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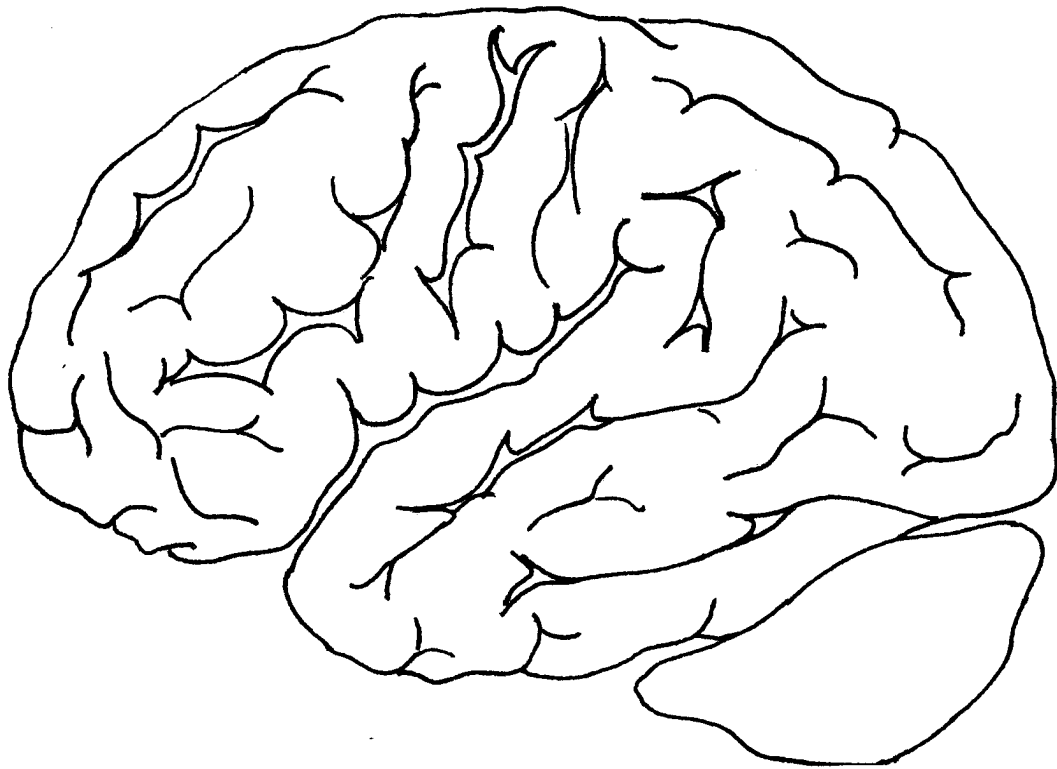
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**ON  
THE REPRESENTATION  
OF LANGUAGE**



**IN THE HUMAN BRAIN**

**harry a. whitaker**

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On the Representation of Language in the Human Brain:  
Problems in the Neurology of Language and  
The Linguistic Analysis of Aphasia

by

Harry Allen Whitaker

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To Vicki

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## Chapter I. The Theoretical Framework

