straightforward. This is almost always the case with the performance of brain-damaged persons, but the criteria according to which such judgments are made are not always made explicit in the literature. For the above figures, slight distortions in place and manner of articulation, unusual pauses between segments, etc. were not counted as errors. This decision was made to allow for the fact that articulatory problems frequently accompany damage to the left frontal lobe, and it seemed necessary to distinguish any such problems from language problems per se. It should be noted, however, that this criterion was a double-edged sword and cannot be said to have selectively lowered or raised the total number correct in any of the four stimulus categories. Thus, the above mentioned criteria allowed the reading of "miokuri" ("seeing off(noun)") as [mioguri] to be marked as correct, but it also forced me to count as correct "mikomu" ("hope(verb)"), read as an indistinct nasal+[i] followed by a lengthy pause and then [komu]. (Recall that the minimum recall hypothesis predicts a better performance on MA nouns like
"miokuri" than on MA verbs like "miokuru.")

On the other hand, metatheses, segmental substitutions (i.e., segments differing from targets by more than one articulatory feature), the addition of segments (i.e., [jasumuko] for "yasumu" ("rest(verb)")), etc. were counted as errors.

Most importantly, nine of the responses OH made when presented with verbs were the corresponding derivationally related nouns. (No nouns were read as verbs.) Recall that such responses have been noted by Marshall and his colleagues and other investigators for patients who show a noun facilitation effect.

Three of OH's "nominalizations" on this first reading aloud test were produced in response to SA verbs, but six were made in response to MA verbs. All of these nominalizations are presented below in (2-12).

<table>
<thead>
<tr>
<th>(2-12) SA items</th>
<th>MA items</th>
</tr>
</thead>
<tbody>
<tr>
<td>targets (verbs)</td>
<td>responses (nouns)</td>
</tr>
<tr>
<td>kurushimu &quot;ache&quot;</td>
<td>kurushimi &quot;ache&quot;</td>
</tr>
<tr>
<td>katamaru &quot;harden&quot;</td>
<td>katamari &quot;lump&quot;</td>
</tr>
<tr>
<td>kumoru &quot;get cloudy&quot;</td>
<td>kumori &quot;cloudiness&quot;</td>
</tr>
<tr>
<td>responses (nouns)</td>
<td></td>
</tr>
<tr>
<td>konomu &quot;like&quot;</td>
<td>konomi &quot;liking&quot;</td>
</tr>
<tr>
<td>negau &quot;wish&quot;</td>
<td>negai &quot;wish&quot;</td>
</tr>
<tr>
<td>nayamu &quot;worry&quot;</td>
<td>nayami &quot;worry&quot;</td>
</tr>
<tr>
<td>kuyamu &quot;regret&quot;</td>
<td>kuyami &quot;regret&quot;</td>
</tr>
<tr>
<td>sasou &quot;invite&quot;</td>
<td>sasoi &quot;invitation&quot;</td>
</tr>
<tr>
<td>nerau &quot;aim&quot;</td>
<td>nerai &quot;aim, goal&quot;</td>
</tr>
</tbody>
</table>
OH was given the reading aloud test for the second time on March 19, 1979—that is, two months after the test described above and approximately six months post onset. By this time there had been some improvement in OH's speech as noted above (p. 118).

For this second reading aloud test the same materials and procedure were used, except that this time all of the stimuli were the same as those used in the tachistoscopic experiment. Again, the stimuli were presented to OH in a (different) randomized order.

The number of words (out of 15) in each category read correctly (according to the above criteria) in this second test is shown in Table 10.

<table>
<thead>
<tr>
<th>Table 10</th>
<th>OH's Results on the Second Reading Aloud Test</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>SA nouns</td>
</tr>
<tr>
<td></td>
<td>14</td>
</tr>
</tbody>
</table>

On this second administration of the reading aloud test, OH produced a total of three nominalizations when asked to read verbs. (Again, OH produced no verbs in response to noun stimuli.) There was one nominalization of an SA verb and two of MA verbs. These are shown in (2-13). All the nominalizations shown in (2-13) from the second testing session were also made during the first testing session.
The results from the two reading aloud tests presented above support my minimum recall hypothesis about the interaction of syntactic class and number of arguments--based on considerations of the optionality of arguments--outlined in section 2.1.1. above. That is, the numerical difference (=2) at each testing session--between the SA nouns and verbs read correctly appears to have been too small to be significant. There was no difference between the number of SA verbs and MA nouns read correctly at either testing session. At the same time, the one category which my minimum recall hypothesis predicted (see p. 81 above) would be more difficult than the others--that is, the MA verbs--had many fewer correct readings than the other three categories in both testing sessions. This interpretation is supported by the fact that the number of nominalizations in response to verb stimuli was twice as many for MA verbs as for the SA verbs on both occasions, although in the second testing session the total number of nominalizations was very small.

OH's performance improved on the second testing session by a total of 10 additional correct responses. This improvement is in line with the overall improvement in his condition noted above and is not unusual
for patients four to six months after the onset of language problems due to brain damage. It is doubtful whether OH could have remembered the test items from the first session to the second, especially since their order of presentation to him was randomized differently on both occasions. We have already seen (p. 117) that OH's retention span was below the mean of that generally shown by brain-damaged patients with language disturbances. In addition, some words missed the first time by OH were read correctly at the second test, and vice versa. Since the majority of SA nouns and verbs and MA nouns were read correctly on each testing session, many of the same items in these categories were read correctly at both sessions. However, only one MA verb was read correctly at both testing sessions. These facts seem to indicate that OH's performance on the second test was independent of his performance on the first test. (Among the five noun-verb pairs—ten stimulus items—which appeared only on the second test (see Appendix 1), only "kooru" ("freeze(SA verb)") was read incorrectly.)

In Chapter 1 (pp. 34 and 59) attention was drawn to the fact that a visual problem, such as a right homonymous hemianopia (an impairment of the right visual field of each eye), could lead to pseudo-derivational errors on the part of brain-damaged persons when reading words aloud. It might be questioned whether such an effect was operative in OH's performance on the reading aloud tests described above, since I have no data on OH's post onset visual capacity (see p. 117). Thus, it might be suggested that if OH had a right hemianopia he would not have been able to see the suffix of a word he was trying
to read aloud and might therefore have tended to guess nouns based on some kind of noun response bias. However, OH's performance does not support this suggestion, whether or not he actually had such a visual impairment, since his performance on the SA verbs was equal to his performance on the MA nouns and nearly equal to his performance on the SA nouns at both testing sessions.

Finally, it should be noted that OH's improvement from the first to the second test also lends support to my minimum recall hypothesis, although, again, the smallness of the numbers involved makes strong conclusions difficult. That is, in each of the three categories of SA nouns and verbs and MA nouns OH produced three more correct answers on the second test, while he had only one more correct answer in the MA verb category. From these data, a corollary of my minimum recall hypothesis might claim that the processing of words with fewer arguments improves faster after brain damage than that of words with more arguments—with MA nouns acting like SA nouns in this regard due to the optionality of their arguments.

Clearly, the above results from the two reading aloud tests with OH are of great relevance to the confirmation or disconfirmation of Marshall et al.'s (1975) hypothesis regarding noun facilitation and the minimum recall hypothesis I proposed in section 2.1.1. These results will be discussed, together with the other data presented throughout this chapter, in the next section where I will discuss the bearing of the data presented above on theories of noun facilitation.
2.5. General Discussion of the Japanese Data in Terms of Competing Theories of Noun Facilitation

To begin with, the results of the tachistoscopic experiment considered without reference to the other data presented above seem equivocal. The SA nouns and verbs showed no difference in recognition thresholds, and both were easier (i.e., recognized at lower thresholds) than either the MA nouns or verbs. This much agrees with Marshall et al.'s hypothesis. However, the MA nouns were also found to be significantly easier than the MA verbs, at a high level of significance even with the greater stringency of the Dunn procedure. This result is contrary to Marshall et al.'s hypothesis but is in line with my minimum recall hypothesis.

To argue from the tachistoscopic data in favor of Marshall et al.'s full recall hypothesis—and against the minimum recall hypothesis—one must find a way to explain away the finding that the MA nouns were easier to recognize than the MA verbs. At the same time, the results with respect to the SA items must be excluded from explanations other than that offered by Marshall et al.'s hypothesis. In other words, in explaining away the ease for the MA nouns one who wishes to argue in favor of Marshall et al.'s hypothesis must be careful to avoid explaining away the ease of the SA items as well.

On the other hand, to argue from the tachistoscopic data in favor of my minimum recall hypothesis—and against Marshall et al.'s full recall hypothesis—we must find a way to explain away the finding that the SA nouns and verbs were significantly easier than the MA nouns. (The minimum recall hypothesis does not differ from Marshall et al.'s
hypothesis in the sense that both theories predict that SA nouns and verbs will be (equally) easier than MA verbs.) At the same time, we must make sure that the results with respect to the MA nouns being easier than the MA verbs are excluded from explanations other than that offered by the factor of optionality of arguments under the minimum recall hypothesis.

Of course, if both the attempt to explain away the lower (faster) thresholds of the SA items and the attempt to explain away those of the MA nouns (relative to the MA verbs) fail, we will have to consider other possible interactions between syntactic class and number of arguments, as suggested above (p. 83).

Many of the single factors discussed in section 1.2.1. and elsewhere in Chapter 1 readily suggest themselves as possible bases for explaining away either the ease of the SA items or that of the MA nouns. However, any such attempt will prove more difficult with respect to the difference between the MA nouns and verbs since a principal advantage of the Japanese stimuli used in the tachistoscopic experiment is the fact that the very nature of the derivationally related noun-verb pairs is to reduce the number of factors which differ between the noun and verb members of such pairs.

Thus, the factors listed in (2-14)—all taken from the discussion in Chapter 1—cannot be used to explain the observed difference in recognition thresholds between the MA nouns and the MA verbs, irrespective of their relevance to the difference obtained between the SA items and the MA items.
The factors listed in (2-14) are not all of those discussed in Chapter 1; other factors are not relevant here or will be discussed below. Basically, the factors in (2-14) cannot be used to explain the lower thresholds for the MA nouns (than for the MA verbs) since the noun-verb members of the MA pairs do not differ in terms of these factors.

An interesting point brought up by the list of factors in (2-14) is that the main difference between the MA nouns and verbs used in the experiment is a syntactic one--i.e., optionality or non-optionality of arguments--and not a semantic one. Of course, in tasks such as tachistoscopic recognition, reading aloud tests, etc. some semantic recall from LTM must occur. However, I assume that this "semantic" recall did not differ for the MA nouns and verbs in my experiment, since to characterize the meaning of the MA nouns and verbs reference must be made to the same types and number of cases/arguments.

Note that the fact that the MA nouns and verbs failed to differ in terms of the factors listed in (2-14) and the fact that the MA nouns were recognized faster than the MA verbs also constitute a
disconfirmation of an explanation of noun facilitation phenomena in general in terms of the factors in (2-14).

However, three factors (one of which was mentioned in Chapter 1) still remain which might be used to distinguish between derivationally related MA nouns and verbs in Japanese, and thus to explain away the observed difference in thresholds between these items. I will discuss these three factors in turn, and why I feel they actually do not succeed in explaining the difference in thresholds between the MA nouns and verbs.

First, it was noted in Chapter 1 (p. 38) that Caplan et al. (1974:334-5) suggest that the frequency/familiarity of a derived form (such as "nomination") might summate with the frequency/familiarity of its base form (i.e., "nominate"), but not vice versa. This suggestion could be used to explain the lower thresholds of the MA nouns in my experiment if we assume the nouns in the derivationally related noun-verb pairs to be derived from the verbs. Some partial evidence against such an approach: We have seen in Chapter 1 examples of noun facilitation involving non-derived forms. When derived nouns are produced by brain-damaged persons for verb targets, the resulting nouns are sometimes much less frequent than the target verbs, making it hard to see the kind of summation effect suggested by Caplan et al. In addition, the results of the familiarity rankings reported in section 2.2. above showed a difference between MA nouns and verbs in the direction of MA nouns being less familiar than MA verbs, although this difference failed to reach statistical significance.
Second, the katakana ("italic") syllabary of Japanese is primarily used for three purposes: onomatopoeic words, signs and foreign words—which are mostly nouns. Here it might be suggested that the Japanese have an expectation for foreign words written in katakana to be nouns and that such an expectation could have transferred to the Japanese nouns in my experiment. That is, according to this suggestion, the subjects could have recognized the MA nouns faster than the MA verbs because they had an expectation for any words written in katakana to be nouns, based somehow on their expectations about foreign words in Japanese. This argument gains force if we consider Japanese onomatopoeic words as nominals. As noted above (p. 89) Japanese readers do have an expectation for Japanese words written in katakana to be onomatopoeic words. However, such a "transferral" effect seems unlikely, since the subjects knew that neither foreign words nor onomatopoeic words would be used in the experiment.

Third, there is a possibility that the nouns used in my experiment are actually category ambiguous. That is, such de-verbal forms ending in /i/ in Japanese might be either nouns or verbs according to one analysis. Kuno (1973:195-9) discusses the "-i continuative" form of the Japanese verb and its use in sentences such as that shown in (2-15) where it is analogous to the "-te gerundive" form of the verb.

\[
(2-15) \quad \text{John wa uwagi o } \{\text{nugi hangaa ni kakeita}. \{nude
\]

"John took off his jacket and put it on a hanger."

In Kuno's analysis the noun stimuli used in my experiment would presumably be category ambiguous in isolation. That is, we would have
to look at their function in a sentence to know whether to assign them to the category of nouns or verbs, much like "-ing" forms in English. Now, one might want to argue that the MA nouns used in my experiment were somehow easier to recognize because of their representation in both noun and verb categories. There are two reasons why I think this argument is not likely to be correct. First, there seems to be little doubt that there is a very strong set for the perception of de-verbal forms in /i/ as nouns when seen in isolation on the part of Japanese readers—as with nouns like "man" in English, which are category ambiguous because of usages like "to man the lifeboats." Second, the available experimental evidence suggests that category ambiguity should not facilitate tachistoscopic recognition. Caplan et al. (1974:30) report that category ambiguous words (e.g., "father") and morphologically "simple" nouns ending in "-er" (e.g., "river") were both recognized at higher (slower) thresholds than verb-derived agentives (e.g., "helper") when presented in the right visual field (favoring left hemisphere processing) via a tachistoscope. (Confusingly, with the left visual field/right hemisphere presentation, words like "helper" and "father" were easier than words like "river.") We also know that the left hemisphere is primarily responsible for the noun facilitation effect in tachistoscopic experiments (Marshall and Holmes 1974), as noted in Chapter 1 (p. 4). Finally, Wolf and Koff (1978:40) found that "verb-derived, pure nouns, and category ambiguous words [again, ending in "-er"] are equally difficult to process in a long-term memory task when lateralized stimulus presentation is not a factor." Recall that in my experiment, the stimuli were not
presented in a hemifield design, but were presented centrally.

I have argued against each of the above three possibilities for explaining away the lower thresholds for the MA nouns (than for the MA verbs) in my experiment. However, there is a further argument which applies equally to each of the three possible explanations discussed above. Namely, if any or all of the three approaches discussed above were valid—despite the counterarguments presented—then the SA nouns in my experiment should also have been easier to recognize than the SA verbs. This was not the case, however. And, yet, all of the three possibilities discussed above—regarding summated frequency, katakana expectations and category ambiguity—are just as applicable to the SA pairs in my experiment as to the MA pairs.

So far I have been discussing the possibility of using some of the factors discussed in Chapter 1 to explain away the lower thresholds for the MA nouns (relative to the MA verbs) in my experiment. Such attempts to explain away the lower thresholds of the MA nouns, if successful, would ultimately argue in favor of Marshall et al.'s full recall hypothesis. What possibility exists for using any of the same factors in the opposite direction? That is, can any of these factors be used to explain the lower thresholds for the SA items relative to the MA nouns, and thus argue in favor of my minimum recall hypothesis?

Actually, some of the factors listed in (2-14) and others discussed in Chapter 1 do stand a better chance of explaining the lower thresholds of the SA nouns and verbs (relative to the MA nouns), since the tachistoscopic stimuli were less controlled across pairs than within pairs. For example, imageability is a factor which seems to
differ between the SA items and the MA nouns. That is, the SA items appear to be more imageable than the MA items. This seems especially true for the SA nouns, some of which are [+ CONCRETE]. Unfortunately, however, Paivio and O'Neill (1970) seem to have disconfirmed a facilitating effect for concreteness/imageability on tachistoscopic recognition thresholds (see pp. 35-6 in Chapter 1). One might want to question the validity of Paivio and O'Neill's results, especially since concreteness and imageability appear to have a facilitating effect for many brain-damaged persons.

Fortunately for my minimum recall hypothesis, the results of the familiarity questionnaire reported in section 2.2. provide a better explanation of the lower thresholds for the SA items (relative to the MA nouns) in my experiment and thus remove the necessity of arguing against Paivio and O'Neill (1970). At the same time, the familiarity rankings cannot be used to explain the lower thresholds for the MA nouns (relative to the MA verbs), since the difference between the MA nouns and verbs in this regard was in the direction of the MA nouns being (non-significantly) less familiar than the MA verbs. In fact, I will argue below that there is even some reason for viewing this trend toward a difference between the MA nouns and verbs in terms of their respective familiarity as being more significant than revealed by the familiarity questionnaire alone.

Thus, in their familiarity rankings the 20 subjects ranked all of the SA items significantly more familiar than all of the MA items. The SA verbs were rated significantly more familiar than the MA nouns and significantly more familiar than the MA verbs (see pp.102-3). The
SA nouns were rated significantly more familiar than the MA nouns, but not significantly more familiar than the MA verbs—although the observed difference was in this direction. Furthermore, all nouns (SA and MA combined) just missed being rated significantly less familiar than all verbs (p<.1, T = 34 for p<.05 with 17 N's showing a difference). In line with this finding, the SA nouns were rated significantly less familiar than the SA verbs, but the familiarity ratings for the MA nouns were not significantly different from those for the MA verbs, although in the direction of being less familiar.

Since word frequency and rated familiarity have been found to be extremely powerful factors affecting tachistoscopic recognition thresholds, the above findings are the likeliest candidate to explain the lower thresholds for the SA items (relative to the MA nouns) in my experiment—given that we would expect the words rated more familiar to have lower thresholds. These results rescue my minimum recall hypothesis because otherwise my hypothesis does not predict any difference between SA items and MA nouns, since I am claiming that MA nouns can be recalled from LTM with no more arguments than SA items due to their ability to occur in grammatical single argument constructions.

The fact that SA nouns were not rated significantly more familiar than MA verbs (although there was a trend in this direction) does not cause any problems for my minimum recall hypothesis, since both my hypothesis and Marshall et al.'s full recall hypothesis predict lower thresholds for SA nouns and verbs than for MA verbs. Under my hypothesis the arguments of the MA verbs are obligatory even in
isolated recall.

However, this brings me to a potential problem in connection with my arguments above against three of the factors that might be used to explain away the lower thresholds for the MA nouns (relative to the MA verbs). That is, it was argued above (p. 133) that the three factors of summated frequency, katakana expectations and category ambiguity could not have contributed to the lower thresholds of the MA nouns (relative to the MA verbs) since the SA nouns—also susceptible to the same factors—did not have lower thresholds than the SA verbs. As an argument against the minimum recall hypothesis about why the MA nouns had lower thresholds than the MA verbs, one might want to claim that the reason the SA nouns were not easier than the SA verbs was that the effect of any or all of the three factors above was cancelled out by the lower familiarity of the SA nouns relative to the SA verbs, as shown in the familiarity ratings by the 20 subjects. This would leave the way open for an explanation of the lower thresholds for the MA nouns relative to the MA verbs on the basis of one or more of these three factors, assuming that the MA nouns and verbs did not differ in terms of rated familiarity.

However, I do not feel the above approach will hold much water. For one thing, the arguments already noted against the factors of summated frequency, katakana expectations and category ambiguity (pp. 130-3) still apply. For another, the fact that the SA verbs were not recognized significantly more easily than the SA nouns could easily have represented a "floor" effect. That is, even though the SA verbs were rated more familiar than the SA nouns, the reason that this did
not result in significantly lower thresholds for the SA verbs was possibly that the subjects simply could not recognize words of the same size any faster. Moreover, there was a trend for the MA nouns to be rated less familiar than the MA verbs ($T = 53$, $N' s$ showing a difference $= 16$, maximum $T$ for $p<.05 = 35$), parallel to the familiarity ratings for the SA nouns and verbs. It is possible that the method used to elicit the familiarity ratings was not sensitive enough to reveal a difference between the MA nouns and verbs. As discussed on p. 100 above, the subjects were asked to rank the members of quadruplets made up of an SA and an MA noun-verb pair matched for orthographic length and frequency in print, rather than being asked to rate each word on its own along a wider scale of, say, 1-10. The age-at-acquisition questionnaire becomes relevant here.

The results of the age-at-acquisition questionnaire discussed in section 2.3. parallel those of the familiarity questionnaire—with judgments of "later acquired" and "earlier acquired" parallel to judgments of "less familiar" and "more familiar," respectively. In fact, a main difference in the results between these two questionnaires lies in the fact that the difference between the MA nouns and verbs in terms of age-at-acquisition (with MA nouns judged later acquired than MA verbs) is statistically significant, whereas it is only a trend in terms of familiarity (with MA nouns judged less familiar than MA verbs, but not significantly so) (see pp. 103 and 111). This significant difference in the age-at-acquisition questionnaire between MA nouns and verbs also contributes to a clearer difference between the ratings of all nouns and all verbs in the case of the age-at-acquisition
questionnaire.

Now, it was pointed out earlier (pp. 106-7) that we cannot be sure to what extent the age-at-acquisition ratings reported by Lyons et al. (1978) were independent of subjective familiarity, since we know that subjective judgments of familiarity can diverge from frequency as determined by word counts of published material (as indicated above with my Japanese stimuli), and since Lyons et al.'s stimuli were only balanced with respect to the latter. In effect, my familiarity and age-at-acquisition questionnaires show that there is a close correlation between subjective ratings of familiarity and age-at-acquisition ratings—whatever the correspondence between the age-at-acquisition ratings and actual age of acquisition. Thus, there may be some reason to consider the age-at-acquisition ratings with respect to the MA nouns being acquired later than the MA verbs as a more revealing indication of the subjective familiarity of the MA nouns versus the MA verbs, especially since the age-at-acquisition questionnaire did not have the methodological problems noted above (p. 137) for the familiarity questionnaire. (Recall that in the age-at-acquisition questionnaire words were rated individually on a scale from 1 to 10.)

In summary, then, the age-at-acquisition ratings support the claim made above (top of p. 137) that both the SA and MA nouns may have been equally less familiar than the SA and MA verbs, respectively. And this point helps support my minimum recall hypothesis and its explanation of the lower thresholds for the MA nouns (relative to the MA verbs) by maintaining the argument noted on p. 133 against summated frequency, katakana expectations and category ambiguity as explanations
of the lower thresholds for the MA nouns (relative to the MA verbs). That is, if any or all of these factors were the cause of the lower thresholds of the MA nouns relative to the MA verbs, then the SA nouns should also have had lower thresholds than the SA verbs, since the same factors apply within SA pairs. We cannot claim that these factors were cancelled out in the case of the SA items by the greater familiarity of the SA verbs relative to the SA nouns, since we have reason to believe that the MA verbs were also significantly more familiar than the MA nouns.

It should be noted here that the results of the familiarity questionnaire and the age-at-acquisition questionnaire (considered as an indicator of subjective familiarity) could actually throw into jeopardy both Marshall et al.'s (1975) hypothesis and my minimum recall hypothesis. That is, it might be claimed that the only cause of the threshold differences found in my experiment was the familiarity differences among the four categories of stimuli. Such an argument would claim that number of arguments has no effect on tachistoscopic recognition thresholds and perhaps plays no role in noun facilitation phenomena in general. However, the fact that the MA verbs, had, after all, higher (slower) thresholds than the MA nouns argues against such a claim. If familiarity were the only effective factor with respect to tachistoscopic recognition thresholds, the MA nouns in my experiment should have had at least equal (and probably higher) thresholds than the MA verbs following the discussion above of the age-at-acquisition ratings as an indication of the subjective familiarity of the stimuli. Below (p. 149), I will discuss OH's performance on my reading aloud
tests as also weighing against a claim that all the differences among
the thresholds in my experiment can be explained solely on the basis
of subjective familiarity.

One might also use the subjective familiarity ratings reported
in section 2.4. to argue in favor of "anti-H_e" (see p. 76 above). Recall that the claims of "anti-H_e" are the reverse of Marshall et al.'s
claims. That is, "anti-H_e" claims that nouns should be easier to
recognize tachistoscopically than verbs, but that there should be no
difference between either SA and MA nouns or between SA and MA verbs.

Using the familiarity ratings reported in section 2.4., one could argue
that the SA nouns and verbs had lower thresholds than the MA nouns and
verbs, respectively, because the former were more familiar. Following
the same line of reasoning, one could claim that the SA nouns failed
to have lower thresholds than the SA verbs—a result which runs counter
to "anti-H_e"—because the greater familiarity of the SA verbs acted
to counteract the noun facilitation effect posited by "anti-H_e" in this
case. Under this view, one might claim that there was a noun
facilitation effect for the MA nouns since the familiarity ratings of
the MA verbs were not significantly greater than those of the MA nouns
(see comparison 2, p. 103 above).

I feel that the above arguments in favor of "anti-H_e" are probably
incorrect in the light of the totality of the data presented throughout
this chapter. As already discussed above (pp. 137ff.), the age-at-
acquisition questionnaire may have been a more adequate measure of the
subjective familiarity of my tachistoscopic stimuli, and in this
questionnaire the MA nouns did differ significantly from the MA verbs.
in the direction parallel to the familiarity ratings. Below (p. 149), I will discuss OH's performance on my reading aloud tests as also weighing against an "anti-H_e" interpretation of the differences among the thresholds in my experiment.

Here, it is necessary to speculate on the implications of the results of the age-at-acquisition questionnaire assuming that they actually do reflect the age-at-acquisition of the four categories of stimuli, at least relative to each other.

To begin with, the SA nouns were rated as being acquired later than the SA verbs, and the MA nouns were rated later than the MA verbs. If these results truly reflect (at least relative) age-at-acquisition, neither Marshall et al.'s (1975) hypothesis nor my alternative predicts such a relative order of acquisition.

Marshall et al. (1975) do not discuss their theory in terms of language acquisition. However, extrapolating from their hypothesis, we would not expect any significant difference between the SA nouns and verbs or between the MA nouns and verbs in terms of relative age of acquisition (assuming, of course, the control of other relevant factors), since the members of the noun-verb pairs used in my experiment do not differ in terms of number of (at least possible) arguments.

On the other hand, a similar extrapolation from my alternative to Marshall et al.'s hypothesis also cannot explain the fact that the SA and MA nouns were rated as being acquired later than the SA and MA verbs, respectively. In fact, my hypothesis would appear to require that MA nouns be acquired earlier than MA verbs, since the arguments for MA nouns are optional in the sense that MA nouns can be used in
grammatical single argument constructions, and children might thus be able to learn MA nouns earlier than MA verbs on the basis of this grammatical difference.

If the SA and MA nouns in my stimulus list are acquired later by Japanese children than the corresponding SA and MA verbs, respectively, as the age-at-acquisition questionnaire suggests, it may be the case that this is due to a general effect of derived forms being acquired after base forms. Alternatively, the age-at-acquisition questionnaire results could simply indicate an a posteriori subjective strategy on the part of the subjects to assign a later age of acquisition to the noun stimuli based on their feelings about the nouns being derived forms and the verbs being base forms, with no necessary correlation between such a response strategy and the actual age-at-acquisition of the stimuli.

If we accept either of the two explanations just offered, there is nothing further to say with respect to the results of the age-at-acquisition questionnaire and Marshall et al.'s (1975) hypothesis. That is, the other differences in the results of the age-at-acquisition questionnaire can all be extrapolated from Marshall et al.'s (1975) hypothesis, since the SA nouns and verbs were rated as being acquired earlier than the MA nouns and verbs, respectively.

However, an extrapolation from my alternative to Marshall et al.'s hypothesis appears to require that MA nouns not differ from SA nouns and verbs in terms of relative age of acquisition, and that SA nouns and verbs and MA nouns all be acquired earlier than MA verbs.
The factor of familiarity could be used again here to help the cause of my claims based on optionality of arguments. That is, the MA nouns being heard and seen less often than the SA items could override the effect on age-at-acquisition which might be expected from the optionality of the arguments of MA nouns. Such an argument based on the factor of familiarity would follow the line developed above (pp. 134 ff.) to explain the lower tachistoscopic thresholds for the SA nouns and verbs relative to the MA nouns in my experiment. Such an argument could even be used to explain the possible later acquisition of SA and MA nouns relative to SA and MA verbs, respectively, in place of the distinction between derived and base forms (p. 142 above).

However, there is another reason why MA nouns could be acquired later than SA nouns based on an aspect of the factor of number of arguments which I have not yet discussed.

Above (p. 129), it was claimed that the main difference between the MA nouns and verbs used in my experiment is a syntactic one, i.e., optional arguments in the case of MA nouns and obligatory arguments in the case of MA verbs. A characterization of the meaning of MA lexical items has to refer to the cases/arguments which enter into their semantic representation. And this is true irrespective of the syntactic class—and, hence, the syntactic optionality of the arguments—of lexical items.

Now, the acquisition of vocabulary of course includes the acquisition of both the semantic and the syntactic representations. Thus, I would suggest that in the acquisition of language the number of arguments needed to characterize semantic representations could very
well obviate any simplification in the learning process which might accrue from the fact that MA nouns can occur in single argument constructions. This in turn could mean that MA nouns cannot be acquired until the child is able to construct syntactically the multiple argument utterances into which such items can enter to form grammatical phrases—or it might not. However, whatever the answer to the immediately foregoing suggestion, my alternative to Marshall et al.'s full recall hypothesis claims that adults can process MA nouns in tasks requiring little or no syntactic computation with a recall from LTM of only the minimum number of arguments required to construct a grammatical utterance with them. This hypothetical "shallow" processing ability on the part of adults which I am proposing may actually accrue to the child as soon as the child has a grasp of the meaning of MA nouns and has learned to construct the simplest grammatical utterances into which MA nouns can enter. On the other hand, the proposed shallow processing ability might require further practice and processes of "automatization" to become fully operative. These are fascinating questions for future research.

To summarize, earlier in this dissertation it was claimed that a theory of noun facilitation which can tie together aspects of language acquisition, experimental findings and data from brain-damaged persons is highly desirable. And it was noted in Chapter 1 (pp.61 ff.) that Marshall et al.'s (1975) hypothesis is such a theory in principle. Also, I have noted (p. 82) that my alternative to Marshall et al.'s hypothesis still makes crucial use of the factor of number of arguments in the explanation of noun facilitation phenomena. In this sense, my
hypothesis retains the potential of tying together aspects of noun facilitation in language acquisition, experimentation with normal adults and observations of brain-damaged persons. Lastly, I have tried to show that the hypothetical relatively late acquisition of MA nouns would not constitute a disconfirmation of my alternative to Marshall et al.'s hypothesis, since there is good reason to expect the complexity of semantic representations to influence the relative age-at-acquisition of lexical items, perhaps to the point of obscuring any simplification in the learning process which might accrue from the fact that a child would hear MA nouns sometimes used in single argument utterances.

At this point it might be suggested that the difference between the tachistoscopic recognition thresholds of the MA nouns in my experiment and those of the SA items was due in part to the difference in the number of arguments required to characterize the semantic representations of these items. Such an approach would constitute one of the possible interactions mentioned on p. 83 above. The line of reasoning might go as follows: "Both the syntactic and the semantic aspects of the number of arguments factor are relevant to performative complexity, whether in language acquisition, psycholinguistic experimentation or linguistic breakdown due to brain damage. The MA nouns in my experiment were easier to recognize than the MA verbs because of the syntactic difference between these items--i.e., optional arguments for the MA nouns and obligatory arguments for the MA verbs. On the other hand, the MA nouns still had higher thresholds than the SA items because of the more complex semantic representations--
in terms of number of arguments—of the former." Under this view we would have expected a threshold difference between the MA nouns and the SA items even if subjective familiarity had been fully controlled across all of the four categories of stimuli.

I feel that the suggestions just made do not contradict my proposed alternative to Marshall et al.'s (1975) hypothesis so much as they amend it. Recall that the strong version of my minimum recall hypothesis predicts no threshold difference between MA nouns and SA items, based on syntactic considerations of optionality of arguments. The confirmation or disconfirmation of the suggestions made above is an empirical question. However, I do not feel that these suggestions are likely to be confirmed. The difference between the familiarity ratings of the MA nouns and the SA items is quite sufficient to explain the observed difference in recognition thresholds between these items. Here, the data from the reading aloud tests with OH become relevant.

As noted in section 2.4. (p. 124), OH did not show much of a difference at either testing session in the total number of SA nouns and verbs read correctly, and no difference between the number of SA verbs and MA nouns read correctly. Only the MA verbs were markedly impaired in OH's performance. These relative results among the four categories of stimuli are reliable since they are the same for the two different sessions—separated by approximately two months—on which OH was tested.

OH's results on the reading aloud tests are exactly those predicted by the minimum recall hypothesis.
At least for OH, therefore, the difference in the number of arguments required to semantically characterize the MA nouns and the SA items did not result in poorer performance for the former. This fits with the fact that in my limited observation of OH I found no evidence of a uniquely semantic impairment—such as might have been evidenced by an abundance of semantic paraphasias, for example. OH's results thus suggest that (for him at least) only the syntactic aspect of the number of arguments factor is operative in the noun facilitation symptoms resulting from his brain damage. OH's results do not rule out an effect for the semantic aspect of number of arguments in tachistoscopic experiments with normals, like mine, where there are also time and visual constraints operating, but they do seem to place the burden of proof on those who would argue for such an effect.

If it is true that the semantic aspect of the number of arguments factor does not operate to distinguish MA nouns and SA items in experiments with normal and brain-damaged subjects, we will need an explanation of why this is the case. This would take us far afield here, and I do not actually have much to say in this regard, except perhaps to note that the semantic representation of any lexical item in an adult's vocabulary is likely to be such a complex matter that the effective difference (in terms of performance) of, say, plus or minus two arguments could easily be lost or masked.

A word should perhaps be said about the nominalizations OH produced in response to the SA target verbs. He made three such nominalizations at the first testing session and one at the second (see pp. 122 and 124
above). Under both Marshall et al.'s (1975) hypothesis and my minimum recall hypothesis, these results are unpredicted. (Note that my hypothesis as already stated (p. 82 above), can explain nominalizations produced in response to MA target verbs.) The numbers involved here are small and may indicate nothing more than a generalized preference for nouns over verbs based on the kinds of considerations noted on p. 82 above. On the other hand, it may be necessary to distinguish between "zero argument" words and SA words. That is, SA verbs "control" one argument, whereas nouns—serving as arguments themselves—inserted into slots such as that in (2-16)

(2-16) I hit the ________.

cannot be said to really control any arguments (cf. Chomsky 1965:115). In this sense, we may wish to characterize nouns as having a "zero argument" possibility not shared by even SA verbs. This difference between nouns and even SA verbs could have formed the basis of the nominalizations OH made in response to the SA target verbs, along the lines of the suggestions made on p. 82 above. The fact that OH produced more nominalizations in response to the MA target verbs than in response to the SA target verbs would then have been due to the greater decrease in number of obligatory arguments when going from MA verbs to (MA or SA) nouns than when going from SA verbs.

The fact that the SA nouns did not have lower recognition thresholds than the SA verbs in my tachistoscopic experiment does not contradict the suggestions just made, since the SA verbs were also rated as significantly more familiar than the SA nouns by the subjects.
OH's performance on both of the reading aloud tests also argues strongly against an explanation of the tachistoscopic results solely on the basis of subjective familiarity (discussed on p. 139 above). Recall that such an explanation would have tried to claim that all of the differences in the thresholds obtained in my experiment were due to the relative familiarity of the stimuli and had nothing to do with the factor of number of arguments. Such an argument is cast in doubt by the fact that OH did not show a poorer performance for the MA nouns relative to the SA items, even though the MA nouns were rated less familiar in the questionnaire discussed in section 2.2. OH's results on the reading aloud tests seem to be due solely to a syntactic impairment. This all fits with the fact that OH's Z-score profile (p. 116 above) shows no difference between high and low frequency words in either auditory comprehension or object naming. Likewise, OH's results on the reading aloud tests do not lend support to the "anti-H" (see pp. 140-1 above) since OH showed no difference in his total number of correct readings between the SA verbs and the MA nouns at either testing session. Since the line of argument I have developed in favor of my minimum recall hypothesis covers both the tachistoscopic results and OH's results, it is to be preferred to explanations (such as one based on subjective familiarity of noun facilitation) which can only be applied to the tachistoscopic results.

OH's results on the reading aloud tests also apply to the possible role of the factors of summated frequency, katakana expectations and category ambiguity discussed earlier (pp. 130 ff.). Claims based on the factors just noted would all have predicted a poorer performance
on the SA and MA verbs than on the SA and MA nouns, respectively. However, as with the tachistoscopic results, OH's performance did not confirm these claims. For OH, SA nouns and verbs and MA nouns were approximately equal in number of correct responses.

2.6. Concluding Remarks

The above discussion of the Japanese data in terms of Marshall et al.'s (1975) hypothesis regarding noun facilitation and my alternative hypothesis does nothing if it does not reveal the complexity of the issues involved in this kind of experimental work.

At the same time, it is this very complexity which I feel is an asset of the present study. That is, in the work discussed above I was able to control or otherwise eliminate a larger number of confounding factors—such as frequency/familiarity, etc.—than usually confronted in studies of this type. This was accomplished by using the Japanese noun-verb pairs and by considering the results from several tests, which also allowed me to say something about noun facilitation in the three areas of language acquisition, experimental studies and linguistic breakdown due to brain damage.

Many new, and perhaps more complex, questions have replaced the ones discussed at the beginning of this chapter as a result of the work discussed above. Although the totality of the evidence discussed above (including that discussed much earlier on p. 82) may not be conclusive, I feel that it does lean heavily in support of the minimum recall hypothesis, rather than toward Marshall et al.'s (1975) full recall theory or other theories based on an interaction of factors.
Basically, the lower thresholds of the Japanese MA nouns relative to the MA verbs found in the tachistoscopic experiment can most plausibly be explained by the minimum recall hypothesis, and the lower thresholds of the SA items relative to the MA nouns (which was unpredicted by the minimum recall hypothesis) have a plausible explanation in terms of the subjects' familiarity ratings of the stimuli. In addition, OH's results on the two reading aloud tests conform exactly to the predictions of the minimum recall hypothesis. The burden of proof thus appears to lie with those who would argue against my alternative to Marshall et al.'s hypothesis.

In the next chapter I will present data from a tachistoscopic experiment in English. This data was gathered both for purposes of comparison with the Japanese data and as a check on the latter. In particular, the English experiment to be discussed allowed the testing of several ideas not testable using Japanese stimuli, just as the use of Japanese stimuli allowed a greater control of factors (such as word-internal orthographic/phonological redundancy) than permitted in English.

To be sure, the kind of work discussed in this chapter, and to be discussed in the next, has many implications for formal theories of grammar, other areas of psycholinguistic investigation and neuropsychology in general. These implications, as well as the possible future directions of work on noun facilitation, will be discussed in the fourth and final chapter of this dissertation.
CHAPTER 3.

TESTING THE EFFECT OF OPTIONAL AND OBLIGATORY ARGUMENTS ON
NOUN FACILITATION IN ENGLISH

3.0. Introduction

This chapter will continue the examination of the role of number
of arguments in noun facilitation phenomena begun in Chapter 2. However,
in this chapter I will be discussing English data. As in Chapter 2, I
will be concerned with the data to be presented mainly in terms of
Marshall et al.'s (1975) theory of noun facilitation and my minimum
recall alternative set out in section 2.1. of Chapter 2.

Accordingly, section 3.1. will deal with a tachistoscopic experiment
(using English stimuli) which is in some respects similar to that
discussed in Chapter 2. However, the experiment to be discussed in 3.1.
will also address aspects of the role of number of arguments in noun
facilitation which could not be dealt with using Japanese stimuli.
Section 3.2. will present the results of a post-test administered to the
subjects who participated in the tachistoscopic experiment. The
questionnaire to be discussed in section 3.2. will again deal with the
subjective familiarity of the tachistoscopic stimuli. In section 3.3.
I will discuss the data from the preceding sections in terms of a con­
firmation or disconfirmation of Marshall et al.'s hypothesis and my
minimum recall hypothesis. Attention will also be given to the correla­
tion of the English data with the Japanese data presented in Chapter 2.
3.1. A Tachistoscopic Investigation of the Role of Optional and Obligatory Arguments in Noun Facilitation Using English Stimuli

3.1.1. Rationale and Experimental Hypotheses

One of the goals of the tachistoscopic experiment to be discussed was simply to replicate in English the results of the experiment presented in Chapter 2 which used Japanese stimuli. To accomplish this end, I had originally planned to reproduce the four categories of stimuli used in the Japanese tachistoscopic experiment for use with English-speaking subjects. Thus, I had originally wanted to have English SA nouns and verbs and MA nouns and verbs as part of the stimuli for the experiment to be described.

However, as already mentioned in Chapter 2 (p. 73), the construction of such stimulus lists in English poses many problems. In the end, I was able to construct lists of SA and MA nouns, and MA verbs with obligatory arguments (like "put"), but I was not able to come up with a list of English SA verbs adequate for use in a tachistoscopic design utilizing single-word presentation. The problems involved with compiling an adequate list of English SA verbs—as well as the other types of stimuli—will be discussed in section 3.1.2.3 ("Materials").

This lack of success in compiling a list of suitable SA verbs in English would not, by itself, have been enough for me to completely give up the English version of the tachistoscopic experiment. However, English actually offers the possibility of three categories of verb stimuli besides SA verbs, which made the English version of the experiment discussed in Chapter 2 all the more interesting in view of the issues discussed in that chapter.
The first of these categories of verb stimuli was that already mentioned: MA verbs (like "put") with obligatory arguments even at the level of surface structure. (See (2-7)--p. 78--in Chapter 2.) I have already noted (p. 86) that Japanese does not have MA verbs (like "put") which have obligatory arguments even at the level of surface structure. That is, in conversational contexts where arguments are understood, all Japanese MA verbs permit a wide range of ellipsis. (In Chapter 2--pp. 86-7--I argued that the Japanese MA verbs used as stimuli in my tachistoscopic experiment had obligatory arguments in underlying structure since there must be shared information in order for "zero pronominalization" to occur in Japanese conversational contexts.) Henceforth, I will refer to English MA verbs which always require two (or more) arguments even in surface structure (as well as underlying structure, of course) as "surface structure MA verbs" ("SS-MA verbs").

The second of the three categories of verb stimuli mentioned above is that of verbs which can function either as SA or MA words. Examples of such verbs are "write" and "grow" as the sentences in (3-1) demonstrate.

(3-1)  a. He usually writes in the morning.
       b. He writes many letters.
       c. The baby is growing quickly.
       d. He grows grapes.

As we will see in section 3.1.2.3. ("Materials"), the fact that many English verbs can function either as SA or MA words is one of the reasons why it is difficult to devise a list of purely SA verbs in English. Japanese does have verbs which can function either as SA or MA words like "write." However, transitivity or intransitivity is often
morphologically marked in Japanese for verbs like "grow." (The Japanese MA verbs used as stimuli in the experiment discussed in Chapter 2 were not of the former types, but were analogous to the type of English verbs to be discussed below.) Henceforth, I will refer to English verbs which can function either as SA or MA words as "SA/MA verbs."

The last of the three categories of verb stimuli mentioned above is actually the most similar to the Japanese MA verbs used in the experiment discussed in Chapter 2. That is, the English verbs in this category normally have two or more obligatory arguments, which can, however, be elided in certain, very limited conversational contexts involving shared information. Examples of verbs in this category are "know" and "listen," as the examples in (3-2) illustrate.

(3-2)  
   a. Do you know that Tom was elected president of his class?  
   b. Yes, I know.  
   c. He's listening. (Said, for example, by someone while engaged in a private conversation with another person when they both catch sight of a third party eavesdropping.)

Note that (3-2b) and (3-2c) are unacceptable in the absence of shared information. Again, in Chapter 2 I argued that the Japanese MA verbs used as stimuli in my experiment had obligatory arguments at the level of underlying structure—despite the widespread ellipsis of arguments in Japanese—since there must be shared information in order for such ellipsis to occur. (And the fact that MA verbs were recognized at higher (i.e., slower) thresholds than SA verbs in my tachistoscopic experiment with Japanese stimuli in part confirmed this view.) Here, with respect to the third category of English verbs, I would make an analogous argument. The only difference is that the English verbs in this category
never permit the ellipsis of the subject argument (except in imperatives) whereas Japanese MA verbs do, as (2-11b) (p. 87) shows. Roeper and Siegel (1978:244-6) discuss syntactic tests to distinguish verbs like "know" and "listen"—which they call "zero morph" verbs—from the two other types of English verbs mentioned above. I will return to their paper at length in Chapter 4. Henceforth, I will refer to this third category of English MA verbs which sometimes permit ellipsis of all but the subject argument in certain limited conversational contexts, but which seem to have fully obligatory multiple arguments at the level of underlying structure, as "underlying structure MA verbs" ("US-MA verbs"). (The Japanese MA verbs used in the experiment discussed in Chapter 2 were also US-MA verbs.)

Thus, for the tachistoscopic experiment with English stimuli to be discussed below I was able to include the five categories of stimuli listed in (3-3).

(3-3)

SA nouns
MA nouns
SS-MA verbs
SA/MA verbs
US-MA verbs

The five categories of stimuli in (3-3) give rise to the ten different pair-wise comparisons listed below in (3-4) of relevance to Marshall et al.'s full recall hypothesis and my minimum recall hypothesis.

(3-4)

(1) SA nouns vs. MA nouns
(2) SA nouns vs. SS-MA verbs
(3) SA nouns vs. SA/MA verbs
(4) SA nouns vs. US-MA verbs
(5) MA nouns vs. SS-MA verbs
(3-4) (continued)

(6) MA nouns vs. SA/MA verbs
(7) MA nouns vs. US-MA verbs
(8) SS-MA verbs vs. SA/MA verbs
(9) SS-MA verbs vs. US-MA verbs
(10) SA/MA verbs vs. US-MA verbs

Marshall et al.'s (1975) full recall hypothesis and my minimum recall hypothesis make different predictions with regards to tachistoscopic recognition thresholds for some of the comparisons listed in (3-4).

Recall that the full recall hypothesis claims that the process of recognizing and reading a word "involves the retrieval of the full lexical entry associated with the particular visual stimulus" (Marshall et al. 1975:321). On the other hand, my minimum recall hypothesis claims that in task situations involving a minimum of syntactic computation—such as single-word tachistoscopic recognition—only the minimum number of arguments necessary to construct a grammatical utterance (with a given word to be recognized) will be recalled from LTM. The different claims with respect to tachistoscopic recognition thresholds made by these two hypotheses for some of the comparisons in (3-4) are stated below. Note that Marshall et al.'s full recall hypothesis and my minimum recall hypothesis do not make different predictions for comparisons 2, 4, 6 and 9.

Comparison 1: Marshall et al. claim that SA nouns will be recognized tachistoscopically at lower (faster) thresholds than MA nouns. The minimum recall hypothesis predicts no difference in tachistoscopic thresholds between SA and MA nouns, since English MA
nouns can be used in grammatical single argument constructions (see p. 77).