### A. Historical Background

The scientific study of language and language behavior has an extremely variegated history and certain aspects of the development of physiological psychology, aphasiology<sup>1</sup> and contemporary linguistic theory -- particularly experimental phonetics, are quite relevant to the topics which will be discussed in this dissertation. A rather interesting interpretation of the history of psychology in this century was made by D. O. Hebb [1963]<sup>2</sup>; he noted that the first work in physiological psychology to have established the methodology used today in this field was Karl S. Lashley's monograph (Lashley [1963]), *Brain Mechanisms and Intelligence*. According to Hebb, psychology since Wilhelm Wundt had been rather fanciful neurologizing.<sup>3</sup> It was thought, for example, that single memories were stored in single nerve cells, it was debated whether or not consciousness was the result of high or low resistance at synaptic junctions, and it was presumed that there existed a tidy neural circuit for each and every habit. Learning was thought to be a process of establishing these circuits, or "reflex arcs", in the cerebral cortex; in short, consciousness was equated with cortical. Such imaginative theories about the central nervous system were the stock-in-trade of physiological psychology up to 1929. Lashley's monograph explored the relationship between learning and retention and surgically caused lesions of the cerebral cortex of the rat. His results seemed to show that former notions about isolated and locatable cortical networks for specific behavior was fiction. In analyzing maze behavior in the rat, he found that this learned behavior was differentially impaired by the *extent* of the cerebral cortex removed and *not* by the *area* (location) of cortex removed. In other words, the more cortex removed, the greater the impairment of the maze habit. These findings exploded most of the myths of physiological psychology to that date. As Hebb noted, the vacuum left by Lashley's results, for all intents and purposes was filled by the great behaviorists -- E. C. Tolman and B. F. Skinner. Tolman and Skinner, as is well-known, argued that it was not only unnecessary to discuss the nervous system in studying behavior, but in fact such an approach hindered adequate behavioral analysis, since it was assumed that the *organism* contributes little or nothing to observed behavior. Thus it can be argued that behaviorism in psychology gained prominence as much due to its intrinsic theoretical arguments as due to Lashley's vitiating of the traditional physiological psychological theorizing of the late 19th and early 20th centuries.

There are two interesting sequels to this historical development: the attack on behaviorism and the re-interpretation of Lashley's original hypotheses in physiological psychology. Lashley himself delivered the first significant critique of the behaviorist approach to higher-order nervous system functions in his 1951 article "The Problem of Serial Order in Behavior (Lashley [1961])." As is generally accepted today, he showed that the stimulus-response hypothesis which had been a mainstay in behaviorism<sup>4</sup> was inadequate for explaining anything as complex as learned voluntary behavior. The details of his critique need not concern us here; however, it is interesting to observe that they went largely unnoticed for nearly a decade. It is clear from reading Lashley<sup>5</sup> that he never intended to dismiss physiologically-based research and in fact he makes explicit reference to physiological facts throughout his 1951 article. Thus it is worth noting that the rejection of behaviorism by contemporary linguists is in part for the same reason Lashley rejected it. The basic tenet of behaviorism is that all responses can be fully explained (predicted) from a knowledge of the stimuli which precede them; the organism itself makes no contribution of its own to behavior. Lashley's paper demonstrated that this model was theoretically inadequate in accounting for human behavior and that in fact it was necessary to study physiological principles of the organism. Although no serious consideration in linguistic theory has yet been given to physiological psychology, it should be noted that Chomsky [1959] also rejected behaviorism because of its doctrine that the organism plays a minor role in complex behavior. He notes:

... elimination of the independent contribution of the speaker and learner can be achieved only at the cost of eliminating all significance from the descriptive system  
... [1959: p. 168]

Interestingly, Chomsky indicates in his review (Sections 5 and 11) that linguistic theory must be concerned with the properties of the organism and the contributions it makes to behavior, just as are psychology and neurophysiology. By this he means to indicate that linguistic hypotheses, and psychological hypotheses and neurophysiological hypotheses ought to be compatible. Katz [1964] expresses the same views but in more detail:

First, since the psychologist and the mentalistic linguist are constructing theories of the same kind, i.e. theories with the same kind of relation to the neurophysiology of the human brain, it follows that the linguist's theory is subject to the requirement that it harmonize with the psychologists theories dealing with other human abilities and that it be consistent with the neurophysiologist's theories concerning the type of existing brain mechanisms. [p. 133]

Further, by subjecting a linguistic theory to this requirement we make it more easily testable. For the requirement enables us to refute a linguistic theory if we can find psychological theories or facts that are inconsistent with

it or neurophysiological accounts which describe brain structure in a way that precludes the linguistic theory from being isomorphic to any of the structures in the human brain. [p. 134]

As described above, the mentalist explains facts about a speaker's and hearer's linguistic performance in terms of a model that reconstructs the process by which a message is transmitted from the speaker to the hearer through the speaker's encoding the message in the form of an utterance and the hearer's decoding that utterance back to the speaker's original message. Such a model explains why an utterance has a certain linguistic property, and what function that property has in the process of communication, by locating the property in the causal chain which links the utterance on one side to the neurophysiological mechanisms that perform the encoding and articulation, on the other side to those that accomplish the perception and decoding. [p. 135]<sup>6</sup>

With respect to the re-interpretation of Lashley's original hypothesis that the amount of cortex was of more significance in behavior than the particular area of the cerebrum, only a few general comments can be made here. First, it is important to realize that Lashley's original experiments were based upon only the grossest interpretation of a fairly complex behavior -- success or failure in completing a maze task. This might be comparable to judging whether or not a person can complete a general intelligence test with a certain predetermined score. In either case, damage to the cerebral cortex in just about any area will affect some aspect of the performance of the test, but what aspect is affected may be different depending upon the locus of the lesion. If the measurement is simply a final summarizing score, then it is not surprising that the larger the lesion the poorer the score. Once a general behavioral skill is broken down into sub-components, however, a different picture emerges. To some extent, Lashley's own experiments were repeated but with an analysis based upon more fundamental psychological abilities by Gross, Chorover and Cohen [1965] and a clearer picture of the cortical localization of functions with respect to maze-ability was demonstrated. They distinguished sensory-discrimination ability from response-alternation ability, which were correlated with posterior and frontal cortical regions, respectively. The above remarks illustrate in a small way a traditional controversy of this century in physiological psychology, neurology and related disciplines: is there cortical localization of function? With respect to the representation of language in the brain, this subject will be taken up in some detail in Chapter II.

The historical antecedents to current linguistics and the development of aphasiology, both of which bear directly upon this dissertation, can be quickly summarized. For nearly a century man has known that specific disorders of language were associated with specific damage to

the human brain.<sup>7</sup> Neurologists were quick to employ this knowledge to study correlations between language behavior and neuroanatomy; later psychologists, to a lesser extent, began studying aphasic language in order to obtain data bearing upon the relationships between neural structures and psychological mechanisms. However, most linguists have shown remarkably little interest in brain mechanisms related to language -- notwithstanding the theoretical claims noted above -- or for that matter, in any of the several disciplines related to the study of the brain. One exception to this generalization is the field of experimental phonetics, which has begun to give careful attention to the peripheral nervous system.<sup>8</sup> There is a growing body of serious research on the structure and function of the respiratory system, the auditory system, the laryngeal area and, to a lesser degree, the oral cavity. Recently some workers in these areas have begun relating the peripheral mechanisms to mechanisms of the central nervous system. For example, see E. Dunker [1968] in which the anatomical connections of the extrapyramidal system which pertain to the laryngeal musculature are discussed and some functional hypotheses are suggested. A second exception to this generalization is the field of psycholinguistics in which one might identify two general trends with respect to aphasiology and the neurological correlates of language. The first principally relies on statistical methods which are used to relate normal and aphasic language behavior according to general psychological hypotheses about verbal behavior -- for example, word associations, word frequencies, response latencies, and the like. There is an extensive literature in this area of psycholinguistics and as would be expected a great number of books and articles discussing aphasia from this point of view.

A second trend in psycholinguistics, of very recent development, is serious consideration of aphasic language in terms of linguistic hypotheses. For example, Reiff and Tikofsky [1967, 1968] have compared judgments of grammaticality between normals and aphasics and examined several aspects of linguistic competence. Blumstein [1968] is currently studying the phonology of aphasic language in terms of phonetic features. Doktor and Taylor [1968] have done some research on possible hierarchical relationships between transformational rules in aphasic language. Weigl and Bierwisch [1968] have considered some general grammatical properties of aphasic language, with particular emphasis on different modalities such as reading and writing, with some reference to semantic and syntactic features and syntactic rules. Since the study by Weigl and Bierwisch most touches upon the substance of this research, a more detailed comment on this paper will be made in Chapter III, as well as a summary of these other papers.