Comparison 2: Both Marshall et al.'s hypothesis and the minimum recall hypothesis predict that SA nouns will have lower thresholds than SS-MA verbs.

Comparison 3: Marshall et al. do not discuss SA/MA verbs in terms of their theory of noun facilitation in their 1975 paper. However, to be consistent it would seem that their full recall hypothesis would have to predict significantly lower (faster) thresholds for SA nouns than for SA/MA verbs. The minimum recall hypothesis, on the other hand, predicts no difference between the thresholds of SA nouns and those of SA/MA verbs, since I am claiming that in single-word recognition SA/MA verbs will be recalled as SA items. (Note that these statements of the claims made by the minimum and the full recall hypotheses regarding SA/MA verbs might require some qualification if we posit separate lexical entries for the SA and MA usages of what I have called "SA/MA" verbs like "grow" and "write." I will return to this question in section 4.1.1. of Chapter 4.)

Here, one might want to distinguish between verbs like "grow" and those like "write." Verbs like "grow" when used as in (3-1c) are both syntactically and semantically intransitive. On the other hand, verbs like "write" when used as in (3-1a) are still semantically transitive. Because of the problems to be discussed in section 3.1.2.3. ("Materials"), I had to include both types of verbs among my SA/MA verb stimuli. I will return to this question below in section 3.3. ("General Discussion") and in Chapter 4.
Comparison 4: Both Marshall et al.'s hypothesis and the minimum recall hypothesis predict that SA nouns will have lower thresholds than US-MA verbs.

Comparison 5: Marshall et al.'s theory predicts no difference between the thresholds of MA nouns and those of SS-MA verbs. Again, the minimum recall hypothesis predicts lower (faster) thresholds for MA nouns relative to SS-MA verbs, since MA nouns can be used in grammatical sentences with none of their arguments present.

Comparison 6: In line with what was said above with respect to comparison 3, Marshall et al.'s hypothesis seems to predict no difference between the thresholds of MA nouns and those of SA/MA verbs. The minimum recall hypothesis also predicts no difference between MA nouns and SA/MA verbs. However, Marshall et al.'s theory and my theory disagree about the reasons for this predicted lack of a difference between MA nouns and SA/MA verbs. Marshall et al.'s theory predicts MA nouns and SA/MA verbs will not differ because both types of stimuli will be recalled from LTM with all arguments. My theory predicts MA nouns and SA/MA verbs will not differ because both types of stimuli will be recalled from LTM with only obligatory arguments. This difference between our theories is, of course, reflected in the predictions our theories make with respect to the other comparisons in (3-4).

Comparison 7: Marshall et al.'s hypothesis predicts no difference between the thresholds of MA nouns and those of US-MA verbs. The minimum recall hypothesis, on the other hand, predicts lower thresholds for the MA nouns than for the US-MA verbs.
Comparison 8: Marshall et al.'s theory predicts no difference between the thresholds of SS-MA verbs and those of SA/MA verbs. The minimum recall hypothesis, on the other hand, predicts lower thresholds for SA/MA verbs than for SS-MA verbs.

Comparison 9: Both Marshall et al.'s hypothesis and the minimum recall hypothesis predict no difference between the thresholds of SS-MA verbs and those of US-MA verbs.


The above predictions regarding the comparisons set out in (3-4) are summarized below in Table 11, where the symbols "<", ">" and "=" refer to threshold values.

Table 11
Summary of Predictions Made by the Full Recall and the Minimum Recall Hypotheses for the English Tachistoscopic Experiment

<table>
<thead>
<tr>
<th>comparison</th>
<th>Marshall et al.'s full recall hypothesis</th>
<th>minimum recall hypothesis</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>SA Ns &lt; MA Ns</td>
<td>SA Ns = MA Ns</td>
</tr>
<tr>
<td>(2)</td>
<td>SA Ns &lt; SS-MA Vs</td>
<td>SA Ns &lt; SS-MA Vs</td>
</tr>
<tr>
<td>(3)</td>
<td>SA Ns &lt; SA/MA Vs</td>
<td>SA Ns = SA/MA Vs</td>
</tr>
<tr>
<td>(4)</td>
<td>MA Ns &lt; US-MA Vs</td>
<td>MA Ns &lt; SS-MA Vs</td>
</tr>
<tr>
<td>(5)</td>
<td>MA Ns = SS-MA Vs</td>
<td>MA Ns = SA/MA Vs</td>
</tr>
<tr>
<td>(6)</td>
<td>MA Ns &lt; US-MA Vs</td>
<td>MA Ns &lt; SS-MA Vs</td>
</tr>
<tr>
<td>(7)</td>
<td>MA Ns = US-MA Vs</td>
<td>MA Ns &lt; US-MA Vs</td>
</tr>
<tr>
<td>(8)</td>
<td>SS-MA Vs = SA/MA Vs</td>
<td>SS-MA Vs &lt; SA/MA Vs</td>
</tr>
<tr>
<td>(9)</td>
<td>SS-MA Vs = US-MA Vs</td>
<td>SS-MA Vs = US-MA Vs</td>
</tr>
<tr>
<td>(10)</td>
<td>SA/MA Vs = US-MA Vs</td>
<td>SA/MA Vs &lt; US-MA Vs</td>
</tr>
</tbody>
</table>
In addition to the comparisons discussed above being tests of Marshall et al.'s full recall hypothesis and my minimum recall hypothesis, the category of US-MA verbs also provides a check on one of the assumptions made in Chapter 2. That is, if my Japanese MA verb stimuli and English US-MA verbs both have obligatory arguments—despite conversational ellipsis—in the sense that even when they are used in superficially single argument constructions their underlying arguments must be generated by a speaker/listener, then English US-MA verbs should be no easier to recognize tachistoscopically than English SS-MA verbs when other factors are controlled and should bear the same relation to English SA and MA nouns in terms of recognition difficulty that Japanese (US-)MA verbs bear to Japanese SA and MA nouns.

3.1.2. Method

3.1.2.1. Subjects

The subjects for the English experiment were 20 normal adult speakers of standard United States mainland English. (I did not want to use speakers of Hawaiian English because of the fact that SS-MA verbs in standard American English are sometimes US-MA verbs in Hawaiian English.) Seventeen of the subjects were graduate students and/or holders of a graduate degree affiliated with the East-West Center (Honolulu, Hawaii) and/or the University of Hawaii. Two of the subjects were undergraduates at the University of Hawaii. One subject was a college graduate working in Honolulu.

There were seven female subjects and 13 male subjects, all considering themselves right handed. All had normal or corrected (20/20) vision. They ranged in age from 18 to 35 years (mean: 27).
The two undergraduate subjects received extra credit for a course in psychology by participating in the experiment, but the other 18 subjects were unrewarded. The subjects can be described as poorly to well motivated.

3.1.2.2. Apparatus

A three-channel Scientific Prototype Model GB tachistoscope, belonging to the Department of Psychology of the University of Hawaii, was used in the experiment. Exposure duration could be adjusted in increments of 1 msec starting from 1 msec. The exposure fields measured 5 x 7 inches.

The tachistoscope was equipped with controls which permitted the programming which will be described in section 3.1.2.4. ("Procedure").

3.1.2.3 Materials

As stated, the stimuli for this experiment consisted of five lists of English words: SA nouns, MA nouns, SS-MA verbs, SA/MA verbs and US-MA verbs. Each list had 15 items, bringing the total number of stimuli to 75.

Although certain derivational noun-verb pairs in English (e.g., "gift"-"give") resemble the Japanese pairs used in the experiment discussed in Chapter 2, there are not many such pairs in English suitable to the experimental design to be discussed. Thus, for the English tachistoscopic experiment I had to construct five lists essentially from scratch. The problems involved in constructing these lists were sometimes quite frustrating.
For one thing, a great number of English words are orthographically category ambiguous. This is the case with many homophonous lexical items (e.g., "desire"--noun and verb) and many non-homophonous homographs (e.g., "convict"--noun and verb). This effectively eliminated many derivationally related noun-verb pairs. The orthographic ambiguity of English lexical items is not limited to derivationally related items. This is the case with "hide" (noun and verb). When one is trying to construct a stimulus list for an experiment such as that to be described, such orthographic ambiguity in the prospective stimuli can be hard to spot.

Other derivationally related noun-verb pairs in English involve a change in orthographic length (e.g., "belief"-"believe"), although the members of such pairs would still be usable in a tachistoscopic experiment as long as the average orthographic length balanced across the different stimulus categories. Still other derivationally related noun-verb pairs in English involve a change in morphological composition as well as length (e.g. "nomination"-"nominate").

Of course, cases of semantic ambiguity even in the absence of category ambiguity had to be eliminated as much as possible from the stimulus list.

As if the above problems were not sufficiently discouraging, English poses a further challenge not posed by Japanese. In Chapter 1 (pp. 39-40) it was noted that a major factor influencing tachistoscopic recognition thresholds is word-internal orthographic/phonological predictability, redundancy, transition probabilities, etc. For
example, Riegel and Riegel (1961:162)—working in German—found that "if a rare syllable appears at the beginning of a word it will facilitate [tachistoscopic] recognition." In my own pilot study for the English tachistoscopic experiment such effects emerged quite strongly.

Thus, in pre-testing I found that the primary strategy of English-speaking subjects when asked to read English words tachistoscopically was what might be called an "orthographic gestalt strategy." That is, when these subjects could not read a word correctly they seemed to use those visual and/or phonological aspects of the word which they had recognized to form a guess about what the stimulus word was. Thus, the stimulus "weep" was sometimes read as "week" or "weed," reflecting a guessing strategy based on a recognition of the first three letters of the stimulus item and the existence of a fourth letter (also possibly indicating a left-to-right reading strategy). Another example: The stimulus "enemy" was sometimes read as "energy." Many other examples could be given.

In the pilot study I found that a primary component of the orthographic strategy discussed above was the occurrence of certain syllables or combinations of letters, especially consonants. Thus, I found that common affixes—such as "con"—seemed to facilitate recognition. Such common affixes are almost certainly processed visually as single units due to the frequency of their occurrence. (Note that Riegel and Riegel's finding—discussed above—of a facilitation for words with uncommon initial syllables probably resulted from
their subjects' ability to predict the rest of such words based on their knowledge of German.) Sequences of two identical letters (e.g., as in "allow"), especially consonants, also seemed to facilitate recognition.

I should note here that, for the most part, the Japanese stimuli used in the experiment discussed in Chapter 2 did not give rise to the kinds of response phenomena discussed above regarding orthographic strategies. The subjects in the Japanese experiment seemed to respond with many fewer incorrect guesses when they could not read a stimulus item. That is, when the Japanese subjects could not recognize a stimulus item they tended to give an "I don't know" response more often than the subjects in the English tachistoscopic experiment. This could very well have been due to the syllabic nature of the Japanese orthography (i.e., katakana) used to present the stimuli and the basically (C)V canonical phonological structure of Japanese. (Note that, although there are some qualifications which should be made regarding this characterization of the canonical phonological structure of Japanese, all of the stimuli in my experiment conformed to this characterization.) That is, within a printed Japanese word the occurrence of one or more syllabic symbols does not contribute much to the predictability of other symbols on a phonological and/or orthographic basis. Of course, a major advantage of the Japanese stimuli was that within pairs the factors of word-internal orthographic/phonological predictability, redundancy, transition probabilities, etc. were exactly the same for the noun and verb members of each pair. This was particularly important since the crucial comparison in Chapter 2 was within MA
pairs--i.e., MA nouns versus MA verbs.

Of course, in addition to the orthographic factors discussed above, which it was necessary to control across the five categories of stimuli for the English experiment, I also had to control the five categories of stimuli for the "usual" factors of frequency, orthographic length, etc.

From the above discussion, it is not hard to imagine why there has been no tachistoscopic test of Marshall et al.'s (1975) hypothesis--at least in English--until now. I was naturally hesitant to invest the amount of time required for an experiment of this sort given the existence of these potentially critical problems from the beginning. Enough can go wrong in a tachistoscopic experiment even with ideal stimuli! However, after extensive pre-testing and trying out different possible stimulus lists, I concluded that it might just be worth trying--especially considering the existence of the three categories of English verbs discussed in 3.1.1. and their relevance to Marshall et al.'s full recall hypothesis and the minimum recall hypothesis.

The results of my attempt to construct suitable stimulus lists for the five categories of stimuli in (3-3) can be seen in Appendix 2, examples of which are contained below in (3-5).

<table>
<thead>
<tr>
<th></th>
<th>SA nouns</th>
<th>MA nouns</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>life</td>
<td>gift</td>
</tr>
<tr>
<td></td>
<td>beauty</td>
<td>danger</td>
</tr>
<tr>
<td></td>
<td>bath</td>
<td>debt</td>
</tr>
<tr>
<td>SS-MA verbs</td>
<td>SA/MA verbs</td>
<td>US-MA verbs</td>
</tr>
<tr>
<td></td>
<td>warn</td>
<td>grow</td>
</tr>
<tr>
<td></td>
<td>obtain</td>
<td>retire</td>
</tr>
<tr>
<td></td>
<td>bury</td>
<td>sing</td>
</tr>
</tbody>
</table>

I consider my attempts to eliminate the problem factors discussed above
from the stimuli in Appendix 2 to have been fairly successful. However, as an additional check on any residual differences among the five categories of stimuli in terms of orthographic processing I will discuss a special analysis of the error responses of the subjects in section 3.1.2.5. ("Results") below.

The five categories of stimuli were matched for frequency using the word count of Thorndike and Lorge (1944) (based on printed material). The categories of MA nouns, SS-MA verbs, SA/MA verbs and US-MA verbs were matched in terms of total number of (at least possible) arguments, ± 1-3 arguments in each category.

The stimuli were printed on the tachistoscopic cards using "Chartpak" rub-on upper case letters (style of letters: "Zentak Grotesk"; size: 20 point) approximately one quarter inch high. The stimuli were printed in the center of the cards, in accordance with the requirements of the experimental procedure to be described below.

3.1.2.4. Procedure

The procedure for the English tachistoscopic experiment was the same as that for the Japanese experiment discussed in Chapter 2 with the following minor exceptions.

The black fixation marker of the Japanese experiment was replaced by a black frame surrounding a white space, as shown in Fig. 4. Like the fixation marker used in the Japanese experiment, the white space within the "fixation frame" used for the English experiment was slightly larger than the longest word among the stimuli. The fixation frame was presented in the center of the white 5" x 7" visual field,
Fig. 4 Fixation frame for the English experiment.

with the white space directly over the space for the appearance of the stimuli.

The fixation frame described above was used for the English experiment rather than the bar-type fixation marker described in Chapter 2 since it was found in the pre-testing to have virtually no masking effect. In fact, its effect was to make the stimuli "stand out" against the white background of the tachistoscopic viewing field. Recall that the bar-type fixation marker in the Japanese experiment was assumed to have equally masked all of the orthographic symbols in each stimulus item (p. 91). I felt that this difference between "no masking" and "equal masking" might have been more important in the English experiment than in the Japanese experiment because of the orthographic factors discussed in the previous section. For example, the English alphabet has several pairs of letters—such as "B" and "R"—which are especially easy to confuse in a tachistoscopic task situation when
masking is present.

The program for the presentation of the stimuli was as described in Chapter 2 (pp. 90-3)—with two seconds for the fixation frame; each presentation of a stimulus followed by a blank white card; etc.

However, in the English experiment the subjects had to initiate the commencement of each trial with a particular stimulus item by means of a foot pedal. In the Japanese experiment one presentation of a stimulus item was followed by a four-second presentation of a blank white card, which was in turn automatically followed by another presentation of the fixation marker. In the English experiment, on the other hand, the subjects initiated the presentation of the fixation frame (by means of the foot pedal)—upon a "go" signal from the experimenter—which was followed by the program described in Chapter 2 up until the blank card following the presentation of the stimulus. In the case of the English experiment, the fixation frame did not reappear until the subject pressed the foot pedal again—upon another "go" signal from the experimenter.

I decided to have the subjects initiate each presentation of a stimulus with the foot pedal in order to allow the experimenter to accurately record all the error responses of the subjects between trials. As in the Japanese experiment, the subjects were encouraged to guess if they were not sure of the stimulus presented. The number of error responses—or misguesses—was used in the interpretation of the threshold results, as will be described in the next section.

All other aspects of the experimental procedure were the same as those described in Chapter 2. The order of the presentation of the
stimuli was completely randomized separately for each subject. The subjects were given a break after completing 38 of the 75 stimuli.

I served as the experimenter for all of the subjects, and the experiment was carried out in the Békésy Laboratory of Neurobiology at the University of Hawaii.

3.1.3 Results

The totals of the recognition thresholds for the five categories of stimuli for each subject were used in the statistical analysis. The total recognition times (in msec) for the 15 stimuli in each category and across all the replications by the 20 subjects can be seen below in Table 12.

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>MA nouns</th>
<th>SS-MA verbs</th>
<th>SA/MA verbs</th>
<th>US-MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>2057</td>
<td>2109</td>
<td>2119</td>
<td>2110</td>
<td>2168</td>
</tr>
</tbody>
</table>

In addition to the recognition thresholds, the total number of error responses—or misguesses—for the five categories of stimuli for each subject were also used in the statistical analysis. An "error response" was any word a subject said in response to a stimulus which was not the actual stimulus word—as, for example, the response "energy"
for the stimulus "enemy." These error responses most often bore a visual/orthographic relation to the actual stimuli, as mentioned above (p. 164).

The total number of error responses for the 15 stimuli in each category and across all the replications by the 20 subjects can be seen below in Table 13.

Table 13

Error Response Totals for the English Tachistoscopic Experiment across All Subjects and All Items in Each Category

<table>
<thead>
<tr>
<th>Category</th>
<th>Total Error Responses</th>
</tr>
</thead>
<tbody>
<tr>
<td>SA nouns</td>
<td>157</td>
</tr>
<tr>
<td>MA nouns</td>
<td>234</td>
</tr>
<tr>
<td>SS-MA verbs</td>
<td>207</td>
</tr>
<tr>
<td>SA/MA verbs</td>
<td>202</td>
</tr>
<tr>
<td>US-MA verbs</td>
<td>248</td>
</tr>
</tbody>
</table>

My rationale for using the number of error responses in the statistical analysis to be discussed below was as follows. If a given stimulus item was easily confused with another word or words in English—due to the kind of orthographic factors discussed above in section 3.1.2.3.—then that stimulus item should have tended to elicit those other words as error responses fairly consistently across subjects. All other things being equal, stimulus items with a high degree of this "orthographic confusability" should have elicited more error responses than stimulus items less easily confused with other English words on the basis of orthographic factors, although even items with a low degree of such confusability should have elicited some error responses at the
lower (faster) exposure durations.

Now, we would naturally expect a higher number of error responses—and, by extension, the orthographic factors underlying them—to correlate with higher (slower) recognition thresholds. This expectation could relate to the threshold results obtained with the English experiment in the following three possible ways.

First, if no difference were found among the five categories of stimuli in terms of number of error responses, then any difference in recognition thresholds among the categories could be attributed to aspects of the number of arguments factor (assuming, of course, the control of other confounding variables).

Second, if there were a difference among the five stimulus categories in terms of number of error responses and it were to completely parallel the difference among the five stimulus categories in terms of recognition thresholds, then the results would definitely be equivocal. In this case, we would not know whether the threshold differences were due to orthographic factors or differences in terms of aspects of the number of arguments factor—or both.

Third, if a difference among the five stimulus categories in terms of number of error responses did not completely parallel the difference among the five stimulus categories in terms of recognition thresholds, then we might still be able to conclude something about the effect of number of arguments independent of orthographic factors. For example, if one stimulus category had significantly lower thresholds than another category, and the former category also had a higher number of error responses (which should cause higher thresholds) than the latter
category, then we could conclude that the factor which caused the lower thresholds of the first category operated in spite of the orthographic effects. Likewise, even if the direction of the differences among the five stimulus categories in terms of thresholds and number of error responses were parallel but the relative size of the differences were not parallel, we might be able to draw a conclusion about the effects of number of arguments independent of orthographic factors. For example, if a given stimulus category had both the lowest thresholds and the lowest number of error responses but the difference between it and the other categories in terms of thresholds were much greater than in terms of number of error responses, we might be able to conclude that the factor which caused the lower thresholds operated independently of the orthographic effects.

Given the above rationale, I concluded—with the help of Greg Lee—that an analysis of covariance was the appropriate statistical procedure to use to separate the two aspects of the threshold results of the English experiment—i.e., threshold values due to stimulus category and those due to orthographic confusability.

Keppel (1973:477ff.) contains a discussion of the logic of the analysis of covariance design, as well as a discussion of the assumptions underlying this statistical procedure. As Keppel notes (ibid.:484), one assumption of this type of design is that the treatment variable (in this case, stimulus category—i.e., SA nouns, MA nouns, etc.) perse does not affect the values of the covariate (i.e., number of error responses). If this assumption is not valid, then the use of the analysis of covariance design is inappropriate, since any attempt to
separate the threshold effects due to stimulus category per se from those due to orthographic confusability using an analysis of covariance in such a situation would be impossible.

Now, of course the number of error responses in the English experiment varied among the five stimulus categories, as Table 13 shows. However, I assumed that this variation was not due to the factors of syntactic class and number and type (i.e., optional versus obligatory) of arguments which defined the five stimulus categories, but rather was due to an unequal distribution of orthographic confusability among the five categories. That is, I assumed that the number of error responses was determined by the factor of orthographic confusability, and that number/type of arguments and syntactic class did not have any effect on the number of error responses. (Of course, I still assumed that both stimulus category and orthographic confusability (independently) affected the threshold values.) The assumption that only orthographic confusability affected the number of error responses is supported by the fact that the error responses most often bore an orthographic relation to the target stimuli as described above (p. 164), and the fact that there appeared to be no tendency for the error responses to be of a particular syntactic class or to have fewer arguments than the target stimuli.

Another requirement of the analysis of covariance is that a test of the homogeneity of regression for the different treatment categories (SA nouns, MA nouns, etc.) be conducted before the results of the analysis can be validly interpreted (Keppel 1973:484). This test compares the regression between the dependent variable (i.e., thresholds)
and the covariate (i.e., number of error responses) across the five treatment categories. The analysis of covariance requires that the shape of this regression not differ significantly among the treatment categories, since the adjustments to be made by the analysis of covariance for the effect of the covariate are based on an average of these regression slopes (loc. cit.).

The test of homogeneity of regression which I computed for the data of the English experiment resulted in an $F$ of .138--far below that required for statistical significance at even the .1 or the .25 level. Thus, it can be concluded that the regression slopes for the relation between the thresholds and the number of error responses for the five stimulus categories were essentially homogeneous.

The analysis of variance and covariances with repeated measures from the P2V program of the Biomedical Computer Programs, P-Series (1977), edited by Dixon and Brown, was used to perform the statistical calculations needed as a basis for calculating the ten comparisons in (3-4). This program included a one-way anova having five levels corresponding to the five stimulus categories. The resulting anova table (Table 14 below) shows the relation of "category" (i.e., SA nouns, MA nouns, etc.) and the covariate (i.e., the number of error responses) to the dependent variable (i.e., recognition thresholds).