In general the above studies represent most of the first efforts into a relatively new area of empirical research in linguistics. I stress "relatively" since there have been some earlier studies which were not mentioned. R. Jakobson ([1966], [1968]) has long been interested in aphasia, as is well-known, but for all intents and purposes his studies have been neither empirical nor particularly explanatory. His major

concern seems to be in providing aphasiologists with a linguistically-oriented descriptive nomenclature for the types of aphasic disturbances. Since this is far removed from the main topics in this dissertation, there has been no effort to evaluate his studies. There is also a dissertation on linguistic analysis of aphasia, approached from the point of view of tagmemic theory (Barrett [1961]); not much use was made of this study since its objectives were primarily to devise a test procedure for obtaining an acceptable (in tagmemic terms) corpus of aphasic speech. As one would expect, not only the methodology of studying aphasics but the relevance and significance of data so obtained is not well-established. This dissertation in part is in this new area -- the application of linguistic hypotheses to aphasic language data (Chapter IV); in part, too, this dissertation is in the area of physiological psychology, specifically the neurological correlates of language (Chapter II). Because the methodology is not firmly established and because the results are not immediately interpretable within current linguistic research frameworks, the balance of this chapter is devoted to a theoretical discussion in which these and related problems are discussed in greater detail. The full specification of the linguistic hypotheses which will be examined in light of aphasic language is in Chapter IV.

#### B. Some General Problems in Linguistic Theory

Even from such a brief historical sketch as the preceding, it is evident that the study of the structure of language as human beings employ it is an interdisciplinary task. The tradition or perspective in Linguistics which is accepted here is that hypotheses about the phonological, syntactic and semantic structure of language *in fact* represent an underlying psychological reality. As Katz [1964] observed, this assumption clearly commits linguistic hypotheses to be compatible with hypotheses about the structure and function of the human brain. And this latter assumption entails, at a minimum, some knowledge about the structure and function of the human brain to begin with -- the neurological bases of language which will be discussed in Chapter II. At least one thing which is contingent upon these assumptions is that, since there are several approaches to the study of the brain, there are no small number of simply technical and terminological problems; few of the several disciplines in the neurosciences have a common set of basic principles and jargon, which of course is in part due to emphasis on different aspects of the brain -- metabolic, cellular, electrical, etc. This is painfully apparent whenever the subject of "behavior" is broached. The disciplines in the neurosciences which are pertinent to this dissertation are: linguistics<sup>9</sup>, psychology, neurology, anatomy, physiology and biochemistry. Since there is no way to sort out either terminological or disciplinary incompatibilities -- given the space and time limitations of this study -- some liberties will be taken which, hopefully, will facilitate the thesis without any serious distortion. The terms

'linguistic' and 'psychological' will be used when behavioral aspects of language are being considered and the term 'neurological' will be used when physical correlates of such behavior are being discussed. This usage implies that the major interface is between behavioral representations<sup>10</sup> and physical representations.<sup>11</sup>

There is little question but that Linguistics, as usually understood, can proceed to study language independently of neurology; most of the work done in the field to date is evidence of that. However, it is being increasingly noticed that there may be some need to consider neurological hypotheses more carefully. This awareness seems to be motivated in some measure by a need for constraints on linguistic hypotheses which otherwise are too 'powerful' in the sense that they become mathematically viable for generalizing about the data at the expense of being neurologically viable in accounting for the way in which the brain uses language. At a conference on syntax and semantics [La Jolla, 3-1-69] S. Peters made some remarks on the excessive mathematical power of transformational rules such that it could be proven by theorem that the base component could be absolutely universal in a trivial way because the transformational component could mathematically represent all the idiosyncracies of syntax which are actually found in the world's languages. However, such a 'grammar' would be unnatural in some ill-defined psychological sense. Peters then noted that linguistic theory is in need of further constraints based on "general psychological principles"; these must be very strong, substantive constraints, not necessarily part of our "linguistic intuition,"<sup>12</sup> which have the effect of actually *enlarging the class of data for which linguistic theory is held accountable*" (emphasis mine). Although Peters was not too explicit as to what he meant by this remark, I interpret it in roughly the same spirit of what Katz [1964] was arguing (cited above) namely that a linguistic model of language and a neurological model of language must be compatible with each other. It would then follow that neurological data, and this would include but not be limited to aphasic language behavior, represents an enlarged domain for linguistic theory. Chomsky seems to agree with this assumption, as indicated in the following remarks (from the discussion following his paper in Millikan and Darley [1967]):

... sooner or later -- in some areas sooner, in other areas later -- it is going to be necessary to discover conditions on theory construction, coming presumably from experimental psychology or from neurology, which will resolve the alternatives that can be arrived at by the kind of speculative theory construction linguists can do on the basis of the data available to them.

... My feeling is that unless the character of psychological research changes fairly radically, not much help is going to come from psychology because of an insistence on attention to peripheral behavior and a refusal to try to do what every other science does, namely, to try to find some underlying theories that will account for behavior. [p. 100]

The above points delineate a major problem in linguistics today: linguistic theory has some fairly strong analytical devices for making generalizations about the structure of language which, at the moment, are not sufficiently constrained by hypotheses about the nature of human beings, how human beings actually use language or about the structure and function of the human brain. The danger is that the linguistic hypotheses which comprise the grammar of a particular language (or which comprise a theory of language universals) could in part be a reflection of the analytical apparatus itself instead of what is termed the 'linguistic competence' of a speaker of that language. Before turning to a discussion of the general framework of linguistic theory and some proposed modifications of it -- the competence/performance dichotomy -- it would be profitable to consider some different issues which bear directly upon the relevance of neurological data to linguistics. First, it is not unreasonable to suggest that there are a priori grounds for bringing neurological information to bear upon linguistic theory and particularly, the current model of linguistic competence. Ultimately we have to. Certain structures and functions of the nervous system are the substrate of both our "knowledge" and our "use" of language. Second, it has been persuasively argued (Chomsky [1967a], [1967b], Lenneberg [1967], Smith [1969]) that only man among the animals has a language which is creative, free from external stimuli, non-finite, etc. This claim does not need to be questioned; it can be convincingly demonstrated on empirical grounds. Put another way, human language is qualitatively different from animal language. The interesting question, however, is to find an explanation for this fact. One could suggest a priori philosophical reasons for the fact that man has language and animals have only finite communication systems, but such explanations in general will not bear up under scientific scrutiny. The fact is assumed in linguistic theory; it is difficult to imagine how linguistic theory as presently formulated could provide an explanation since, because of the assumption, it only encompasses human language. On the other hand, it is relatively easy to imagine how neurological theory could provide an explanation.

At face value one might not think too much of the assumption that human language is qualitatively different from animal language. Notice, however, that there are some contingent assumptions of considerable importance. Human language behavior could be thought of as merely another type of cognitive behavior in man or animals -- one of the tenets of behaviorism; alternatively, it could be thought of as a special case. This in turn relates to whether one considers language acquisition just another aspect of learning phenomena or something different. Linguists have assumed that language is something special to human beings, that language acquisition is a unique human phenomenon and that therefore these can only be studied experimentally by studying man himself.<sup>13</sup> Furthermore, linguists often seem to implicitly assume that language use is a special human capacity -- i.e. that it is not even parallel to other human abilities such as vision, tactile sense and the like. Yet, in some sense it does seem to be parallel to certain 'cognitive' abilities such as playing chess (although clearly different in terms of acquisition). Not a



little of the difficulty in assessing such assumptions is the pervasiveness of the language system; the visual and tactile systems are both adaptable to linguistic manipulation since we do read and write. And certainly the above set of assumptions bear upon the question of the degree to which linguistic structure is innately specified. It would be peculiar indeed to talk about a "species-specific" ability which had no genetic specificity or a structural-functional uniqueness in the brain. At this juncture it hardly needs to be reiterated that neurological hypotheses ought to *fit* in with linguistic hypotheses; it would be more appropriate to ask *how* than *why*. Unfortunately there are a great number of lacunae in our knowledge of the neurological correlates of any type of behavior, much less of language; given the present state of knowledge it would be hard to propose how membrane permeability relates to negation.<sup>14</sup>