Table 14 indicates that the category to which stimulus items belonged (i.e., SA nouns, MA nouns, SS-MA nouns, SA/MA verbs or US-MA verbs) had a significant effect ($p < .046$) on their recognition thresholds, independent of the effect that can be attributed to the covariate or its causes.
Table 14

Analysis of Covariance Results for the Thresholds of the English Tachistoscopic Experiment

<table>
<thead>
<tr>
<th>source</th>
<th>sums of squares</th>
<th>df</th>
<th>mean squares</th>
<th>F</th>
<th>one-tail probability</th>
<th>beta estimates</th>
</tr>
</thead>
<tbody>
<tr>
<td>regression within stimulus categories (i.e., SA nouns, MA nouns, etc.):</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>mean</td>
<td>202082.57504</td>
<td>1</td>
<td>202082.57504</td>
<td>35.95</td>
<td>p &lt; .001</td>
<td></td>
</tr>
<tr>
<td>covariate (i.e., number of error responses)</td>
<td>98381.47227</td>
<td>1</td>
<td>98381.47227</td>
<td>17.50</td>
<td>p &lt; .001</td>
<td>3.48997</td>
</tr>
<tr>
<td>error</td>
<td>101175.03773</td>
<td>18</td>
<td>5620.83543</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>totals</td>
<td>401639.08504</td>
<td>20</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>regression across stimulus categories:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>stimulus category</td>
<td>62.52904</td>
<td>4</td>
<td>15.63226</td>
<td>2.55</td>
<td>p &lt; .046</td>
<td></td>
</tr>
<tr>
<td>covariate</td>
<td>605.38055</td>
<td>1</td>
<td>605.38055</td>
<td>98.63</td>
<td>p &lt; .001</td>
<td>0.88294</td>
</tr>
<tr>
<td>error</td>
<td>460.35945</td>
<td>75</td>
<td>6.13813</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>totals</td>
<td>1128.26904</td>
<td>80</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 14 also shows that the covariate (i.e., number of error responses) had a highly significant effect (beyond the .001 level) on the recognition thresholds both within and across stimulus categories, independent of the effect that can be attributed to the category factor. From an inspection of the threshold results and the number of error responses, in Table 12 and Table 13 respectively, we can see that a smaller number of error responses was associated with lower (faster) recognition thresholds, which was to be expected.

From the above, it appears that the number of error responses (the covariate) had a much stronger effect on the recognition thresholds than the category to which the stimuli belonged. Recall that the effect of number of error responses on the recognition thresholds was significant at the .001 level (both within and across stimulus categories), and that the effect of stimulus category was significant at the .046 level.

However, Table 14 also shows that the category to which the stimuli belonged had a significant effect on the recognition thresholds independent of the covariate's effect. To determine the nature of this effect—i.e., which stimulus categories were lower than which others—it is necessary to examine the subsequent t-tests (based on the above anova) of the ten comparisons in (3-4).

Now, the analysis of covariance used for the results of the experiment also yielded "adjusted means" for the recognition thresholds of the five stimulus categories. That is, the statistical program recalculated the mean thresholds for the five stimulus categories after removing the statistical effect of the number of error responses. The non-adjusted and the adjusted threshold means for each stimulus
category are shown below in Table 15.

Table 15

Non-Adjusted and Adjusted Mean Recognition Thresholds in Milliseconds for Each Stimulus Category in the English Tachistoscopic Experiment

(The non-adjusted means presented below represent the totals in Table 12 divided by the number of subjects. The adjusted means likewise represent the average adjusted threshold total in each stimulus category containing 15 items across the 20 subjects.)

<table>
<thead>
<tr>
<th></th>
<th>SA nouns</th>
<th>MA nouns</th>
</tr>
</thead>
<tbody>
<tr>
<td>non-adjusted  mean</td>
<td>102.85</td>
<td>105.45</td>
</tr>
<tr>
<td>adjusted mean</td>
<td>105.25</td>
<td>104.34</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>SS-MA verbs</th>
<th>SA/MA verbs</th>
<th>US-MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>non-adjusted  mean</td>
<td>105.95</td>
<td>105.50</td>
<td>108.40</td>
</tr>
<tr>
<td>adjusted mean</td>
<td>106.07</td>
<td>105.85</td>
<td>106.65</td>
</tr>
</tbody>
</table>

Now we can begin to put together the results shown in Tables 12 and 13. First, notice that the relation of the mean threshold values of the five categories in Table 15 to each other is partly different for the non-adjusted and the adjusted means. That is, for the non-adjusted means the SA nouns have the lowest (fastest) thresholds of all the five stimulus categories, while for the adjusted means the MA nouns have the lowest thresholds of all the categories. Furthermore, the difference between the mean thresholds of the SA and MA nouns with the non-adjusted means (where SA nouns have the lower values) appears much greater than with the adjusted means (where MA nouns have the lower values). These differences between the non-adjusted and the adjusted means of the SA and MA nouns reflect the much lower number of error responses for the
SA nouns than for any of the other four stimulus categories, as shown in Table 13. That is, when the threshold differences among the stimuli that correlate with the number of error responses (i.e., the covariate) are removed, the SA nouns end up with a slightly higher mean threshold value than the MA nouns.

The categories of SS-MA verbs, SA/MA verbs and US-MA verbs have the same relative positions with respect to each other with both the non-adjusted and the adjusted mean threshold values. That is, the SS-MA verbs, SA/MA verbs and US-MA verbs ranked fourth, third and fifth, respectively, in order of higher (slower) threshold values. However, the adjusted means in Table 15 also show a noticeable effect on these three categories associated with the removal of the differences in thresholds which correlate with the number of error responses. Thus, the mean threshold of the SS-MA verbs increased slightly (i.e., by .12) after adjustment for the covariate, and the mean threshold of the SA/MA verbs increased by .35. On the other hand, the mean threshold of the US-MA verbs decreased sharply (i.e., by 1.75) after adjustment for the covariate. Again, these results of the adjustment in the mean threshold values for the three verb categories reflect the differences among the stimulus categories in terms of their number of error responses, as shown in Table 13. Thus, for example, the adjusted mean threshold value for the US-MA verbs is less than the non-adjusted value, since the US-MA verbs had the highest number of error responses of all five stimulus categories—and a certain amount of this value of the covariate correlates with the fact that the US-MA verbs had by far the highest thresholds of all five stimulus categories in terms of the non-adjusted
threshold values.

Naturally, I was interested in comparing the threshold differences among the stimulus categories in terms of those threshold differences which did not correlate with the number of error responses, since—in line with the remarks made above (p. 172)—we cannot know whether the threshold differences which do correlate with the covariate were due to orthographic factors (underlying the number of error responses) or differences in terms of aspects of the number of arguments factor—or both.

Accordingly, I performed the subsequent t-tests on the threshold differences for the ten comparisons in (3-4) using the adjusted threshold values in Table 15 in accordance with standard practice (see, for example, Keppel 1973:493). The results of these t-tests are shown in Table 16.

Notice that the probability values given in Table 16 are for the "unprotected" t-test. In Chapter 2 (see pp. 96-7) I used a "protected" t-test—i.e., the Dunn test—for the reasons stated there. Of course, the same reasoning applies to the analysis of the present results. However, none of the comparisons shown in Table 16 were significant with the Dunn test—i.e., at the alpha level of .005 (= .05 divided by 10, the total number of comparisons in the experiment; see pp. 96-7 above). Comparison 7—MA nouns versus US-MA verbs—was significant with the Dunn test at the level of six comparisons; none of the other comparisons in Table 16 were significant at even the level of two comparisons with the Dunn test. These aspects of the present results will be discussed in section 3.3. ("General Discussion") below.
Table 16

Ten Comparisons of Thresholds Based on the Five Categories of Stimuli Used in the English Tachistoscopic Experiment

<table>
<thead>
<tr>
<th>threshold comparison</th>
<th>t value</th>
<th>df</th>
<th>probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) SA nouns vs. MA nouns</td>
<td>1.16</td>
<td>19</td>
<td>*NS</td>
</tr>
<tr>
<td>(2) SA nouns vs. SS-MA verbs</td>
<td>1.05</td>
<td>19</td>
<td>***NS</td>
</tr>
<tr>
<td>(3) SA nouns vs. SA/MA verbs</td>
<td>0.76</td>
<td>19</td>
<td>*NS</td>
</tr>
<tr>
<td>(4) SA nouns &lt; US-MA verbs</td>
<td>1.78</td>
<td>19</td>
<td>***NS (p&lt;.1)</td>
</tr>
<tr>
<td>(5) MA nouns &lt; SS-MA verbs</td>
<td>2.21</td>
<td>19</td>
<td>*p&lt;.05</td>
</tr>
<tr>
<td>(6) MA nouns &lt; SA/MA verbs</td>
<td>1.93</td>
<td>19</td>
<td>**NS (p&lt;.1)</td>
</tr>
<tr>
<td>(7) MA nouns &lt; US-MA verbs</td>
<td>2.95</td>
<td>19</td>
<td>*p&lt;.01</td>
</tr>
<tr>
<td>(8) SS-MA verbs vs. SA/MA verbs</td>
<td>0.74</td>
<td>19</td>
<td>**NS</td>
</tr>
<tr>
<td>(9) SS-MA verbs vs. US-MA verbs</td>
<td>0.28</td>
<td>19</td>
<td>***NS</td>
</tr>
<tr>
<td>(10) SA/MA verbs vs. US-MA verbs</td>
<td>1.02</td>
<td>19</td>
<td>**NS</td>
</tr>
</tbody>
</table>

* supports the minimum recall hypothesis
** supports the full recall hypothesis
*** agrees with both the minimum and the full recall hypotheses
**** is counter to the predictions made by both the minimum and the full recall hypotheses
The results shown in Table 16 can be paraphrased as follows. Recall that these results are for comparisons of threshold differences minus those differences which correlate with the number of error responses.

Comparisons 1, 2, 3, 8, 9, 10: No significant difference even with the less stringent, unprotected t-test.

Comparison 4: There was a trend (p < .1) with the unprotected t-test for the SA nouns to have lower (faster) thresholds than the US-MA verbs.

Comparison 5: Again with the unprotected t-test, the MA nouns had significantly (p < .05) lower thresholds than the SS-MA verbs.

Comparison 6: There was a trend (p < .1) with the unprotected t-test for the MA nouns to have lower thresholds than the SA/MA verbs.

Comparison 7: Again with the unprotected t-test, the MA nouns had significantly (p < .01) lower thresholds than the US-MA verbs.

The above results concerning the ten comparisons set out in (3-4) do not conform entirely to either the predictions made by Marshall et al.'s (1975) full recall hypothesis or my minimum recall hypothesis presented in Table 11. Taking the results shown in Table 16 at face value, the results for comparisons 1, 3, 5 and 7 (marked with a single asterisk in Table 16) support the minimum recall hypothesis. The results for comparisons 6, 8 and 10 (marked with two asterisks) support the full recall hypothesis. Comparisons 4 and 9 (marked with three asterisks) are in agreement with both the minimum and the full recall hypotheses (see p. 160). The result for comparison 2 (marked with four asterisks) was unpredicted by either the minimum or the full recall
hypothesis, which both predicted that SA nouns should have lower thresholds than SS-MA verbs (see p. 160).

Despite the mixed nature of the results described above with respect to the minimum and the full recall hypotheses, I will argue in section 3.3. ("General Discussion") below that these results do lean much more toward a confirmation of my hypothesis. In section 3.3. I will also discuss the failure of the above results to achieve significance with the Dunn test, especially in the light of the results of the familiarity questionnaire to be discussed next.

3.2. Post-Test on the Subjective Familiarity of the English Stimuli

I had originally planned to use a very large group of speakers of standard American English to rank items from a long list of prospective stimuli for use in the experiment. If this plan had worked out, I would have selected words from the list of prospective stimuli in each of the five stimulus categories which were balanced both in terms of the subjective rankings of this large sample of English speakers and the Thorndike and Lorge (1944) word count.

However, the above plan failed for two reasons. First, there simply was not a population of speakers of standard United States mainland English large enough and readily available in Hawaii for my purpose. More importantly, due to the restrictions posed by the orthographic considerations discussed in section 3.1.2.3. above, I did not have a very large number of potentially suitable stimuli to work with in constructing the five different categories of stimuli. That is, to a considerable extent, I was forced to go with the words I had been able
to find that were suitable both in terms of orthographic considerations and balanced for frequency in terms of Thorndike and Lorge (1944). The situation in the Japanese experiment was similar.

Therefore, I administered a questionnaire to the 20 subjects used in the English experiment asking them to rank the stimuli according to subjective familiarity approximately four to five months after the experiment. By this time none of the subjects were able to recall more than one or two words of the stimuli used in the experiment.

The rationale for the post-test questionnaire on the subjective familiarity of the English stimuli was the same as that outlined in Chapter 2 (see pp. 99ff.) for the Japanese stimuli. For the most part, the procedure was also the same, with the following exceptions.

The scale used was from 1 to 7, with "1" standing for the least familiar items and "7" for the most familiar. In addition, a zero was also provided on the scale in the event that a subject felt he or she had not seen or heard a particular stimulus item before, but none of the 20 subjects made use of it.

The order of the stimuli on the response sheet was completely randomized separately for each subject. There was no other identification of the stimuli—e.g., numbers—on the response sheet. The subjects were asked to rank each stimulus according to the scale on its own, in contrast to the familiarity questionnaire discussed in Chapter 2 for the Japanese stimuli.

The sums of the rankings for the five categories of stimuli for each subject were used in the statistical analysis of this questionnaire data. The total sum of the rankings for the 15 stimuli in each category
for all 20 subjects can be seen below in Table 17. Please recall that lower numbers indicate items rated less familiar, and higher numbers indicate items rated more familiar.

Table 17
Total Familiarity Rankings for the English Tachistoscopic Stimuli across All Subjects and All Items in Each Category

<table>
<thead>
<tr>
<th>Category</th>
<th>SA nouns</th>
<th>MA nouns</th>
</tr>
</thead>
<tbody>
<tr>
<td>SA nouns</td>
<td>1223</td>
<td>1110</td>
</tr>
<tr>
<td>SS-MA verbs</td>
<td>1198</td>
<td>1110</td>
</tr>
<tr>
<td>SA/MA verbs</td>
<td>1340</td>
<td>1335</td>
</tr>
<tr>
<td>US-MA verbs</td>
<td>1335</td>
<td>1335</td>
</tr>
</tbody>
</table>

Wilcoxon matched-pairs signed ranks tests were used to analyze the familiarity rankings by the 20 subjects according to the ten comparisons indicated in (3-4) to determine whether any of these showed a statistically significant difference. The results are shown in Table 18, where "<" and ">" signify "rated less familiar than" and "rated more familiar than," respectively.

Table 18 shows that the SA nouns were rated more familiar than the MA nouns and that, in general, the verbs were rated more familiar than the nouns—with the exception of the SA nouns versus the SS-MA verbs. The results in Table 18 can be paraphrased as follows. (Please refer to the totals in Table 17).

Comparison 1: The SA nouns were rated significantly more familiar than the MA nouns, p < .01 with the two-tailed test.
Table 18
Ten Comparisons of the Familiarity Rankings for the Five Categories of Stimuli Used in the English Tachistoscopic Experiment

<table>
<thead>
<tr>
<th>familiarity comparison</th>
<th>T value</th>
<th>N showing a difference</th>
<th>two-tailed probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) SA nouns &gt; MA nouns</td>
<td>25.5</td>
<td>20</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(2) SA nouns vs. SS-MA verbs</td>
<td>60.5</td>
<td>17</td>
<td>NS</td>
</tr>
<tr>
<td>(3) SA nouns &lt; SA/MA verbs</td>
<td>19.5</td>
<td>19</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(4) SA nouns &lt; US-MA verbs</td>
<td>32.5</td>
<td>20</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(5) MA nouns &lt; SS/MA verbs</td>
<td>37</td>
<td>19</td>
<td>p &lt; .02</td>
</tr>
<tr>
<td>(6) MA nouns &lt; SA/MA verbs</td>
<td>1</td>
<td>19</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(7) MA nouns &lt; US-MA verbs</td>
<td>1.5</td>
<td>20</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(8) SS-MA verbs &lt; SA/MA verbs</td>
<td>13</td>
<td>20</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(9) SS-MA verbs &lt; US-MA verbs</td>
<td>13</td>
<td>20</td>
<td>p &lt; .01</td>
</tr>
<tr>
<td>(10) SA/MA verbs vs. US-MA verbs</td>
<td>73</td>
<td>19</td>
<td>NS</td>
</tr>
</tbody>
</table>
Comparison 2: There was no statistically significant difference between the familiarity ratings of the SA nouns and those of the SS-MA verbs.

Comparison 3: The SA/MA verbs were rated significantly more familiar than the SA nouns, p < .01 with the two-tailed test.

Comparison 4: The US-MA verbs were rated significantly more familiar than the SA nouns, p < .01 with the two-tailed test.

Comparison 5: The SS-MA verbs were rated significantly more familiar than the MA nouns, p < .02 with the two-tailed test.

Comparison 6: The SA/MA verbs were rated significantly more familiar than the MA nouns, p < .01 with the two-tailed test.

Comparison 7: The US-MA verbs were rated significantly more familiar than the MA nouns, p < .01 with the two-tailed test.

Comparison 8: The SA/MA verbs were rated significantly more familiar than the SS-MA verbs, p < .01 with the two-tailed test.

Comparison 9: The US-MA verbs were rated significantly more familiar than the SS-MA verbs, p < .01 with the two-tailed test.

Comparison 10: There was no statistically significant difference between the familiarity ratings of the SA/MA verbs and those of the US-MA verbs.

As in Chapter 2, the above results of the post-test familiarity questionnaire are at odds with the word count (see p. 167) which was used to balance the five stimulus categories for frequency. These familiarity results are both helpful and problematic for both Marshall et al.'s (1975) full recall hypothesis and my minimum recall hypothesis in terms of the tachistoscopic results discussed above. These points
will be discussed below.

3.3. General Discussion of the English Data in Terms of Competing Theories of Noun Facilitation

The relative lack of significant differences for the comparisons in Table 16—especially with the stringency of the Dunn procedure—naturally results in less that can be said about these results and their relevance for theories of noun facilitation. This was not the case with the Japanese experiment of Chapter 2, where the differences obtained—some supporting Marshall et al.'s hypothesis and some supporting my minimum recall hypothesis—were clearly statistically significant.

However, there are still important things which can be said about the results of the English experiment with respect to the role of number of arguments in tachistoscopic noun facilitation. Moreover, what can be said on the basis of these results supports the minimum recall hypothesis over Marshall et al.'s (1975) full recall hypothesis, as I will try to show.

I will first discuss the results—in terms of recognition thresholds, number of error responses and familiarity ratings—for each of the comparisons set out in (3-4) and how they relate to the full recall hypothesis and the minimum recall hypothesis. I will then conclude with some general remarks on the data presented in this chapter. Please refer to the composite table (Table 19) below, in addition to Tables 16 (p. 181) and 18 (p. 186), throughout the following discussion.

Comparison 1: As Table 16 indicates, there was no significant difference between the SA and MA nouns in terms of thresholds when the threshold differences which correlated with the differences between the
Table 19
Composite Table of the Data from the English Tachistoscopic Experiment and the Familiarity Questionnaire

Tachistoscopic Threshold Totals (from Table 12, p. 148)

<table>
<thead>
<tr>
<th></th>
<th>SA nouns</th>
<th>MA nouns</th>
<th>SS-MA verbs</th>
<th>SA/MA verbs</th>
<th>US-MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2057</td>
<td>2109</td>
<td>2119</td>
<td>2110</td>
<td>2168</td>
</tr>
</tbody>
</table>

Tachistoscopic Error Response Totals (from Table 13, p. 148)

<p>| | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>157</td>
<td>234</td>
<td>207</td>
<td>202</td>
<td>248</td>
</tr>
</tbody>
</table>

Non-Adjusted Mean Recognition Thresholds (from Table 15, p. 152)

<p>| | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>102.85</td>
<td>105.45</td>
<td>105.95</td>
<td>105.50</td>
<td>108.40</td>
</tr>
</tbody>
</table>

Adjusted Mean Recognition Thresholds (from Table 15, p. 152)

<p>| | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>105.25</td>
<td>104.34</td>
<td>106.07</td>
<td>105.85</td>
<td>106.65</td>
</tr>
</tbody>
</table>

Familiarity Ranking Totals (from Table 17, p. 158)*

<p>| | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1223</td>
<td>1110</td>
<td>1198</td>
<td>1340</td>
<td>1335</td>
</tr>
</tbody>
</table>

* Higher numbers represent items rated more familiar.
SA and MA nouns in terms of number of error responses were removed. This result agrees with the claim made by the minimum recall hypothesis (see Table 11, p. 160) that SA and MA nouns should not differ in terms of recognition thresholds since both types of nouns can be used in grammatical single argument sentences.

Table 18 also indicates that the SA nouns were rated significantly (p < .01) more familiar than the MA nouns by the 20 subjects of the tachistoscopic experiment. This difference in familiarity could have easily contributed to the fact that the non-adjusted thresholds of the SA nouns (see Table 19) were lower (faster) than those of the MA nouns—which, in turn, had lower thresholds than any of the three categories of verb stimuli.

It should be admitted, however, in line with the remarks made above (p. 172), that the English data with respect to comparison 1 are not a conclusive disconfirmation of Marshall et al.'s claim that SA nouns should have lower thresholds than MA nouns, since in this case the threshold values and the number of error responses parallel each other. That is, Marshall et al. could claim that the SA nouns had fewer error responses due to their greater ease of recognition, and not vice versa, which is what I would argue in support of the minimum recall hypothesis. That is, I would claim that the lower non-adjusted thresholds of the SA nouns relative to the MA nouns were due to their having been less easily confused orthographically with other words in the language.

Comparison 2: Both the minimum recall hypothesis and the full recall hypothesis predicted lower thresholds for the SA nouns than for the SS-MA verbs. The results in Table 16, however, show no
significant difference between these categories in terms of adjusted threshold values, although the observed (non-significant) difference was in the direction predicted by both Marshall et al. and myself.

The familiarity rankings cannot be used to explain this lack of statistical significance since the SA nouns and the SS-MA verbs did not show a statistically significant difference in this regard (see Table 16 and Table 19), and the observed (non-significant) difference was even in the direction of SA nouns being rated more familiar than SS-MA verbs.

Marshall et al. and I could argue—to save our predictions—that the adjustment made by the statistical analysis (section 3.1.3) went too far in correcting for the effect of the covariate. That is, at least part of the lower number of error responses for the SA nouns might have been due to their being more easily recognized (and not vice versa) than the SS-MA verbs. This is precisely the argument that Marshall et al. could make (as noted above, p. 189) to explain the lack of a threshold difference between SA and MA nouns. Thus, in keeping with the minimum recall hypothesis, I have to argue that the statistical analysis over-corrected for the effect of the covariate enough to obscure the difference which should have appeared between the SA noun thresholds and those of the SS-MA verbs, but not to the point of obscuring a threshold difference between the SA and MA nouns which Marshall et al. might like to claim.

Comparison 3: Table 16 indicates that there was no significant difference between the thresholds of the SA nouns and the SA/MA verbs when the effect of the covariate (i.e., error responses) was removed. This follows the prediction of the minimum recall hypothesis and runs
counter to the prediction of higher thresholds for the SA/MA verbs which would be expected from the full recall hypothesis. Recall that the minimum recall hypothesis predicted no difference in terms of threshold values between SA nouns and SA/MA verbs since the latter can be used in grammatical single argument sentences even in the absence of shared information.

Here, however, Marshall et al. have two possible explanations available to them: 1) the one noted above regarding comparison 1 (p. 189); and 2) an explanation based on the familiarity ratings of the stimuli. That is, Table 18 shows that the subjects rated the SA/MA verbs significantly (p < .01) more familiar than the SA nouns. Marshall et al. could thus argue that the SA/MA verbs failed to have higher thresholds than the SA nouns because the effect of number of arguments was obviated by the greater familiarity of the SA/MA verbs.

Comparison 4: Both the minimum recall hypothesis and the full recall hypothesis predicted lower thresholds for the SA nouns than for the US-MA verbs. Recall that the minimum recall hypothesis claims that the arguments of US-MA verbs are obligatory (at the level of underlying structure) even though they can be used in grammatical single argument sentences, since these sentences are only possible in limited conversational contexts involving shared information.

Table 16 (p. 181) shows that there was a trend (p < .1) for the SA nouns to have lower thresholds than the US-MA verbs with the unprotected t-test when the effect of the number of error responses was removed. This result is weak support for the predictions made by the minimum recall hypothesis and the full recall hypothesis.
Here again, one could attribute the failure of comparison 4 to show a statistically significant difference in terms of thresholds to an overcorrection for the effect of the covariate on the threshold values of the SA nouns (see p. 189 above).