### C. Competence and Performance

Although the case for studying the neurological aspects of language may be convincing, it is not quite clear how the present framework of linguistic theory -- particularly the standard differentiation between competence and performance -- can accommodate or incorporate such data. One need hardly be reminded that the discussion in linguistics concerning the distinction between competence and performance has been protracted and seemingly unresolvable, that it has involved philosophers (see Moravcsik [1967], Harman [1967]), psychologists (see Osgood and Miron [1963], Jakobovits and Miron [1967], Lenneberg [1967]) and of late, although less directly, neurologists (see Weigl and Bierwisch [1968], Luria and Tsvetkova [1968]) is interesting, not too surprising, and simply reiterates the arguments presented in the preceding pages. Some general considerations of the debate reveal why this is the case. Note that the issue is purely theoretical -- there is no way to empirically distinguish competence from performance per se since the distinction itself determines what constitutes empirical evidence; once formulated, however, it is possible to cite data as evidence for or against a particular view in the sense of the reasonableness of that view. The delineation of what constitutes empirical evidence in linguistic theory is of fundamental importance for in effect it draws the line between what is of linguistic concern and what is not; more specifically, the competence-performance distinction defines the scope of hypotheses about the structure of language.

It is improbable that this issue has a correct and final solution, given the preceding considerations, for it is characteristic of fundamental theoretical distinctions that in the course of time they are altered to suit new discoveries and new approaches to the study of a discipline; in part this may be what gives "life" to a science. Thus, whenever new sources of data or evidence are suggested, the fundamental theoretical premises must be made again and revised in order to accommodate

the new ideas. The research and theoretical speculations in Chapters II and III are offered in just this way; they comprise, for linguistics at any rate, a new source of data and evidence and entail hypotheses of a qualitatively different nature. In order to place them in linguistic perspective, I will first turn to the standard competence-performance distinction and then suggest how it could be modified to incorporate this data; this seems to be the most fruitful way of approaching the questions which were posed in sections A and B above.

The current formulation of the notions competence and performance appear to place neurological data such as will be considered later under the rubric of performance. If one maintains this it may be necessary to argue that hypotheses of performance should constrain hypotheses of competence. Alternatively, the notion of competence might be revised to show that analyzable data and hypotheses of a behavioral and neurological nature are to be included in such models. Before making a decision on this, a brief review of the current framework is necessary. My general impression is that the work done to date in linguistics, phonetics, psycholinguistics and psychology on models of language in one form or another can be classified as in Figure 1 (from Fromkin [1968]).

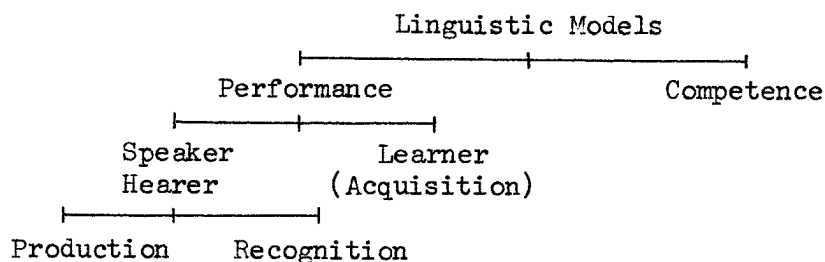


Figure 1. Linguistic Models

Cross-classified with these points on the chart are such aspects as the depth of the analysis, for example the nature of deep and surface structures. Chomsky's ([1965] pp. 3-9) formulation of the competence-performance dichotomy, which is the primary one to which other formulations refer, can be summarized as follows. Linguistic competence is the ideal speaker-hearer's perfect knowledge of his language, assuming a homogenous speech community and unaffected by grammatically irrelevant factors such as memory limitations, shifts of attention and errors. Linguistic performance is the actual use of language in concrete situations; performance is a direct reflection of competence only under the idealizations just mentioned because the record of actual speech will show numerous false starts, deviations from rules, slips of the tongue and the like. A full account of competence includes the particular grammar of the speaker-hearer's language, supplemented by a universal grammar which accounts for such regularities that are found in all languages, and the creative aspect of language. The grammar, that is the