On the other hand, one could use the significantly higher familiarity ratings of the US-MA verbs relative to the SA nouns (p < .01; see Table 18) to explain why there was not a significant difference between the thresholds of the SA nouns and the US-MA verbs. This raises an interesting question with respect to comparison 3 discussed above. Namely, Table 18 (p. 186) shows that the SA/MA and the US-MA verbs did not differ in terms of familiarity ratings. Thus, if Marshall et al. use the familiarity ratings of the SA/MA verbs to explain the lack of a significant difference between the thresholds of the SA nouns and those of the SA/MA verbs (p. 192 above), it would seem that the US-MA verbs should have differed from the SA nouns in terms of threshold values by roughly the same amount as that found between the thresholds of the SA/MA verbs and the SA nouns—since the SA/MA and the US-MA verbs did not differ in number of arguments or familiarity ratings. However, Table 16 shows a t value of .76 (=NS) for comparison 3 and a t value of 1.78 (p < .1) for comparison 4.

Comparison 5: This is the second clearest result from the English experiment, and it argues in favor of the minimum recall hypothesis. Recall that Marshall et al.'s full recall hypothesis predicted no difference between the thresholds of the MA nouns and those of the SS-MA verbs. The minimum recall hypothesis predicted lower thresholds for the MA nouns since they can be used in grammatical single argument sentences.
The results in Table 16 show that the MA nouns had significantly lower thresholds (p < .05), unprotected t-test) than the SS-MA verbs when the effect of the covariate was removed. Even with the non-adjusted threshold values, the MA nouns had a slightly lower threshold total across the 20 subjects than the SS-MA verbs—despite the fact that the MA nouns also had a higher number of error responses than the SS-MA verbs (see Table 19).

This result of lower thresholds for the MA nouns than for the SS-MA verbs is made all the stronger since the SS-MA verbs were rated significantly (p < .02) more familiar than the MA nouns (Table 18).

Comparison 6: The result of this comparison is problematic for both the full recall hypothesis and the minimum recall hypothesis. Both of these theories predicted no difference between the MA nouns and the SA/MA verbs, but for different reasons (see p. 159 above). However, the results in Table 16 indicate a trend (p < .1) for the MA nouns to have lower thresholds than the SA/MA verbs when the effect of the covariate was removed. This trend gains greater significance from the fact that the SA/MA verbs were rated significantly (p < .01) more familiar than the MA nouns (Table 18, p. 186).

The result of this comparison is also weak evidence for some sort of overcorrection with respect to the adjusted threshold values of the SA nouns. Recall that Table 16 (p. 181) shows no significant difference (t = .76, df = 19) between the SA nouns and the SA/MA verbs in terms of thresholds, even though there was less of a difference between the SA nouns and the SA/MA verbs in terms of their respective familiarity ratings than between the MA nouns and the SA/MA verbs (see Table 17, p. 185).
Comparison 7: This comparison showed the clearest result from the English experiment, and it argues strongly in favor of the minimum recall hypothesis. Recall that Marshall et al.'s full recall hypothesis predicted no difference between the thresholds of the MA nouns and the US-MA verbs. The minimum recall hypothesis predicted lower thresholds for the MA nouns since they can be used in grammatical single argument sentences.

The results in Table 16 show that the MA nouns had significantly lower thresholds (p < .01, unprotected t-test) than the US-MA verbs when the effect of the covariate was removed. This result gains greater significance from the fact that the US-MA verbs were rated significantly (p < .01) more familiar than the MA nouns. In fact, the difference between the MA nouns and the US-MA verbs in terms of the familiarity ratings was the largest of any of the ten pair-wise comparisons of the five stimulus categories.

Comparison 7 is also the only comparison in the English experiment to reach any level of significance with the Dunn test. The obtained t for this comparison was 2.95, which is significant at the level of six comparisons with the Dunn test (3.16 required for significance at the level of ten comparisons). I will return to the issue of the level of the significance of this comparison below.

Comparisons 8 and 10: Table 15 shows no statistically significant difference between either the SS-MA verbs and the SA/MA verbs or between the SA/MA verbs and the US-MA verbs. This result is what was predicted by the strongest version of the full recall hypothesis (see p. 160 above). Here, the familiarity ratings are not of much help to
the minimum recall hypothesis since the SA/MA verbs were rated significantly (p < .01) *more* familiar than the SS-MA verbs and were equal in terms of their familiarity ratings to the US-MA verbs (see Table 18).

Comparison 9: Table 16 indicates that there was **no** significant difference between the adjusted thresholds of the SS-MA verbs and those of the US-MA verbs. This result was predicted by both the minimum recall hypothesis and the full recall hypothesis. Recall that the minimum recall hypothesis does **not** predict any facilitation for US-MA verbs despite the fact that these verbs can sometimes be used in single argument sentences, since these sentences are only possible in certain limited conversational contexts of shared information. The minimum recall hypothesis assumes that the arguments of US-MA verbs are obligatory at the level of underlying structure.

The above result lends some cross-linguistic support to the assumptions made in Chapter 2 (pp. 86-7) about the Japanese MA verb stimuli having obligatory arguments at the level of underlying structure, since the English US-MA verbs behave like the Japanese MA verb stimuli in Appendix 1 in permitting ellipsis of arguments in cases of shared information and since they were no easier to recognize in the English experiment than the SS-MA verbs.

The above discussion of the results of the English experiment can be summarized as follows.

There is fairly strong evidence for a facilitation effect for MA nouns relative to SS-MA verbs and US-MA verbs, and especially the latter.
(I feel this conclusion is warranted despite the failure of either of these comparisons to attain significance with the Dunn test since both of these verb categories were rated by the subjects as being more familiar than the MA nouns.) On the other hand, there is no convincing evidence that the factor of number of arguments affects tachistoscopic recognition thresholds among nouns. That is, the English data discussed above offer no confirmation of Marshall et al.'s claim that SA nouns should have lower thresholds than MA nouns. (This point is not conclusive since—as discussed under comparison 1 above (p. 189)—the SA nouns did have lower non-adjusted thresholds than the MA nouns, and this difference between the SA and MA nouns only disappears with the elimination of the effect associated with the covariate.)

The two points just made support the minimum recall hypothesis and tend to disconfirm the full recall hypothesis. However, one final conclusion from the English data leans more toward a confirmation of at least one of the predictions made by the full recall hypothesis. Namely, there was no evidence for a facilitation effect for SA/MA verbs relative to SS-MA and US-MA verbs as predicted by the minimum recall hypothesis. Of course, there was also no convincing evidence for a facilitation effect for SA nouns relative to SA/MA verbs (comparison 3, pp. 191-2), but in this case Marshall et al. could claim that the greater familiarity of the SA/MA verbs cancelled out this effect that their theory had predicted. (They could also argue as in the third paragraph on p. 189 above.) It is perhaps ironic that the MA nouns in the English experiment—which Marshall et al. claim should not differ in terms of recognition thresholds from MA verbs—came closer than the SA nouns to showing a
facilitation effect (p < .1) vis-à-vis the SA/MA verbs! (Here again, the SA/MA verbs were also rated more familiar than the MA nouns.)

As can be seen from the list of English stimuli in Appendix 2, seven of the SA/MA verbs (marked with an asterisk in Appendix 2) were like "grow" in that when functioning as SA verbs they are also semantically intransitive, whereas the other eight were like "write" in that when functioning as SA verbs they are still semantically transitive — i.e., they have understood objects (see p. 158). Here it might be suggested that the somewhat ambiguous status of the SA/MA verbs in the results of the English experiment was due to this mixing of verbs like "grow" and "write" in the SA/MA category. Under this approach, one might suggest that verbs like "grow" would not differ from SA nouns in terms of recognition difficulty, but that verbs like "write" would act like SS-MA and US-MA verbs. However, as attractive as this proposal is, it was not confirmed by a re-examination of the data for the 15 items in the SA/MA verb category.

From the above it would appear that the predictions of the minimum recall hypothesis (see Table 11, p. 160) with regards to SA/MA verbs might not be correct. This does not mean, however, that the full recall hypothesis is necessarily correct in this regard. For one thing, one could take the kind of approach indicated on p.148 of Chapter 2 to argue that SA/MA verbs should have recognition thresholds somewhere between SS-MA or US-MA verbs and nouns. This is clearly a matter for future research.

I will return to the possible reasons for the threshold results for the SA/MA verbs in section 4.1.1. of Chapter 4.
In conclusion, I feel that the primary reason that the results of the experiment discussed in this chapter were not clearer was the "noise" introduced into the experiment by the orthographic factors discussed in section 3.1.2.3. above and the resulting "orthographic" guessing strategy on the part of the subjects. This can be seen in comparison 2, for example, where there is some possibility that the statistical analysis used in this chapter overcorrected for the effect of the covariate (see pp. 189, 191 and 194).

In applying measures of added stringency--like the Dunn test--when attempting to determine statistical significance one is often faced with a dilemma: the possibility of being too conservative and thus ignoring trends that with further testing may turn out to be significant versus being too liberal and thus being overly confident in obtained differences that may not replicate. It was in the spirit of looking for "trends worthy of further efforts at confirming or disconfirming" that I attempted the above discussion and will conclude this dissertation in Chapter 4.

3.4. Concluding Remarks

Despite the partially equivocal nature of the results discussed in section 3.3. above, one thing is clear from the Japanese data of Chapter 2 and the English data of this chapter: Optionality of arguments appears to affect the role of number of arguments in noun facilitation phenomena. This is particularly clear in the case of the facilitation found for MA nouns relative to MA verbs in both the Japanese and the English experiments.
The relevance of this and other findings from the first three chapters to theoretical syntax, other psycholinguistic research and issues in neuropsychology will be discussed in Chapter 4.

Chapter 4 will conclude with a brief outline of areas for future research on the role of number of arguments in noun facilitation phenomena which have been suggested by this study.
CHAPTER 4.
CONCLUSION: NOUN FACILITATION AND THE WIDER RESEARCH CONTEXT

4.0. Introduction

In this chapter I will discuss the data presented in Chapters 2 and 3 from several points of view.

Section 4.1. will be devoted to a discussion of the role of number of arguments in noun facilitation phenomena. Section 4.1.1. will review the relevance of the data in the preceding chapters for the minimum recall hypothesis versus Marshall et al.'s (1975) full recall hypothesis. In section 4.1.2. I will evaluate the minimum recall hypothesis as an explanation of noun facilitation phenomena in general.

In section 4.2. I will discuss the relevance of noun facilitation phenomena to other areas of research in linguistics and psychology. Section 4.2.1. will deal with the relevance of noun facilitation phenomena for current psycholinguistic research on syntactic processing strategies. Section 4.2.2. will present some implications of the data in Chapters 2 and 3 for formal theories of syntax. In section 4.2.3. I will discuss the relevance of my data to issues in neuropsychology.

Finally, section 4.3. will deal with the issues requiring further investigation—in experimental studies with adults, language acquisition and linguistic breakdown due to brain damage—in order for our understanding of the causation of noun facilitation phenomena to be advanced.
4.1. Number of Arguments and an Explanatory Theory of Noun Facilitation

4.1.1. The Minimum Recall Hypothesis versus the Full Recall Hypothesis

I feel that the data discussed in the preceding chapters supports the minimum recall hypothesis over Marshall et al.'s (1975) full recall hypothesis for the reasons I will present. First, to review:

Recall that the minimum recall hypothesis says simply that a lexical item will be recalled from LTM—in tasks involving little or no syntactic computation—with the minimum number of argument features required in order to use that item in a grammatical sentence. Since at least many MA nouns in Japanese and English can be used in single-argument constructions such as (2-6), the minimum recall hypothesis predicts that they can be recalled from LTM—again, in tasks involving a minimum of syntactic processing—with no more argument features than SA nouns. For verbs, the prediction made by the minimum recall hypothesis with respect to processing complexity depends on whether the associated arguments are optional or obligatory. As argued in Chapter 3, SS-MA verbs (e.g., "put") and US-MA (or "zero morph") verbs (e.g., "listen") have obligatory multiple arguments (at least at the level of underlying structure) as shown by the ungrammaticality of (2-7a-c) and (3-2b and c) without shared information. Of course, the above summary of the claims made by the minimum recall hypothesis are not meant to apply to situations requiring complex syntactic computation, such as in the test sentences used in Fodor et al. (1968) (see p. 77 above). In such situations lexical items may indeed be recalled from
LTM with a variety of optional arguments as part of an overall parsing strategy. (More on this in section 4.3.1. below.)

Recall also that the full recall hypothesis—on the other hand—predicts that a lexical item will be recalled from LTM with "the full lexical entry associated with the particular [lexical item]" (Marshall et al. 1975:321). In putting forth this hypothesis, Marshal et al. do not offer any qualifications based on optionality of arguments or different types of task situations.

The full recall hypothesis as stated by Marshall et al. is not explicit with respect to what constitutes a "full lexical entry." On a priori grounds, Marshall et al.'s theory seems improbable as stated. For example, it seems to predict that lexical items—even in isolation—will be recalled from LTM along with such optional subcategorization frames as adjectives and adverbs. This clearly brings us back to the problem discussed in Chapter 1 (pp. 67ff.) with regards to the criteria according to which we can assign a given number of arguments to a lexical item. Of course, once we begin to eliminate optional subcategorization frames from the arguments which our theory predicts must be recalled from LTM with a given lexical item, what is to prevent us from adopting the minimum recall hypothesis with regard to MA nouns and verbs? In other words, where do we draw the line in excluding optional arguments from those that our theory predicts must be pulled from LTM with the recall of a lexical item?

Besides the a priori considerations just noted, I feel that the available psycho- and neurolinguistic data—contained in this
dissertation and elsewhere--support the minimum recall hypothesis over Marshall et al.'s full recall hypothesis.

First, the minimum recall hypothesis is superior to the full recall hypothesis since it can be used to explain the crucial case of the nominalizations often made by brain-damaged patients who show some type of noun facilitation effect. As noted (pp. 65, 82), such nominalizations frequently leave the number of possible arguments unchanged, as in "admirer" substituted for "admire." The sentences in (4-1) below show that the number of possible arguments is preserved in such nominalizations.

(4-1)  a. John admires fine old cabernet.
    b. John is an admirer of fine old cabernet.

Other examples of nominalizations which do not change the number of arguments can be found on p. 20 above (from Whitaker 1972). OH's nominalizations (see (2-12)--p. 122--and (2-13)--p. 124) also fall into this category, as do the substitutions in "-ing" cited in Whitaker (1972:67-8). Examples of these are shown below in (4-2).

(4-2)    target    response
    eat         eating
    wear        wearing
    believe     believing (loc. cit.)

As stated on p. 82, the minimum recall hypothesis can account for the greater ease of such nominalizations for patients who show a noun facilitation effect in a natural way: Such nominalizations have the effect of changing obligatory arguments to optional ones, as shown in (4-3) below in the case of "admirer."

(4-3)  Unwanted admirers can be a pain in the neck.
Note that "admire" is an SS-MA verb, as shown by the ungrammaticality of (4-4b)—even with shared information.

(4-4)  

a. How do you feel about Mondavi's 1974 cabernet?  
b. *I admire. (meaning "I admire it")

Thus, the minimum recall hypothesis claims that derivationally related nominals (like "admirer") are easier for brain-damaged persons who show a noun facilitation effect than their related source verb ("admire"), since the former can be used in grammatical sentences with fewer arguments than the latter. That is, brain-damaged persons will not be able to use lexical items which can only be used in sentences that they are not capable of constructing.

The case of substitutions in "-ing" can be explained along the lines discussed above, although there are complications. That is, there is some variation in the number of arguments that become optional with the substitution of an "-ing" form for different types of verbs. For example, "wearing" (see (4-2)) must be used in sentences with at least a direct object argument as (4-5a and b) show.

(4-5)  

a. Wearing clothes is a pain in the neck.  
b. *Wearing is a pain in the neck.

Here it might be thought that only the "-ing" forms of SA or SA/MA verbs—and not SS-MA or US-MA verbs—lend themselves to use in sentences like (4-6) where the nominal appears with no arguments.

(4-6)  

Hunting is forbidden in the city.

"Hunt" is, of course, an SA/MA verb. However, there are SS-MA and US-MA verbs which have a corresponding "-ing" form which can be used in sentences similar to (4-6). "Warn" is one such SS-MA verb as (4-7a-c) show.
(4-7)  

a. Did you warn Tom?
b. *Yes, I warned. (meaning "I warned him")

but

c. Warnings offered with a smile are somehow sinister.

Here the distinction appears to be between gerundive nominals (e.g., "wearing") and derived nominals in "-ing" (e.g., "warning"). That is, the "-ing" forms of SS-MA and US-MA verbs that are independently lexicalized as nouns (as in the case of "warning") can be used in sentences without arguments such as (4-6) and (4-7c). I will have more to say on this matter in section 4.3.2. below.

In any case, even SS-MA and US-MA gerundive nominals (like "wearing") can be used with one less argument—i.e., the subject argument—than their corresponding root verb. This may be enough to cause some facilitation for gerundive nominals like "wearing" versus their root form on the part of brain-damaged persons who show a noun facilitation effect. This much follows the predictions of the minimum recall hypothesis. On the other hand, the minimum recall hypothesis would predict a greater facilitation for derived nominals (like "warning") than for gerundive nominals (like "wearing") on the part of patients showing noun facilitation, since all of the arguments of the derived nominals are optional (as shown in (4-7c)) whereas only the subject argument is optional in the case of gerundive nominals like "wearing" (as shown in (4-5)). This prediction is an interesting one for future research with brain-damaged persons who show a noun facilitation effect.

So far, I have discussed a priori considerations against the full recall hypothesis and how the case of nominalizations by brain-damaged
persons supports the minimum recall hypothesis over the full recall hypothesis. The other data presented throughout this dissertation also lean heavily toward the same conclusion.

First, consider the results of the tachistoscopic experiment using Japanese stimuli discussed in Chapter 2. As stated in section 2.4. (p. 127), the results of this experiment partially support and partially contradict both the full recall hypothesis and the minimum recall hypothesis when taken at face value. That is, the lower thresholds for the MA nouns relative to the MA verbs—even with the protected t-test—support the minimum recall hypothesis and disconfirm the full recall hypothesis. On the other hand, the lower thresholds for the SA nouns and verbs relative to both the MA nouns and verbs seem to support the idea of full recall at the expense of the minimum recall hypothesis. The bulk of section 2.4. was devoted to the pros and cons of attempts to explain away the lower thresholds of the MA nouns relative to the MA verbs (thus arguing in favor of the full recall hypothesis)—or to explain away the lower thresholds of the SA items relative to the MA nouns (thus arguing in favor of the minimum recall hypothesis). (Recall that the lower thresholds of the SA items relative to the MA verbs pose no problem for the minimum recall hypothesis since both it and the full recall hypothesis predict lower thresholds for SA items than for MA verbs on the basis of number of arguments.) These arguments are multi-faceted—reflecting the inherent complexity of this type of research, but I have argued that there is a much better case for explaining away the lower thresholds of the SA items relative to the MA items, and thus arguing in favor of the minimum recall hypothesis.
Thus, the three main arguments of summated frequency, katakana expectations and category ambiguity which might be used in an attempt to explain away the lower thresholds of the MA nouns relative to the MA verbs were each shown to have serious inadequacies (see pp. 130-3). However, the most serious argument against each of these possible attempts to explain away the lower thresholds of the MA nouns relative to the MA verbs was that if any or all of them were the basis of the lower thresholds of the MA nouns they should have also resulted in lower thresholds for the SA nouns relative to the SA verbs (see p. 133).

On the other hand, the lower thresholds of the SA items relative to the MA nouns in the Japanese experiment can be easily explained on the basis of the results of the familiarity questionnaire discussed in section 2.2. of Chapter 2. That is, the SA nouns and verbs were both rated more familiar than the MA nouns by the 20 subjects of the tachistoscopic experiment.

Therefore, since the lower thresholds of the SA items relative to the MA nouns are easily explained on the basis of subjective familiarity and since the lower thresholds of the MA nouns relative to the MA verbs can not be explained away, it was concluded in Chapter 2 that the results of the Japanese tachistoscopic experiment support the minimum recall hypothesis and disconfirm the predictions of the full recall hypothesis.

In addition, OH's results on the two administrations of the reading aloud test utilizing the tachistoscopic stimuli also support the minimum recall interpretation of the tachistoscopic results discussed above. (Of course, this assumes—as a hypothesis—that the order of the breakdown of linguistic functions due to brain damage parallels the
order of difficulty in experiments--such as tachistoscopic studies--
with normal adults.) The relative difficulty among the four categories
of stimuli for OH--SA nouns and verbs, MA nouns and verbs--conforms
exactly to the predictions of the minimum recall hypothesis. Thus, OH's
results are a strong confirmation of the minimum recall hypothesis and a
strong disconfirmation of the full recall hypothesis. In both testing
sessions--separated by two months--OH did approximately equally well
on the SA nouns and verbs and the MA nouns. His performance on the MA
verbs, however, was much worse than his performance on any of the other
three stimulus categories in both testing sessions. On the second testing
session, OH read at least 12 of the 15 stimuli in the categories of SA
nouns and verbs and MA nouns, but only 5 of the MA verbs.

Note that OH's high performance on the MA nouns--in fact, equal to
his performance on the SA verbs and nearly equal to his performance on
the SA nouns--in both testing sessions was in spite of the fact that
these nouns were rated by the 20 tachistoscopic subjects significantly
less familiar than the SA items. This finding fits with the fact that
OH's Z-score profile (p. 116) does not show an impairment for low
frequency words relative to high frequency words in either auditory
recognition or oral expression. Thus, OH's performance on the reading
aloud tests can be taken as the result of a syntactic impairment, largely
independent of any frequency effect, which made it more difficult for
him to read aloud lexical items--i.e., MA verbs--which can only be used
in the type of complex multiple argument sentences that he was unable
to produce.
So much for the Japanese data of Chapter 2 and its relevance to the full recall hypothesis and the minimum recall hypothesis.

The English data of Chapter 3 are just as interesting—if not as conclusive—as those discussed above for an understanding of noun facilitation. The tachistoscopic experiment in Chapter 3 basically had two purposes. The first was to check on the recognition difficulty of MA nouns relative to SA nouns, and the relative difficulty of both of these to several types of verbs. The second purpose was to examine the recognition difficulty of several categories of verbs—i.e., SS-MA verbs, SA/MA verbs and US-MA verbs—relative to each other.

Regarding the first purpose noted above, the results lean towards the minimum recall hypothesis, as I will now review.

The non-adjusted threshold means for the five categories of stimuli—SA and MA nouns and SS-MA, SA/MA and US-MA verbs—were lowest for the SA nouns. The non-adjusted threshold mean of the SA nouns (102.85) was lower than that of the MA nouns (105.45) (see Table 15). So much seems to support the full recall hypothesis which predicts SA nouns will have lower thresholds than MA nouns.

However, it was argued (pp. 171-4) that the number of error responses made by the subjects in each stimulus category should be taken into account in the statistical analysis as a check on the kind of orthographic factors which seemed to form the basis of the subjects' guessing strategy (see p. 164). When this was done, the relative values of the threshold means among the five categories of stimuli changed somewhat.
Thus, the adjusted threshold mean of the SA nouns (105.25) was (nonsignificantly) higher than that of the MA nouns (104.34). This agrees with the minimum recall hypothesis, which predicts no significant difference between the thresholds of SA and MA nouns, since both can be used in single-argument sentences.

As Table 16 shows, the MA nouns also had a lower adjusted mean threshold than either the SS-MA verbs ($p < .05$, unprotected t-test) or the US-MA verbs ($p < .01$, unprotected t-test), despite the fact that both the SS-MA and the US-MA verbs were rated by the subjects as more familiar than the MA nouns. This again agrees with the minimum recall hypothesis and runs counter to the full recall hypothesis, which predicts no difference between the thresholds of SS-MA and US-MA verbs and MA nouns (assuming the number of at least potential arguments is held constant across these categories). Strangely, there was no significant difference between the adjusted mean threshold of the SA nouns and that of any of the verb categories—as Table 16 shows—with the exception of a trend for the SA nouns to have lower thresholds than the US-MA verbs ($p < .1$, unprotected t-test). This result is strange since it runs counter to both the full recall hypothesis and the minimum recall hypothesis and previous noun facilitation experiments in the literature. In Chapter 3 (pp. 189, 194) it was suggested that the analysis of variance with covariance may have gone too far in adjusting for the effect of the number of error responses on the thresholds of the SA nouns. That is, at least part of the lower number of error responses for the SA nouns might have been due to their being easily recognized, rather than to low orthographic confusability. Thus, it is possible to
claim—in line with the minimum recall hypothesis—that if only the
effect of the small number of error responses for the SA nouns due to
low orthographic confusability could have been removed from the SA nouns'
threshold mean, then the SA nouns would have had an adjusted threshold
mean equal to that of the MA nouns and significantly lower than that of
either the SS-MA or US-MA verbs.

The above results with regards to the first purpose stated on p. 210
support the minimum recall hypothesis, although some doubt remains.
Principally, since there is no way of knowing to what extent the low
non-adjusted thresholds of the SA nouns were due to less orthographic
confusability (see p. 172), it is still possible for someone wishing to
argue for the full recall hypothesis to claim that the SA nouns had
fewer response errors than MA nouns and other stimulus categories due to
their greater ease of recognition relative to MA nouns (because of the
difference in number of arguments, according to the full recall hypo­
thesis), and not vice versa (see p. 189).

However, the above line of reasoning in support of the full recall
hypothesis cannot explain the facilitation of the MA nouns relative to
the SS-MA and US-MA verbs, since the MA nouns had more error responses
than the SS-MA verbs and very nearly as many as the US-MA verbs (see
Table 13). In this sense, the threshold results for the MA nouns relative
to the SS-MA and US-MA verbs remain strong evidence for the minimum
recall hypothesis and against the full recall hypothesis.

Regarding the second purpose stated on p. 210—i.e., that of
comparing the thresholds of SS-MA, SA/MA and US-MA verbs with each other,
the results were disappointing for the minimum recall hypothesis. There
is some consolation, however, in the fact that the results with regards to this second purpose are certainly more equivocal than those discussed above with regards to the purpose of comparing SA and MA nouns with each other and with the verb stimuli.

Basically, the SA/MA verbs did not show the lower thresholds relative to the SS-MA and US-MA verbs predicted by the minimum recall hypothesis. But, on the other hand, neither did the SA/MA verbs have significantly higher thresholds than either the SA nouns or the MA nouns, although the latter did show a trend \( (p < .1) \) toward lower thresholds than the SA/MA verbs (see Table 16). Thus, the results with respect to the thresholds of the SA/MA verbs leave the question of the full recall hypothesis versus the minimum recall hypothesis wide open.

I will next present some speculations regarding the possible causes and the implications of the threshold results for the SA/MA verbs in my English tachistoscopic experiment. First, some possible explanations of the ambiguous results for the SA/MA verbs:

As mentioned in Chapter 3 (p. 198), one reason for the ambiguous status of the SA/MA verbs in the English tachistoscopic experiment may have been the fact that the SA/MA verb stimulus list mixed seven verbs like "grow" (which are semantically intransitive when used without a direct object) with eight verbs like "write" (which have understood objects even when they function intransitively). Under this line of reasoning the minimum recall hypothesis would hold for verbs like "grow" and the full recall hypothesis would hold for verbs like "write." As also noted (p. 198), a re-examination of the threshold data for the 15 items in the SA/MA verb category did not confirm this possibility.
However, it could still be the case that this suggestion might actually turn out to be correct when a large enough number of stimuli like "grow" and "write" are used and the proper controls are employed.

With regards to the speculations just put forward, it should be noted that there is some syntactic evidence that an underlying object should be posited for SA/MA verbs like "write" when used intransitively. The "do so" test seems to suggest an underlying object, as I will argue below (p. 246). Anaphoric reference also seems possible with the object of SA/MA verbs like "write" even when such an object is not present. ("I wrote all night, but it all reads like junk today.") Bresnan (1978) analyzes the underlying structure of SA/MA verb sentences without an object at the level of surface structure as containing no underlying object, although she does not present any direct evidence for this position. However, in favor of her position it should be reiterated that SA/MA verbs like "write" do not seem to depend on shared information for grammaticality when used without an object, although this might be questioned since when SA/MA verbs are used without their object they seem to be understood with a certain kind of object. Thus, "I ate" means "I had a meal" (i.e., not "I had a snack"); "I drank last night" refers to alcohol; and "He was carving when the phone rang" cannot be in reference to a turkey unless shared information exists about the turkey.

The kind of evidence just discussed also has a bearing on whether we should view SA/MA verbs as actually two separate lexical entries—i.e., as an SA verb with no direct object possibility and an SS-MA verb with an obligatory direct object. Here, it is not possible to review
all the evidence which bears on whether we should view SA/MA verbs as single lexical entries or not, especially since the interpretation of some of this evidence is bound up with the assumptions of the grammatical framework one chooses to adopt. Some of the reasons which might make us want to view SA/MA verbs as two separate lexical entries include the meaning differences— noted above— between the SA and MA usages of SA/MA verbs like "write" and "grow" and corresponding differences in selectional restrictions on arguments in the case of inchoatives like "grow." This is certainly a complex issue, since—as noted above—some of this evidence could also be interpreted as indicating an obligatory underlying object for SA/MA verbs like "write" (but not like "grow") even when used in single argument constructions—that is, some of this evidence could be interpreted as indicating that SA/MA verbs are actually US-MA verbs.

It is important to note that the predictions of the full and minimum recall hypotheses regarding SA/MA verbs are unclear without a decision on these questions (see also p. 158). That is, if verbs like "write" are US-MA verbs, then both the full recall hypothesis and the minimum recall hypotheses would make the same prediction about their processing complexity relative to other types of lexical items. On the other hand, if a given SA/MA verb is actually an SA verb and an SS-MA verb, then there is no difference between the full and minimum recall hypotheses in terms of how many arguments must be recalled from LTM with these separate entries. Both hypotheses predict that the SA item will be recalled with one argument and the SS-MA item with more than one argument. The only question is what determines which entry—the SA or the SS-MA entry—will be recalled from LTM in different task situations.
All other things (such as familiarity, etc.) being equal, the minimum recall hypothesis might still be interpreted as claiming a preference for the SA entry in cases of single-word processing tasks.

Of course, performance data would not necessarily offer clear answers to any of the above questions. For example, a finding of facilitation for SA/MA verbs relative to SS-MA and US-MA verbs in a single-word tachistoscopic task or as a result of brain damage would be consistent with either the view that 1) SA/MA verbs are actually single lexical entries and—in line with the minimum recall hypothesis—only the arguments necessary to construct grammatical sentences with these items are recalled from LTM; or with the view that 2) SA/MA verbs are actually two separate lexical entries, but in single-word recognition tasks or in cases of brain damage the entry with the fewer arguments is chosen for recall from LTM or selectively spared, respectively—familiarity and other factors being equal. This latter view would not necessarily contradict the full recall hypothesis since this hypothesis refers only to the recall of arguments within lexical entries. On the other hand, a finding of performance complexity for SA/MA verbs equal to that of SS-MA and US-MA verbs would be consistent with either 3) the full recall hypothesis under the single entry view of SA/MA verbs; or 4) the single entry view with the assumption that SA/MA verbs are actually US-MA verbs (which would not contradict the minimum recall hypothesis); or even with 5) a two-separate-lexical-entries view of SA/MA verbs and some modification of the full recall hypothesis which claimed that even in the isolated processing of words in a certain semantic class—such as SA and MA "write"—the more complex entry is chosen. Still other views consistent with either a finding of facilitation or no facilitation for
SA/MA verbs relative to SS-MA and US-MA verbs could be imagined. However, it does seem clear that it will be some time before performance data are sufficient to resolve the formal issues raised above, given the lack of a generally-agreed-upon model of the relation between formal aspects of the lexicon and neuropsychological processes.

Next, a factor which—alone or in combination with the first possible explanation of the thresholds of the SA/MA verbs (p. 213)—might explain the ambiguity of the results of the English tachistoscopic experiment for SA/MA verbs has to do with the concept of a "cognitive set." Such "sets" in psychology have been defined as "a...condition of an organism, facilitating a certain...specific type of activity or response" (Drever 1964:267). With regards to SA/MA verbs in English, the concept of "mental set" might come into play if, for example, speakers of English have an expectation (based on frequency of occurrence, etc.) for certain SA/MA verbs to appear in sentences with their optional arguments present. This expectation might, in turn, function to override the general cognitive principle posited by the minimum recall hypothesis. That is, if an SA/MA verb is usually encountered in sentences with its optional arguments present then it might be the case that these verbs are recalled from LTM even in tasks requiring little or no syntactic computation with all their arguments—thus overriding my prediction that words will be recalled from LTM in such tasks with only the minimum number of arguments necessary for use in a grammatical sentence. Although the factor of psychological set could be operative even under the view of SA/MA verbs as single lexical entries, it becomes especially important under the view of SA/MA verbs as two separate lexical entries.
Of course, when I put together the SA/MA verb stimulus list, I tried to select items which functioned equally often transitively or intransitively—judging by my own intuitions. I may not have succeeded, though—especially with verbs like "compose" and "carve" (see Appendix 2). Thus, this remains an area for future research of great relevance to theories of noun facilitation.

Finally, as noted on p. 198, we might try to explain the ambiguous results for the SA/MA verbs in my experiment with the kind of consideration discussed in Chapter 2 (p. 148) with regards to explaining the nominalizations of even SA verbs by brain-damaged persons who show a noun facilitation effect. That is, although the threshold differences in my experiment for comparisons of the SA/MA verbs with the other stimulus categories were not significant, it might actually turn out to be the case that SA/MA verbs (and SA verbs for that matter) should be ranked midway between SS-MA or US-MA verbs and SA or MA nouns, because all nouns are actually "zero argument" words and SA or SA/MA verbs are recalled from LTM—in tasks requiring little syntactic computation—as single argument words. (See p. 148 for discussion).

If the possibilities just discussed for explaining the ambiguity of the threshold results of the SA/MA verbs in my experiment should—with future research—dissolve into thin air, and SA/MA verbs do turn out to be every bit as difficult to recognize tachistoscopically as SS-MA and US-MA verbs and significantly more difficult than SA or MA nouns, we might want to consider the following possibility. Namely, the minimum recall hypothesis might be correct for nouns and the full recall hypothesis might be correct for verbs! This might not be as arbitrary
as it sounds. For example, if it can be shown that verbs are somehow "central" to the processing of utterances (for both decoding and encoding), then it might be the case that it is a natural function of the human language parser to activate as much information as possible about verbal (but not nominal) lexical items when they are recalled from LTM—regardless of considerations of optionality of arguments, the nature of task demands (i.e., those requiring much syntactic computation versus those requiring little), etc. Yamada (1979:18) comes very close to arguing for the kind of assumption regarding the "centrality" of verbs for processing that would be required as a basis for the possibility just noted. Chomsky (1965:15–6) is again relevant here as well.

Of course, for the above speculations to be taken seriously, two things must be done. First, it must be made clear exactly how verbs are "central" to language processing in a way that nouns are not. Then—and this is more important—one would have to devise some empirical test to confirm that when MA nouns are easier to process than MA verbs it is really because optional arguments are not recalled from LTM, and that when SA/MA verbs are not easier to process than SS-MA or US-MA verbs it is really because optional arguments are recalled from LTM. In other words, a test or an argument to show that the speculations contained in the paragraph above are not ad hoc is required. Otherwise, the suggestion that the minimum recall hypothesis is correct for nouns and the full recall hypothesis is correct for verbs begs the question.

I do not think the speculations just discussed are likely to be confirmed. I think that with further testing—perhaps using novel methods—and the appropriate controls, SA/MA verbs (at least those like
"grow") can be shown to be equal to SA and MA nouns in processing difficulty and significantly easier than SS-MA or US-MA verbs. Recall that OH did approximately equally well on SA and MA nouns and SA verbs. Many—if not all—of the Japanese SA verbs used with OH and in the tachistoscopic experiment can occur with such optional arguments as GOAL, SOURCE, INSTRUMENT, etc. According to the speculations outlined two paragraphs above, these possible arguments should have increased the difficulty of the SA verbs for OH if verbs are really always recalled from LTM with as much information as possible about potential arguments.

As we will see in section 4.3.1. below, an answer to the question of how many arguments must be recalled along with SA/MA verbs from LTM in different task situations is an extremely interesting one for current psycholinguistic research on parsing strategies.

Above I have discussed the relative merits of the minimum and full recall hypotheses in terms of normal and brain-damaged persons. The results of the age-at-acquisition questionnaire for the Japanese stimuli discussed in Chapter 2 completely paralleled those of the familiarity questionnaire also discussed in Chapter 2, and as such make it impossible to conclude anything definite about the actual relative age-at-acquisition of the stimuli. However, I did present (pp. 141ff.) some speculations on the implications of the results of the age-at-acquisition questionnaire assuming they reflected the actual relative age-at-acquisition of the stimuli. In section 4.4.2. I will outline the research that is needed to clarify the facts about language acquisition relative to corollaries of the minimum and full recall hypotheses.
In conclusion to this section, I think it is fair to say that there is some solid evidence (contained in this dissertation and elsewhere) and some circumstantial evidence in favor of the minimum recall hypothesis. On the other hand, the "evidence" in support of the full recall hypothesis is at best open to other interpretations (as with the low non-adjusted threshold mean of the SA nouns relative to that of the MA nouns in the English experiment), and at worst completely equivocal (as with the threshold results for the SA/MA verbs in the English experiment).

The questions that have been raised above as a result of trying to decide between the full recall hypothesis and the minimum recall hypothesis are at least as interesting as the question of the correctness of these theories themselves. I will return to some of these questions throughout the remainder of this chapter.

4.1.2. The Minimum Recall Hypothesis as an Explanation of Noun Facilitation Phenomena in General

Besides being relevant to a choice between the full recall hypothesis and the minimum recall hypothesis, the data presented in Chapters 2 and 3 also support the minimum recall hypothesis over other theories as an explanation of noun facilitation phenomena in general.

As stressed in Chapter 2, the derivationally related noun-verb stimulus pairs used in the Japanese experiment offered a unique opportunity to control many of the factors suggested in Chapter 1 as possibly influencing tachistoscopic recognition. This was especially true within the noun-verb pairs. Thus, within the noun-verb pairs the factors listed in (2-14)—p. 129—were held constant. That is, the noun and verb members of each derivationally related pair used as stimuli did not differ in terms of the factors listed in (2-14), with the exception of a
greater imageability and/or concreteness for the SA nouns than for the SA verbs. Because of this exception, it is better to consider the MA pairs at this point. (In addition, the SA verbs were rated more familiar than the SA nouns which makes conclusions with regards to the factors listed in (2-14) inconclusive. On the other hand, the MA nouns' being—if anything—less familiar than the MA verbs (see pp. 137-139), makes the facilitation for these nouns all the more striking).

Thus, to put things another way, the MA noun-verb pairs in the Japanese experiment offered an empirical test of the effect of the factors listed in (2-14) on noun facilitation. Since the MA nouns and verbs did not differ in terms of such frequently discussed factors as imageability, concreteness, semantic complexity, etc.—and since there was a strong facilitation effect for the MA nouns relative to the MA verbs—we can conclude that the factors listed in (2-14) do not form the basis of the noun facilitation effect seen in normal and brain-damaged persons.

This is not to say that the factors listed in (2-14) are without effect on different kinds of processing complexity. On the contrary, we know that factors such as concreteness have a demonstrable facilitating effect on the performance of some brain-damaged persons (see p. 35). Many of the factors listed in (2-14) do unquestionably also have an effect on tachistoscopic recognition. However, we know that the factors in (2-14) cannot offer a unified explanation of noun facilitation phenomena in general since the MA nouns and verbs in my experiment did not differ in terms of these factors.

In retrospect—that is, in the light of the English tachistoscopic experiment—one of the most advantageous aspects of the Japanese stimuli
must surely have been the fact that within pairs the kinds of orthographic factors discussed in Chapter 3 (section 3.1.2.3.) were held constant. The noun and verb members of each pair differed orthographically only in terms of the suffix symbol which determined their syntactic category, and hence were not subject to any of the problems discussed in 3.1.2.3. Again, this was fortunate since orthographic factors were found to be a major factor in the guessing strategy of the subjects in the English experiment (see p. 164) and were a major source of difficulty in interpreting the results of that experiment.

OH's results offer insights into the basis of noun facilitation not offered by the Japanese tachistoscopic experiment because of the differences in the familiarity of the stimuli as revealed by the questionnaire discussed in 2.2. OH's results on the two reading aloud tests conformed exactly to the predictions of the minimum recall hypothesis despite this difference in familiarity among the stimuli. Principally, this means that OH's performance on the MA nouns was equal to his performance on the SA nouns and verbs despite the lower familiarity ratings of the MA nouns (relative to the SA items) according to the 20 tachistoscopic subjects. The MA verbs—on which OH performed much more poorly than on the other three categories—are actually probably more familiar to Japanese speakers than the MA nouns (see pp. 137-139).

Many of the insights offered by OH's data in terms of the superiority of the minimum recall hypothesis as a general theory of noun facilitation have to do with other theories of noun facilitation that could come under the general heading of "anti-\(H_e\)" in Chapter 2 (see p. 76). For example, one such "anti-\(H_e\)"-type theory might suggest that nouns are
easier to process than verbs just because it is somehow more natural to use nouns than it is to use verbs in isolation. (Such a suggestion rapidly becomes more complex when one considers the question of Japanese ellipsis, including the optionality or necessity of shared information.) One can imagine several variations on this suggestion, perhaps taking into account frequency and other factors, but OH's results just do not support any such approach, since OH's performance on the SA verbs was at the level of his performance on the SA and MA nouns. All such "anti-\(H_e\)" theories which follow the two points on p. 76 ((2-3) and (2-4)) predict a facilitation of all nouns relative to all verbs regardless of number or optionality or arguments.

Other theories of noun facilitation not explicitly refuted by the Japanese tachistoscopic results but which OH's results do disconfirm are discussed in Chapter 2 (pp. 145-50). For example, the blend of syntactic and semantic aspects of the number of arguments factor (suggested on pp. 145-146) is disconfirmed by OH's results. Recall that this suggestion claimed that MA nouns should be midway in difficulty between SA items (nouns and verbs) and MA verbs since MA nouns can be used syntactically in simpler ways that MA verbs due to the optionality of their arguments, and since MA nouns are still semantically more complex that SA items because of the additional number of arguments required to characterize their meaning. OH's results disconfirm this suggestion—for cases of noun facilitation due to brain damage, at least—since his performance on the MA nouns was at the level of his performance on both the SA nouns and verbs.
Other possibilities regarding an explanation of noun facilitation in general and/or the Japanese tachistoscopic results are discussed on pp. 145-150. For example, OH's results also count against an explanation of the lower thresholds of the MA nouns in the Japanese experiment relative to the MA verbs on the basis of summated frequency, katakana expectations and category ambiguity (see pp. 130ff.) These ideas cannot explain OH's results since they all predict an equally poorer performance on SA and MA verbs than on SA and MA nouns, respectively.

In conclusion, OH's results on the two reading aloud tests present a challenge to any general theory of noun facilitation. The minimum recall hypothesis accounts for OH's results and, at the same time, accounts for many other aspects of noun facilitation phenomena as discussed throughout this dissertation.

Why it is important to understand the causal basis of noun facilitation phenomena--aside from the challenge to cognitive psychology of explaining these phenomena in themselves--will become clearer in the following sections.

4.2. The Relevance of Noun Facilitation Phenomena to Other Areas of Research

4.2.1. Noun Facilitation and Current Psycholinguistic Research on Parsing Strategies

A great deal of the current experimental work being done in psycholinguistics concerns the nature of the human "parsing mechanism." Most of this research is devoted to determining the processing mechanisms which allow people to comprehend sentences. "Stated simply, the task of a natural language parser is to assign a meaning to each of the sentences of a language" (Frazier 1979:1).
This area of research is extremely complex and reflects the wide variety of approaches to formal syntax currently available. However, one general criticism which can be made of many models of the human parsing mechanism has to do with the fact that these models do not sufficiently exploit lexical information to facilitate parsing. For example, this is a criticism which might be made of the interesting augmented transition network (ATN) approach to a parsing model, such as that outlined in Wanner and Maratsos (1978). It seems unlikely that the actual human parser ignores lexical information in making parsing decisions.

There are some reasons for the failure of many current parsing models to exploit (or their decision to ignore) lexical information. For one, just getting a model to work on algorithmic principles for a limited data set is difficult and interesting enough. For another, the range of lexical information of potential relevance to parsing strategies is so enormous that it is difficult to find a systematic way of beginning to incorporate this information into parsing models. Some recent work by Frazier (1979) on syntactic parsing strategies offers an opportunity to explore some possibilities in this regard which are relevant to the issues discussed throughout this dissertation in connection with the role of number of arguments in noun facilitation phenomena.

A large part of Frazier's (1979) dissertation is concerned with testing the "late closure" principle which she defines as follows.

**Late Closure:** When possible, attach incoming material into the phrase or clause currently being parsed (ibid.: 33).

As a parsing strategy, late closure is meant to contrast with an "early closure"-type principle which would have the parser tend to close clauses.
and phrases and shunt them away to memory as soon as possible. Thus, Frazier claims that people's performance on (4-8)

(4-8) While Mary was mending the sock fell off her lap (ibid.:76).

gives evidence for late closure rather than early closure since there is a clear tendency to parse "the sock" as the direct object of "mending"—even after this analysis is proven incorrect by the succeeding words. That is, Frazier claims that early closure predicts that the parser would try to close the "while" clause after "mending," since (she claims) the first potential clause boundary in (4-8) occurs after "mending."

Frazier's test sentences such as (4-8) constitute—according to her—early closure sentence versions in that the actual clause boundary precedes a merely potential clause boundary (ibid.:80). That is, in an early closure sentence like (4-8) the actual clause boundary is after "mending," although the parser might attempt to fix the clause boundary after "the sock." Sentences like (4-8) are called "early closure sentences" because the actual clause boundary is located where an early closure parsing strategy would prefer to mark the clause boundary. These early closure sentences are contrasted in Frazier's experiments with late closure versions such as (4-9).

(4-9) While Mary was mending the sock it fell off her lap (ibid.:76).

In Frazier's late closure versions a merely potential clause boundary precedes the actual clause boundary (ibid.:80). Sentences like (4-9) are late closure versions because the actual clause boundary is located where a late closure parsing strategy would prefer to mark it—i.e., after "the sock." Using a rapid serial visual presentation (RSVP),
Frazier tested the processing complexity of the early closure versus the late closure versions, along with an early closure semantic control version in which the actual clause boundary was followed by a semantically implausible clause boundary—as in (4-10).

(4-10) While Mary was mending the sky turned grey and dark (loc. cit.).

In her first experiment, subjects were asked to recall the test sentences. In the second and third experiments subjects were asked to give grammaticality judgments of the same sentences, and reaction times were measured. Frazier concluded—despite some discrepancies among her three experiments—that her results supported late closure over early closure in that the late closure versions showed better recall and lower reaction times than the early closure versions. She also concluded that there was no difference between the semantic control sentences and the late closure sentences. Frazier interprets this last result as supporting a "weak" interpretation of the role semantic information plays in sentence processing. That is, according to Frazier, semantic constraints alone are not enough to facilitate the processing of early closure semantic control sentences relative to late closure sentences.

Now, here I cannot go into the details of Frazier's discussion of parsing strategies or her rationale for testing parsing strategies outside of conversational or textual context and without the presence of disambiguating pausal and intonational cues (which in sentences like (4-8) and (4-9) would have a corresponding comma in printed form). The following discussion is offered with these reservations in mind. Nor are the remarks to follow intended to argue against late closure per se. On the
contrary, late closure seems to make great intuitive sense. I am interested in looking at the relation between late and early closure, on the one hand, and the full and minimum recall hypotheses on the other. The idea of late closure appears analogous to the full recall hypothesis since it implies that the parser tends to fill optional argument slots rather than beginning a new clause. Early closure appears analogous to the minimum recall hypothesis since it implies that the parser is reluctant to fill optional argument slots.

However, I feel that the early closure—late closure dichotomy as set up by Frazier is somewhat misleading. Rather than being competing principles, it seems to me that early and late closure are just two sides of the same coin. Let us assume that the parser tries to analyze sentences in such a way as to minimize syntactic complexity. Under this assumption, when the parser encounters an SA/MA verb in a one-clause sentence it might "prefer" to posit a clause boundary after the verb. However, if the parser encounters a word after the SA/MA verb which can serve as its direct object it might prefer to so interpret the word, since to do otherwise leads to an expectation of greater syntactic complexity (i.e., another clause). At this point, late closure is early closure. Note that these remarks apply to sentences such as (4-11) which Frazier also discusses as evidence of late closure—and not sentences like (4-8), since the first words of the latter lead the parser to expect another clause in any case.

(4-11) The children were playing football was on television and Granny was knitting (ibid.:152).

Of course, something should also be said about the appropriateness of sentences like (4-11) for Frazier's tests since these sentences seem
anomalous without punctuation. (Recall that Frazier asked for grammaticality judgments from her subjects in order to measure reaction time.)

According to the view just outlined, early closure and late closure are complementary—rather than mutually exclusive—strategies. That is, early closure and late closure could both be viewed as a result of the parser's tendency to adopt syntactic analyses which minimize complexity. Under this view, late closure no longer appears to parallel the full recall hypothesis or to run counter to the minimum recall hypothesis. Nonetheless, there are still problems with Frazier's tests of late closure which must be discussed in view of the issues raised in this dissertation.

Notice that (4-8)—used by Frazier as evidence of late closure—contains a US-MA (or "zero morph" verb). That is, "mend"—in my dialect, at least—cannot be used in a sentence without a direct object unless the object is part of shared information. Notice that (4-12) is ungrammatical with the reading of "mend" as "fix clothes."

(4-12) *Mother mended well. (with the reading of "mend" as "fix clothes")

In my dialect (4-12)—like sentence (4-8) itself—is grammatical only in a context of shared information, or if we assume "mother" had some broken bones and "mended" means "got better." (However, I have met one person for whom "mend" is a true SA/MA verb, although most of the people I have talked to consider it a US-MA verb. People who do a lot of clothes mending might consider "mend" an SA/MA verb.)

In support of late closure, Frazier cites other sentences having US-MA verbs, such as (4-13).
In order to help the little boy put down the package he was carrying (ibid.:21).

(4-14) seems bad to me without shared information.

*He helped.

The following two sentences--along with (4-8)--were used in Frazier's three experiments on late closure.

(4-15) Anne was watching you were laughing and nobody knew why (ibid.:151).

(4-16) John was hitting Jack was pitching and Gerry was coaching Tom (loc. cit.).

Sentence (4-15) contains the US-MA verb "watch," although the sentence as a whole is perhaps not ungrammatical--in contrast to my judgment of (4-8)--since it is possible to interpret what follows "watching" in (4-15) as providing the information necessary to permit the deletion of the object of "watching." Regarding sentence (4-16), "hitting" is an SS-MA verb unless we share the information that baseball is under discussion. That baseball is, in fact, under discussion in (4-16) does not become clear until the occurrence of the word "pitching."

The major point I want to make with regards to sentences (4-8, 9, 10, 13, 15 and 16) cited in Frazier is that US-MA verbs can not be used as a fair test of late closure versus early closure (especially since grammaticality judgments were asked for in two of Frazier's experiments). The fact that US-MA verbs can only undergo deletion of their direct object in limited contexts of shared information suggests that in comprehension and recall tasks involving US-MA verbs the parser must act on the lexical information that US-MA verbs must have their arguments specified in underlying structure. The parser must therefore try to fill the direct object frame for these verbs when shared information is not present--even
in sentences like those above from Frazier where it is impossible to do so. The only thing that Frazier has to say regarding the use of this type of lexical information in parsing is the following:

Clark and Clark (1977) discuss the following [parsing] strategies:

**Strategy 4:** After encountering a verb, look for the number and kind of arguments appropriate to that verb.

Strategy 4 is intended to capture a suggestion made by Fodor, Garrett and Bever (1968) [see p. 77 of this dissertation] that the POTENTIAL roles a verb can play in a sentence also guides sentence perception. The only import of Strategy 4 is that the parser's predictions about future constituents can be based on the subcategorization features of particular verbs as well as general phrase structure rules (ibid.:55-6).

In the above quote Frazier completely ignores the distinction between optional and obligatory arguments.

Furthermore, the results of my English tachistoscopic experiment support the view expressed immediately above the quote from Frazier (1979) since they suggest that US-MA verbs are recalled from LTM with all of their arguments **even in tasks requiring little or no syntactic computation.**

At the very least, an adequate test of late closure would have to use SA/MA verbs. This is precisely where clearer data on the difficulty of SA/MA verbs relative to SS-MA and US-MA verbs in tachistoscopic recognition would be useful. But this leads to consideration of another problematic point in Frazier's test materials.

The majority of her test sentences (ibid.:150-2) do not contain US-MA verbs. However, many **do** contain SA/MA verbs (or other words with a strong collocational association with a particular kind of argument as in (4-17) below.
(4-17) After you drank the water was discovered to be polluted (ibid.:150).

Other examples of this sort among Frazier's test sentences (ibid.:150-2) include those listed in (4-18).

(4-18) reading the story
walks her dog
barbecue the steaks
rides his horse
graduation speeches
monster movies
rich politicians
playing football

In Frazier's stimulus materials the phrases listed in (4-18) all occur in both an early closure and a late closure version, as described above, and the early closure version always has the actual clause boundary after the first word of the phrases listed in (4-18).

The strong association of the initial word in each of the phrases in (4-18) with the word which follows it should not be underestimated as having a possible effect on processing strategies. Indeed, many of the phrases in (4-18) have an almost "frozen" or "automatized" nature in the sense that if a person were given one of the initial words in (4-18) and asked to make a two- or three-word phrase, he or she might very well come up with some of the phrases listed in (4-18). (Charles Fillmore--1979 LSA summer course on conventionality in language--uses the phrase "collocational strength" to refer to the degree of automatization in phrases such as those in (4-18).)

This strong association between the words of the phrases in (4-18)--due to frequent occurrence, etc.--would have a confounding effect on any experiment whose purpose was to test late closure versus early closure. In addition, the strong association between the words of these phrases
could be a reason Mr. Frazier failed to notice any facilitating effect for semantically implausible early closure sentences versus late closure sentences (see p. 228). The remarks made above (pp. 213ff.) regarding semantically transitive versus semantically intransitive SA/MA verbs when used as SA verbs and cognitive set as possible reasons for the failure of the SA/MA verbs in my English experiment to achieve lower thresholds than the SS-MA and US-MA verb stimuli are all relevant here.

In line with the remarks made on pp. 213ff. and in this section, I would suggest that a strong test of early versus late closure should follow these guidelines: 1) The test sentences should set up the expectation of a main clause to follow a subordinate clause as in (4-8) and not as in (4-11), since in the latter the parser could be expected to resist positing a second clause at the occurrence of a potential direct object as discussed above (see p. 229). 2) SA/MA verbs should be used rather than US-MA verbs, preferably SA/MA verbs which—when used as SA verbs—are semantically intransitive like "grow." 3) Whatever type of SA/MA verbs are used, care should be taken to ensure that no strong collocational association exists between the verb and its object in the test sentence. 4) Also, the SA and MA usages of the SA/MA verbs should be controlled—by some kind of pretest—so as to ensure that neither of these is more common or expected, thus avoiding the problem of "cognitive set" discussed above.

I know full well that the above guidelines would be very difficult to follow. Examples which follow the guidelines are contained in (4-19) below.
(4-19)  

a. While Mary was relaxing her legs gradually loosened up. (early closure)

b. While Mary was relaxing her legs they gradually loosened up. (late closure)

If "relax" is more common as an SA verb, the sentences in (4-19) provide an even stronger test of the validity of late closure. The important thing is that an initial verb which is more common as a transitive verb could confound any test of late closure due to the effects of cognitive set.

Finally, an additional work of relevance to the minimum recall hypothesis which Frazier also discusses (ibid.:122) is Carroll and Tanenhaus (1975). Carroll and Tanenhaus were concerned with the ability of different segmentation units within sentences to repel (the subjective location of) clicks in a paradigm similar to Bever, Lackner and Kirk (1969). The segmentation units which they investigated included main and subordinate clauses, noun phrases and headless nominalizations (e.g., "Meeting the pretty young girl was the highlight of Peter's trip"). One puzzling result of their work was that long headless nominalizations (of which they provide no examples) were much less effective than long clauses, long noun phrases and short nominalizations (such as the example just given) in repelling clicks.

Here I would like to suggest that a key factor in experiments like that of Carroll and Tanenhaus (1975) could well be whether any of the constituents of the stimulus segmentation units are obligatory arguments of other constituents in such units. As far as I can tell from the limited stimulus examples provided by Carroll and Tanenhaus (ibid.:5), their main and subordinate clauses involve SS-MA and US-MA verbs. We
might well expect segmentation units involving several obligatory arguments to be more solid as a perceptual unit and thus more effective at repelling clicks. We might also expect this factor to interact with length. That is, increased length due to a greater number of obligatory arguments could be expected to increase a segmentation unit's ability to repel clicks, but increased length due to the addition of optional arguments may even be expected to decrease it.

Thus, the above speculations might help explain the result for the long headless nominalizations reported in Carroll and Tanenhaus (1975). In any case, an interesting point for future research in their paradigm would certainly be to test the hypothesis just offered. For example, it would be enlightening to see whether a noun phrase like "John's gift of candy to Mary" will be equally or less effective in repelling clicks than, say, a subordinate clause like "(the fact that) John gave candy to Mary." The arguments presented in this dissertation would suggest that the foregoing noun phrase should be less effective at repelling clicks since it contains optional arguments. (However, a disconfirmation of this hypothesis would not disconfirm the minimum recall hypothesis for cases involving the isolated recall of lexical items from LTM.) As far as I can tell, the long noun phrases in Carroll and Tanenhaus (1975) consisted only of the following type: "The old painted wooden pipe." Such noun phrases do not allow us to compare the effect of optional versus obligatory arguments since the piling up of adjectives (within limits) merely increases the expectation of a head noun, and thus increases the perceptual solidity of the noun phrase.
4.2.2. Noun Facilitation and Formal Theories of Syntax

In discussing the effect of number of arguments on noun facilitation I have tried to avoid touching on contested issues in theoretical syntax. However, the issues raised throughout this dissertation do bear a relation to questions in formal syntax.

Chief among these questions is the nature of the underlying representation of sentences with MA nouns used without explicit arguments, such as (4-20) below.

(4-20) Books make nice gifts.

The question: In the theory of grammatical competence do we want to derive sentences like (4-20) from underlying structures containing explicit indefinite arguments similar to (4-21)?

(4-21) Books make nice gifts from people to other people.

For example, work in the lexicalist framework such as Chomsky (1972) assumes that the answer to the foregoing question is "no," while generative semantic work such as Postal (1969) assumes that the answer is "yes." In this section, I will examine some aspects of this question.

First of all, note that the data reported in this dissertation seem to point to a "no" answer to the above question, since the evidence presented in this dissertation supports the minimum recall hypothesis with regards to MA nouns. However, here we run into the now overly familiar competence-performance catch (see also the discussion of Whitaker 1972, pp. 57-8 above). That is, no matter how strong the evidence which could be mustered to support the processing facility of MA nouns, one could always claim that the empirical data reflected the nature of performance strategies and not competence representations.
Since there is no real way to refute this line of reasoning—which amounts to an axiomatic pre-theoretical rejection of performance data—there is no easy way out of the competence-performance catch, other than to reject it outright as a desirable assumption for the basis of linguistic research. (Of course, one could still try to use purely formal data to support claims based on performance considerations, as I will do below.) One could, however, question the empirical content of theories of competence under such assumptions (cf. Whitaker 1974).

The above remarks notwithstanding, it is still of great interest to examine whether some purely formal syntactic evidence can be adduced to indicate that sentences containing MA nouns without explicit arguments are indeed derived from underlying structures without such arguments, thus paralleling the experimental data on the minimum recall hypothesis presented in this dissertation. Of course, if such evidence can be adduced it will greatly strengthen the minimum recall position.

I will briefly discuss three pieces of evidence which support a "no" answer to the question posed at the beginning of this section.

First, whenever we begin to posit underlying forms which do not show up on the surface it is legitimate to ask "Where do we stop?" Thus, if we are going to posit underlying indefinite direct object and indirect object arguments for sentences like (4-20), why not posit underlying indefinite manner adverbs, arguments of place, etc.? In answer to this question one might suggest the following answer. Namely, we need only posit underlying indefinite arguments for MA nouns like "gift"—in the case of sentences like (4-20)—to the extent that their corresponding verb form has obligatory arguments. Thus, since "give"
when it is used in the sense of "hand over"—has three obligatory arguments, these would have to be included in the underlying representation of sentences like (4-20).

This solution has a major—and, by now, familiar—flaw: Not all MA nouns have corresponding verb forms. In fact, many of the MA nouns used in my English tachistoscopic experiment (see Appendix 2) have no corresponding root verb. This is the case with "greed," "truth," "pal," "debt," etc. Of course, this point is exactly the same as that made in Chomsky (1972) for not generating "derived" nominals from underlying verbs. The significance of this point for the question posed at the beginning of this section is that without a corresponding root verb there seems to be no systematic way to fix a limit on the number and types of underlying arguments one can posit for sentences with MA nouns—assuming one would want to posit such underlying indefinite arguments in cases like (4-20). Of course, it might be worthwhile to conduct a tachistoscopic or other experimental study comparing MA nouns having a derivationally related SS-MA or US-MA verb with MA nouns having no such related verb.

My second argument against positing underlying indefinite arguments in sentences like (4-20) derives from Roeper and Siegel (1978). To explain how their paper provides this argument, I will first have to describe the main topic of their paper and then a class of apparent exceptions to their hypothesis. It is the existence of these exceptions—and Roeper and Siegel's explanation of these exceptions—that provides the argument against positing underlying indefinite arguments in sentences like (4-20).
Roeper and Siegel's paper is concerned with describing a lexical transformation to account for verbal compounds such as those shown in (4-21) below.

(4-21) clean ovens → oven-cleaner
break jaws → jaw-breaker
sound strange → strange-sounding
fried in a pan → pan-fried (ibid.:199)

Roeper and Siegel's basic problem in this paper is to explain the impossibility of some verbal compounds versus the existence or possibility of other types of verbal compounds.

The generalization Roeper and Siegel come up with is the "first sister" principle.

All verbal compounds are formed by incorporation of a word [which can appear] in first sister position of the verb (ibid.:208).

The first sister principle states that if an argument obligatorily intervenes between a verb and another word, this latter word cannot be incorporated into a verbal compound—since it cannot occur in first sister position of the verb. Thus, an example like "quick-making" is impossible whereas "rapidly-rising" is an existing compound, since when "make" is used in sentences with an adverb like "quick" this adverb must come after the obligatory direct object argument for "make." Note also that an example like "high-jumping" is an existing compound even though when "jump" is used with a direct object the object must occur in first sister position of the verb, since "jump" does not have to be used with a direct object. Roeper and Siegel (ibid.:244-6) also show that US-MA verbs—which they term "zero morph" verbs—obey the first sister principle. For example, *"clear(ly)-hearer" is unacceptable despite the
fact that "hear" can occur without a direct object in certain conversational contexts involving shared information. This observation confirms that it is the obligatory nature of arguments at the level of underlying structure which matters in the formation of verbal compounds.

Crucial to Roeper and Siegel's arguments is their explanation of examples which appear to violate the first sister principle, such as those in (4-22).

(4-22) apprentice welder
surprise winner
salad dressing
Aztec drawing (ibid.:218-9)

They explain such apparent counterexamples by arguing that they are not verbal compounds but phrase-structure generated adjective + noun sequences, which obey the following principle:

In a PS-generated sequence, each lexical item must have a separate representation in the lexical core (ibid.:218).

Note that the first sister principle, on the other hand, can form verbal compounds even out of items that are not independently represented in the lexicon, as in "sword-swallowers" (ibid.:219).

Roeper and Siegel assume that there is no restriction similar to the first sister principle on what adjective + noun sequences can be generated by the phrase structure rules, as long as the items involved have an independent representation in the lexicon.

Roeper and Siegel's explanation of these apparent exceptions to the first sister principle dovetail with what I have assumed and argued about the underlying structures of sentences with MA nouns. That is, their explanation of these exceptions is that the phrase structure rules operate to generate sequences such as those in (4-22) and those in (4-23),
without regard to the placement of arguments. This suggests that the nominal lexical items used as the second item in such sequences are not derived from verbs with obligatory arguments. If the nominal elements inserted into phrase-structure generated sequences such as (4-22) and (4-23) had obligatory underlying arguments, we would have no explanation of why the phrase structure rules do not take account of these obligatory arguments in generating sequences like (4-22) and (4-23). If we assume that MA nouns like those in (4-23) have (at least indefinite) obligatory underlying arguments, then we also have to assume that in generating sequences such as (4-23) phrase structure rules (or subsequent filtering devices) ignore these underlying arguments—whereas the lexical transformations which form verbal compounds do not. But there is no reason to assume that phrase structure rules (or subsequent filters) would ignore such information—if it existed—since this information is not ignored in the generation of verb phrases, for example.

The most likely conclusion that can be drawn from examples like (4-23)—assuming the correctness of Roeper and Siegel's arguments—is, therefore, that MA nouns do not have obligatory underlying arguments.

Of course, Roeper and Siegel's analysis of exceptions to the first sister principle such as (4-22) and (4-23) depends on whether it can be shown that the second member of sequences such as (4-22) and (4-23) is actually an independently lexicalized noun. As a test for independently lexicalized nouns in "-ing," Roeper and Siegel note that independently lexicalized nouns in "-ing" (like "drinking" and "warning") are listed
in the lexicon as uncontrolled, while those that are in fact verbs must have control (ibid.:220), as the following examples show.

(4-24)  a. I hate drinking (ibid.:219).
       b. I hate warnings.
   c. I hate growing (loc. cit.).

In (4-24a and b) anyone can be doing the drinking or the warning, while in (4-24c) only "I" can be taken as the subject of "growing." These facts suggest that "-ing" forms like "growing" in (4-24c) derive from underlying sentential complements with their subject arguments deleted under equi-NP. In contrast, it seems that "-ing" forms like "drinking" and "warning"—as independently lexicalized nouns—permit interpretation of their subject arguments as either identical or non-identical to that of the matrix verb in sentences like (4-24), since they derive from non-sentential underlying structures and thus have no underlying subject arguments. Thus, the contrast between sentences like (4-24a and b) and (4-24c) is evidence both for the independently lexicalized status of nouns like "drinking" and "warning" and evidence against positing underlying arguments for (independently lexicalized) MA nouns in "-ing" when used in sentences like (4-24a and b).

A criterion not mentioned by Roeper and Siegel for determining independently lexicalized nominals in "-ing" in the case of SS-MA and US-MA verbs is whether a given "-ing" form can occur in sentences with no arguments. (Of course, SA and SA/MA verbs can do so in their "-ing" form whether or not the "-ing" form is an independently lexicalized nominal.) Thus, sentences such as those in (4-27) are grammatical because the "-ing" forms involved are independently lexicalized
nominals.

(4-27)  

a. Warnings offered with a smile are somehow sinister.  
(< an SS-MA verb)

b. Rock concerts can damage hearing.  (< a US-MA verb)

c. Meetings go better with wine!  (< an SS-MA verb)

Note that similar sentences involving "putting," "solving," etc. are not possible.  (With regard to this point, consider again the discussion on pp. 204-6 above).

Considering the above arguments, I think it is safe to conclude that Roeper and Siegel's distinction between verbal compounds and phrase-structure generated adjective + noun sequences is a valid one, and can therefore be used as evidence that MA nouns have no obligatory underlying arguments.

My third argument against positing underlying indefinite arguments in sentences like (4-20) and (4-27) has to do with a contrast between anaphoric reference involving "do so" as a pro-verb for SS-MA and US-MA verbs and anaphoric reference involving "one" as a pronoun for MA nouns.  Note the following contrasts.

(4-28)  

a. That clock was a gift from my mother on my last birthday and these tapes were one from my girlfriend.

b. *My mother gave me that clock on my last birthday, and my girlfriend did so these tapes.

c. John's proof of Theorem A was more elegant than the one by Tom of Theorem B.

d. *John proved Theorem A in a more elegant manner than Tom did so Theorem B.

Some speakers I questioned did not think (4-28a) was too good, but the majority found it perfectly acceptable.  In any case, everyone I
questioned agreed that it was much better than (4-28b). Sentence (4-28c) seems perfectly acceptable.

There are many complexities and subtleties of judgment involved with comparisons among sentences like those in (4-28) involving other MA verbs and nouns, different forms of reference, etc. (For example, gapping in (4-28b)—but not in (4-28d)—seems to result in acceptability.) However, the hypothesis I want to make regarding the behavior of the forms of anaphoric reference in the sentences in (4-28) is this: In cases of anaphoric reference involving "do so," the anaphoric element "do so" has to encompass all of the obligatory arguments of the antecedent verb phrase in order to be grammatical. On the other hand, anaphoric reference involving "one" appears (at least) much more acceptable in sentences like (4-28a and c) than sentences like (4-28b and d) involving "do so" since—I would suggest—MA nouns like "gift" (even though related to "give" with its obligatory arguments) do not have obligatory underlying arguments.

The following sentences show that optional verb phrase arguments do not have to be included in the antecedents of anaphoric reference with "do so."

(4-29) a. I put the books on the shelf by hand, but Mike did so with a forklift.

b. *I put the books on the shelf, but Mike did so on the floor.

c. I warned John about the invasion in the conference room, but Mike did so on the roof.

d. *I warned John about the invasion, but Mike did so Mary.

In (4-29a and c) the reference with "did so" is grammatical since the
INSTRUMENT and LOCATIVE phrases changed in these sentences are optional arguments. (4–29b and d) show that the obligatory verb phrase arguments of "put" and "warn" cannot be so changed. Note that this distinction between obligatory and optional verb phrase arguments corresponds to the distinction drawn in Lakoff and Ross (1966) between the "inner constituents" of a verb phrase and its "outer constituents," respectively. (See also the discussion on p. 68 above.)

As also noted in Chapter 1 (p. 68) and on p. 214, the arguments which may be optional at the level of surface structure in the case of SA/MA verbs also may not be changed in anaphoric reference with "do so." This could suggest that the direct object of SA/MA verbs when not present in surface structure is obligatorily present—at least as an indefinite—in underlying structure. (Consider *"I read the magazine, and Mary did so the book" or *"I was reading, and Mary was doing so the magazine." ) On the other hand, this could suggest separate SA and SS–MA entries for SA/MA verbs as discussed above (see pp. 214ff.).

4.2.3. Noun Facilitation and Neuropsychology

In cognitive psychology, neuropsychology and related disciplines today there is a great deal of interest in "levels" of processing, awareness, etc. The concept of "levels of processing" was brought up in Chapter 2 (p. 79) as background to the minimum recall hypothesis. A "level" in the sense intended here is a processing stage and/or mental representation ordered with respect to other such "levels"—according to considerations of complexity, elaboration, etc.—which may also be susceptible to selective impairment in the event of brain damage.
In cognitive psychology, a hierarchy of processing levels has been used in models of memory to help explain performance phenomena in such areas as retention, recognition, incidental learning, and rehearsal effects. Such an approach is evident, for example, in Posner (1969), Norman and Rumelhart (1970) and--more explicitly--Craik and Lockhart (1972). In neuropsychology, models incorporating a hierarchy of levels are contained in the work of Brown (1972, 1977), Sommerhoff (1974), Milner (1976) and Thatcher and John (1977), among many others. Such discussions have been concerned with levels in the formation of consciousness, phylogenetic and ontogenetic developmental levels, pathological regression, etc. Furthermore, many of these concepts of "levels" in cognitive studies and neuropsychology have roots at least as far back as the nineteenth century--particularly in the work of Hughlings Jackson.

It is not hard to see why such ideas of "levels"--in their varied forms--are attractive to researchers in the areas of cognition and neuropsychology. Such concepts in fact offer a way of tying together models of diverse areas such as evolution, ontogenetic development, brain damage and psychological testing with adults--based on the clearly hierarchical organization of the CNS. In addition, models incorporating hierarchical levels are extremely powerful. It might be the case, for example, that performance data which are at odds with otherwise well motivated formal models can be explained by appeal to processing at different levels.

However, this power is also a drawback since--at least in models of cognition incorporating processing levels--it is largely unconstrained.
Baddeley (1978:150) notes that current versions of models incorporating levels of processing can "explain virtually any result in a post hoc manner while having little or no predictive value." Baddeley (ibid.:141) also implies that a major problem in testing depth of processing and other "level" theories is a lack of studies using clearly defined levels within a given domain--such as semantics, syntax, etc. Many, if not most, of the existing studies of Craik and Lockhart's (1972) proposals involve single levels from different domains (ibid.:141-2). Baddeley is not optimistic about a confirmation of Craik and Lockhart's proposals given stimuli within clearly defined levels within a given domain, but he does note that

the evidence [regarding this matter] is so sparse it would be unwise to draw any stronger conclusion than that the levels-of-processing approach has not yet produced any convincing evidence to suggest a continuum of [processing] levels rather than a more limited number of processing domains (ibid.:142).

Now, it is surely too early to give up on the levels-of-processing approach to cognition--both because of its attractions noted above (p. 247), and because, as a theory, it still has not been disconfirmed. Linguistic theory--with its variety of formal descriptive devices--would appear to be a good, if not the best, place to start looking for clearly defined levels within a given domain such as syntax or semantics.

Marshall et al.'s (1975) hypothesis--based on previous work such as Fodor, Garrett and Bever (1968)--regarding the role of number of arguments in noun facilitation offers a good place to begin. Let us assume the following regarding levels within the domain of syntax: A "level" of syntax contains items (words, morphemes, etc.) and rules (phrase structure rules, transformations, word formation rules, etc.) for
combining these items into larger units. Attached to each item are "features" (subcategorization frames, selectional restrictions, etc.) which correspond to the combinatory rules which affect them. Marshall et al.'s (1975) theory assumes that the processing complexity of a given item is determined by the complexity of its "combinatory features" corresponding to the constructions into which it can enter (and, of course, the combinatory rules which generate these constructions).

Now, to follow Baddeley's suggestions discussed above with respect to the assumptions just outlined we have to specify what might constitute different levels of syntactic complexity. This is where the greatest danger lies because there is a tendency toward circularity here. We do not want to end up saying that a given lexical item is, for example, difficult to process because it is stored at a deep or complex level, without saying exactly what constitutes the complexity of this level.

In line with what was said two paragraphs above, what we want to do in order to begin to follow Baddeley's suggestions is correlate the representation of lexical items at certain levels with the kind of constructions possible at these levels. Thus, assuming that there is a relatively simple level of linguistic processing for single argument constructions (such as SUBJECT \( \rightarrow \) VERB), we would expect items which can enter into such constructions to be marked with combinatory features appropriate to single argument constructions at that level. On the other hand, items--like SS-MA verbs--which must be used in multiple argument constructions will have no appropriate representation with regards to the level just discussed. Instead, such items will have a representation—complete with combinatory features—at a "deeper" level
capable of generating/processing multiple argument sentences. In line with the minimum recall hypothesis, MA nouns—and perhaps other items such as SA/MA verbs, some adjectives, etc.—which can occur in both single and multiple argument constructions will have representations (subcategorization features, etc.) at both of the levels just discussed. One can, of course, imagine more than two levels of complexity corresponding to different kinds of syntactic complexity.

So much for a first try at defining "levels" within the domain of syntax. What we still need are hypotheses about the operating principles for these different levels. That is, we need to specify—for example—the kinds of features that will be recalled from LTM with lexical items in different task situations. Marshall et al.'s approach in this regard is in a curious position. On the one hand, it in part implies the model outlined above (pp. 248-50). On the other hand, it actually has the effect of nullifying much of the complexity of this model by ignoring the various aspects of optional and obligatory arguments.

In contrast, an approach to the processing complexity of lexical items which takes full account of the aspects of optional and obligatory arguments—such as the minimum recall hypothesis—does justice to the model presented above (pp. 248-50) and makes a priori sense in terms of the time and effort constraints a human processor could be expected to operate under. Furthermore, the minimum recall approach—perhaps more than the full recall approach—offers the kinds of constraints on a levels-of-processing approach to cognitive processing which Baddeley (1978) suggests are needed to make disconfirmable predictions. The central idea of the minimum recall approach is that the processing
complexity of any lexical item is determined by the complexity of the least complex grammatical sentence in which it can be used independent of shared information. This idea represents a strong claim within the domain of lexical and syntactic processing which makes possible predictions capable of clear confirmation or disconfirmation—as shown by the discussion throughout this dissertation. And the evidence presented in this dissertation in favor of the minimum recall approach is also initial evidence in support of a levels-of-processing approach—as outlined on pp. 248-50—to the processing complexity of lexical items.

Note that some of the possibilities discussed in this dissertation would introduce qualifications to the minimum recall approach if confirmed by future research, and may even serve to weaken its empirical content. This is the case with the hypothesis combining semantic and syntactic domains (see pp. 145-6); the status of SA/MA verbs see pp. 213ff.); and the degree of collocational association between a word and its arguments (see pp. 232-4). Note that in the case of the status of SA/MA verbs, the interchange between formal syntactic evidence (as discussed on pp. 185-6) and performance data is particularly crucial. For example, if convincing syntactic evidence should become available that English SA/MA verbs like "write" have underlying obligatory direct objects, then a finding of such SA/MA verbs having a processing complexity equal to SS-MA and US-MA verbs would not force us to abandon the minimum recall hypothesis. In such a case the empirical nature of the minimum recall hypothesis would be maintained by the formal syntactic evidence. That is, maintenance of the minimum recall hypothesis in such a case would not be ad hoc. (Note, however, that no
such case exists; the processing complexity of all SA/MA verbs is still an open question. The above speculations were offered to illustrate the interplay between the disconfirmability of a theory like the minimum recall hypothesis and formal syntactic evidence.)

Although some of the issues discussed in this dissertation--such as those mentioned in the above paragraph--are still open questions, OR's results on the two reading aloud tests strongly suggest an independent effect for levels of processing within the lexical-syntactic domain according to the strongest prediction of the minimum recall hypothesis--whatever the effectiveness of other factors (such as semantic features, etc.) on different kinds of processing.

An interesting goal for future research would be to see if the minimum recall approach to the processing complexity of lexical items can be used to provide some evidence for or against the levels-of-processing approach to such tasks as retention, incidental learning and rehearsal effects--which Baddeley (1978:142) states is needed (see p. 248). It may be the case that depth of processing within different domains--e.g., phonology, semantics, syntax, etc.--has different effects in these different tasks. For example, syntactic depth (as, for example, measured by number of obligatory arguments) may be the primary factor in tachistoscopic recognition, whereas semantic depth (as perhaps measured by associative meaningfulness) may be the primary factor in LTM tasks involving retention. (Baddeley (ibid.:141) notes that the levels-of-processing approach within the domain of semantics deserves a great deal more investigation.) These are rich areas for future research.
Finally, there are many related areas of research which can and should be linked to a levels-of-processing approach incorporating the minimum recall hypothesis. For example, do lexical items have multiple representations in the brain corresponding to different levels of syntactic complexity? Or, are the "levels" of complexity contained within a single lexical entry? This is perhaps a question for neuropsychology rather than linguistics.

Another area for research would be to see to what extent the processing complexity of additional arguments within a sentence can be overridden by automatization due to frequency of occurrence, etc. Are some automatized, multiple argument sequences processable as single argument units? My own previous work (1978) argues that this may be the case.

4.3. Areas for Future Research on Noun Facilitation

The above remarks bring me to the research needed on noun facilitation per se to put us in a position to address some of the interesting related issues discussed in this chapter. I will briefly summarize the possible directions for future research on noun facilitation in studies of adults, language acquisition and brain damage in the sections below.

4.3.1. Experimental Studies with Adults

To begin with, still further tachistoscopic studies are needed to compare SA and MA nouns—particularly with subjective familiarity and orthographic factors controlled.
Recall that in the Japanese experiment the SA nouns had lower thresholds than the MA nouns, and I argued that this was due to a greater subjective familiarity for the SA nouns. In the future, this difference between SA and MA nouns could be eliminated—given sufficient time and resources—by pre-test questionnaires on the subjective familiarity of a pool of stimuli by a very large number of subjects. Recall also that the non-adjusted thresholds of the SA nouns in my English experiment were lower than those of the MA nouns, and I argued that this was due to the greater orthographic confusability of the MA nouns (and perhaps the greater familiarity of the SA nouns.)

The orthographic factors discussed in Chapter 3 will be very difficult to control. Here, it might be worthwhile considering non-tachistoscopic studies that would not be influenced to such an extent by the visual characteristics of the stimuli. For example, a search task—finding a word in a list of distractor words—might be employed. There are also various LTM paradigms which might be attempted. However, as noted above (p. 252) there is a caution which must be heeded here. Namely, the effect of syntactic depth on the processing of isolated lexical items could very well be masked in a LTM task where the intrusion of other factors—such as semantic factors—would be more likely.

The remarks made above apply also to the status of SA verbs since the SA verbs in the Japanese experiment also had lower thresholds than the MA nouns—contrary to the minimum recall hypothesis. In Chapter 2, I argued that this was also due to a greater subjective familiarity for the SA verbs relative to the MA nouns. Recall also that it was not
possible to compile an adequate stimulus list of SA verbs for the English experiment.

Of primary interest for future research, however, is the exact status of SA/MA verbs in tachistoscopic experiments—particularly the relative recognition difficulty of SA/MA verbs like "write" and those like "grow" vis-à-vis each other and vis-à-vis SS-MA and US-MA verbs and SA and MA nouns. As pointed out in section 4.3.1., this area for future research is crucial not only to theories of noun facilitation but also to an understanding of parsing strategies. Here again, however, the problem will be to construct appropriately controlled stimulus lists. This seems very difficult indeed, given the necessity of controlling the possible effect of "set" (i.e., whether an SA/MA verb more often occurs with two or more arguments or with only one) discussed above (pp. 217-8).

Finally, an area of great relevance to theories of noun facilitation is the status of adjectives in tachistoscopic tasks. Recall that Holmes et al. (1971) found adjectives to have thresholds midway between nouns and verbs. Naturally, we would want to determine the effect of number of arguments on the tachistoscopic recognition of adjectives. I know of no adjectives parallel to SS-MA verbs in English in that they always require more than one argument at the level of surface structure. However, there do appear to be adjectives parallel to US-MA verbs in that they can occur with only one argument only in cases of shared information. This is the case with "envious," since (4-30) is grammatical only when we understand "of what" or "of whom."

(4-30) He is envious.

On the other hand, "greedy" appears to be an SA (or SA/MA) adjective. I
will come back to the status of adjectives vis-à-vis noun facilitation below in section 4.4.3.

4.3.2. Studies of Language Acquisition

Simply put, an interesting goal for future research would be to determine the role of number of arguments in language acquisition.

In Chapter 1 (p. 6) it was argued that part of the noun facilitation effect seen in the course of language acquisition may be due to it being easier for the child to learn to label concrete, unchanging things before abstract notions or objects subject to a great deal of change. It was also stated that there may or may not be a noun facilitation effect early in language acquisition above and beyond this labeling effect. My age-at-acquisition questionnaire for the Japanese stimuli was intended to produce some information in this regard. However, since the results of this questionnaire paralleled those of the subjective familiarity questionnaire, nothing could be interpreted with respect to the actual age-at-acquisition of the Japanese stimuli.

With respect to noun facilitation and language acquisition, three issues remain to be solved:

First, is there a noun facilitation effect beyond the (hypothetical) initial facilitation for the labeling of concrete, unchanging things?

Second, if the answer to the above question is affirmative, does number of arguments play a role in this noun facilitation?

Third, if the answer to each of the above questions is affirmative, does the semantic complexity of MA nouns make their age of acquisition equal to that of MA verbs, or are they acquired at the same time as SA
nouns and verbs? (See the discussion on pp. 143-5.)

With regards to this third question, there will be further complexities to investigate—as noted on p. 144. For example, can MA nouns be processed at the shallow level of SA nouns—as the evidence in this dissertation suggests for adults—as soon as they are acquired by the child or does this shallow processing ability require further practice and processes of "automatization" to become fully operative?

In any case, answers to the above questions—or analogous questions for adjectives or SA/MA verbs—cannot easily be solved by an examination of existing inventories of the vocabulary of young children, since the question here is not just whether children have a given MA word before another, but also whether children have an MA concept of the word's meaning and usage comparable to an adult's at various developmental stages. It is notoriously difficult to know what significance a very young child attaches to words in his or her vocabulary which happen to have the same form as adult words.

4.3.3. Studies of Linguistic Breakdown Due to Brain Damage

The issues which must be examined with brain-damaged people to advance our understanding of noun facilitation are basically the same as those outlined in section 4.4.1. for experimental studies with normal adults. In particular, it would be interesting to begin by seeing how an English-speaking brain-damaged patient showing a clear noun facilitation effect would perform when asked to read aloud the stimuli of my English tachistoscopic experiment—especially the SA/MA verbs. This is something I have not yet had the opportunity to try.
The status of adjectives in noun facilitation due to brain damage also needs investigating. As noted in Chapter 1 (pp. 18-9), Marshall et al. (1970) have observed some curious facts regarding adjectives in a dyslexic who showed a noun facilitation when asked to read words aloud. This dyslexic found it easier to read verb-derived nominals than the corresponding verbs, but more difficult to read adjective-derived nominals than adjectives. Verb-derived nominals and adjectives were almost equal in difficulty. (See discussion pp. 18-9.) These results contrast with Holmes et al.'s (1971) findings that 1) adjectives had tachistoscopic thresholds midway between verbs and nouns for normal adults; and that 2) a group of brain-damaged dyslexics found adjectives as difficult as verbs (see p. 18)! Clearly, there remain challenges here for any theory of noun facilitation (including the minimum recall hypothesis)—especially given the behavior of adjective-derived nominals reported in Marshall et al. (1970) (see p. 19).

An interesting aspect of future work on noun facilitation with brain-damaged individuals is that pointed out on pp. 23-4. Namely, tasks which are often too easy to reveal differential processing in adults often become taxing for the brain-damaged patient, and hence of interest to the investigator. This is the case with the "deblocking" technique of Weigl (1961) for example (see pp. 22-3). In the future it would certainly prove interesting to use this technique to compare the performance by brain-damaged persons (showing a noun facilitation effect) on MA nouns and SA/MA verbs in single argument sentences and in multiple argument sentences, along with their performance on SS-MA and US-MA verbs in appropriate multiple argument constructions. It would
also be interesting to see whether brain-damaged persons who show a noun facilitation effect perform differently on single argument sentences with SA or SA/MA verbs (which do not depend on shared information for grammaticality) and US-MA verbs (which do require shared information). Moreover, single argument sentences incorporating US-MA verbs could be tested with brain-damaged persons both with and without the experimenter providing the shared information.

4.4. Concluding Remarks

In this chapter I have summarized the data presented in Chapters 1, 2, and 3 with respect to competing theories of noun facilitation and areas for future research on noun facilitation.

In addition, I have tried to show that the cause of noun facilitation phenomena is not an isolated issue in search of a solution. Rather, I believe it to be a question of great relevance to psycholinguistics and neuropsychology in general.

Languages differ to some extent in terms of the characteristics associated with nouns and verbs. Just as the Japanese data in this dissertation have provided important clues toward an ultimate explanation of noun facilitation phenomena, it is to be hoped that similar investigations into a wide variety of languages will eventually result in a solid understanding of these phenomena and no small insight into universals of linguistic competence, processing strategies and--ultimately--the structure of the brain itself.
APPENDIX 1.

STIMULI USED IN THE JAPANESE TACHISTOSCOPIC EXPERIMENT

**Derivationally related SA noun-verb pairs**

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>SA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>fukurami &quot;bulge&quot;</td>
<td>fukuramu &quot;swell, bulge&quot;</td>
</tr>
<tr>
<td>hataraki &quot;work, function&quot;</td>
<td>hataraku &quot;work&quot;</td>
</tr>
<tr>
<td>*nokori &quot;remainder&quot;</td>
<td>*nokoru &quot;remain&quot;</td>
</tr>
<tr>
<td>hibiki &quot;sound&quot;</td>
<td>hibiku &quot;sound, echo&quot;</td>
</tr>
<tr>
<td>hikari &quot;light&quot;</td>
<td>hikaru &quot;shine&quot;</td>
</tr>
<tr>
<td>itami &quot;ache&quot;</td>
<td>itamu &quot;ache&quot;</td>
</tr>
<tr>
<td>kasumi &quot;mist&quot;</td>
<td>kasumu &quot;be misty&quot;</td>
</tr>
<tr>
<td>katamari &quot;lump&quot;</td>
<td>katamaru &quot;harden&quot;</td>
</tr>
<tr>
<td>katamuki &quot;incline, inclination&quot;</td>
<td>katamuku &quot;incline&quot;</td>
</tr>
<tr>
<td>koori &quot;ice&quot;</td>
<td>kooru &quot;freeze&quot;</td>
</tr>
<tr>
<td>*ugoki &quot;movement&quot;</td>
<td>*ugoku &quot;move&quot;</td>
</tr>
<tr>
<td>kumori &quot;cloudiness&quot;</td>
<td>kumoru &quot;get cloudy&quot;</td>
</tr>
<tr>
<td>kurashi &quot;living&quot;</td>
<td>kurasu &quot;live&quot;</td>
</tr>
<tr>
<td>kurushimi &quot;pain&quot;</td>
<td>kurushimu &quot;feel pain, suffer&quot;</td>
</tr>
<tr>
<td>nemuri &quot;sleep&quot;</td>
<td>nemuru &quot;sleep&quot;</td>
</tr>
<tr>
<td>sodachi &quot;upbringing&quot;</td>
<td>sodatsu &quot;grow up&quot;</td>
</tr>
<tr>
<td>*odori &quot;dance&quot;</td>
<td>*odoru &quot;dance&quot;</td>
</tr>
<tr>
<td>yasumi &quot;rest&quot;</td>
<td>yasumu &quot;rest&quot;</td>
</tr>
</tbody>
</table>

**Derivationally related MA noun-verb pairs**

<table>
<thead>
<tr>
<th>MA nouns</th>
<th>MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>chigai &quot;difference&quot;</td>
<td>chigau &quot;differ&quot;</td>
</tr>
<tr>
<td>kirikomi &quot;cut&quot;</td>
<td>kirikomu &quot;cut&quot;</td>
</tr>
<tr>
<td>konomi &quot;liking, preference&quot;</td>
<td>konomu &quot;like, prefer&quot;</td>
</tr>
<tr>
<td>kuyami &quot;condolences, regret&quot;</td>
<td>kuyamu &quot;condole, regret&quot;</td>
</tr>
<tr>
<td>mikomi &quot;hope&quot;</td>
<td>mikomu &quot;hope&quot;</td>
</tr>
<tr>
<td>miokuri &quot;seeing off&quot;</td>
<td>miokuru &quot;see off&quot;</td>
</tr>
<tr>
<td>nayami &quot;worry&quot;</td>
<td>nayamu &quot;worry&quot;</td>
</tr>
<tr>
<td>negai &quot;wish&quot;</td>
<td>negau &quot;wish&quot;</td>
</tr>
<tr>
<td>nerai &quot;aim, goal&quot;</td>
<td>nerai &quot;aim for&quot;</td>
</tr>
<tr>
<td>shiharai &quot;payment&quot;</td>
<td>shiharau &quot;pay&quot;</td>
</tr>
<tr>
<td>tanomi &quot;request&quot;</td>
<td>tanomu &quot;request&quot;</td>
</tr>
</tbody>
</table>

* Stimuli marked with an asterisk indicate words used in the first reading aloud test with OR and replaced in the tachistoscopic experiment by the words under which they are listed.
tatakai "fight"
    *kaeshi "something returned"
tsunagari "connection"
urami "resentment"
yurushi "permission"
    *sasoi "invitation"

tatakau "fight"
    *kaesu "return"
tsunagaru "connect"
uramu "resent"
yurusu "permit"
    *sasou "invite"
APPENDIX 2.

STIMULI USED IN THE ENGLISH TACHISTOSCOPIC EXPERIMENT

<table>
<thead>
<tr>
<th>SA nouns</th>
<th>MA nouns</th>
</tr>
</thead>
<tbody>
<tr>
<td>ant</td>
<td>danger</td>
</tr>
<tr>
<td>bath</td>
<td>debt</td>
</tr>
<tr>
<td>beauty</td>
<td>duty</td>
</tr>
<tr>
<td>blood</td>
<td>enemy</td>
</tr>
<tr>
<td>gym</td>
<td>gift</td>
</tr>
<tr>
<td>hat</td>
<td>greed</td>
</tr>
<tr>
<td>life</td>
<td>joy</td>
</tr>
<tr>
<td>morning</td>
<td>opinion</td>
</tr>
<tr>
<td>mouse</td>
<td>pal</td>
</tr>
<tr>
<td>music</td>
<td>proof</td>
</tr>
<tr>
<td>night</td>
<td>purpose</td>
</tr>
<tr>
<td>ocean</td>
<td>ratio</td>
</tr>
<tr>
<td>odor</td>
<td>son</td>
</tr>
<tr>
<td>pyramid</td>
<td>thief</td>
</tr>
<tr>
<td>salad</td>
<td>truth</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>SS-MA verbs</th>
<th>SA/MA verbs</th>
<th>US-MA verbs</th>
</tr>
</thead>
<tbody>
<tr>
<td>admit</td>
<td>beg</td>
<td>argue</td>
</tr>
<tr>
<td>adopt</td>
<td>*begin</td>
<td>ask</td>
</tr>
<tr>
<td>bring</td>
<td>carve</td>
<td>enter</td>
</tr>
<tr>
<td>bury</td>
<td>compose</td>
<td>err</td>
</tr>
<tr>
<td>contain</td>
<td>eat</td>
<td>excel</td>
</tr>
<tr>
<td>infer</td>
<td>*grow</td>
<td>hear</td>
</tr>
<tr>
<td>lend</td>
<td>*prosper</td>
<td>inquire</td>
</tr>
<tr>
<td>mow</td>
<td>*relax</td>
<td>join</td>
</tr>
<tr>
<td>obtain</td>
<td>:retire</td>
<td>know</td>
</tr>
<tr>
<td>opt</td>
<td>sing</td>
<td>learn</td>
</tr>
<tr>
<td>prove</td>
<td>*sit</td>
<td>listen</td>
</tr>
<tr>
<td>provide</td>
<td>teach</td>
<td>plead</td>
</tr>
<tr>
<td>put</td>
<td>think</td>
<td>prepare</td>
</tr>
<tr>
<td>solve</td>
<td>*weep</td>
<td>pry</td>
</tr>
<tr>
<td>warn</td>
<td>write</td>
<td>react</td>
</tr>
</tbody>
</table>

* The SA/MA verbs marked with an asterisk are semantically intransitive when used as SA verbs. The SA/MA verbs without an asterisk are semantically transitive even when used without a direct object. (Note that "begin" must have an inanimate subject to be semantically intransitive.)


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