generative grammar, will be considering mental processes, faculties or abilities which transcend the level of actual or perhaps even potential consciousness. How the speaker-hearer actually proceeds to construct or interpret a sentence in context belongs to the theory of language use -- the theory of performance. A performance model will incorporate as a basic part the generative grammar of the language in question; however, the grammar does not prescribe the character or the functioning of either a model of speech production or a model of speech recognition. Included in the notion of performance are such matters as memory limitations, semi-grammaticality and stylistic devices. In short, a competence model is idealized -- the errors have been removed -- and it is static -- it models structure; it also is constrained to be consistent with physiological and psychological theories of language.

The implication is clear that neurological data in the current formulation is considered part of a performance model, although this could merely be an artifact due to the fact that the neurological data considered so far by linguists and phoneticians has been of a somewhat rudimentary nature; e.g., bearing upon the musculature of the speech apparatus or the acoustic-electrical transformation in the auditory periphery.

I think the above sums up most of what linguists have said on the subject; it is apparent that this is not sufficient, particularly with respect to the relationships between linguistic hypotheses and neurological hypotheses about language. The theoretical framework proposed here, based upon the viewpoint just outlined, provides some approaches to this crucial relationship, I believe. The usual interpretation of the notion linguistic competence is, in my opinion, too narrow; conversely, the usual interpretation of the notion linguistic performance is too broad. When we speak of the idealized knowledge a speaker-hearer has of his native language, what is really meant is little more than the representation of semantic, syntactic and phonological regularities in sentence structure. The linguist represents this as a grammar composed of three components which have certain formal and substantive properties, for which utterances in the language constitute evidence. Linguists are not very specific as to how these utterances are obtained as data, although we can identify perhaps three broad contexts: (1) intuitions about one's own dialect, (2) informal questioning of someone else's dialect and (3) more or less structured field methods. What is not included in the notion of competence, rightly so in my view, and at the same time what is apparently part of the total knowledge a speaker-hearer has of his language, are such things as the global context of a conversation, discourse context, the intention of the speaker, various presuppositions of both speaker and hearer, psychological states, emotions, attitudes, certain types of referential information, real-time phenomena and so forth.<sup>15</sup> These are not trivial, but at present they are not readily amenable to the analytical methods of linguistics and possibly for that reason are currently excluded from the model. The deplorable result is that occasionally one subjectively divides the several variables

which influence how a person produces and recognizes language into two classes labelled competence and performance;<sup>16</sup> if it is possible to study a variable -- a class of facts -- in a systematic way, that is, if it is possible to interpret it with a generalization or a rule, then it is assigned to the domain of competence, and if one cannot interpret a variable in this fashion, it is assigned to the domain of performance. This is surely an unsatisfactory basis for distinguishing competence from performance and performance would make it impossible to ever develop a model of performance.

Clearly performance is much more than an inventory of errors in speech production, lapses of memory or memory limitations, although such variables are certainly relevant data for gaining insights into the problem. Performance should, I think, be broadly conceived as language behavior and consequently, a performance model should be couched in terms of a representation of the production and recognition of language by a speaker-hearer. In this view, the four modalities which we customarily associate with verbal behavior -- the visual and auditory systems in the recognition of language and the speech and writing systems in the production of language -- would be the primary components of a performance model. In this view the competence which linguists investigate forms what one might term a "core" or the central part of performance and is equated with the representation of language in the central nervous system. A diagram (Figure 2) will summarize the above outline more cogently:

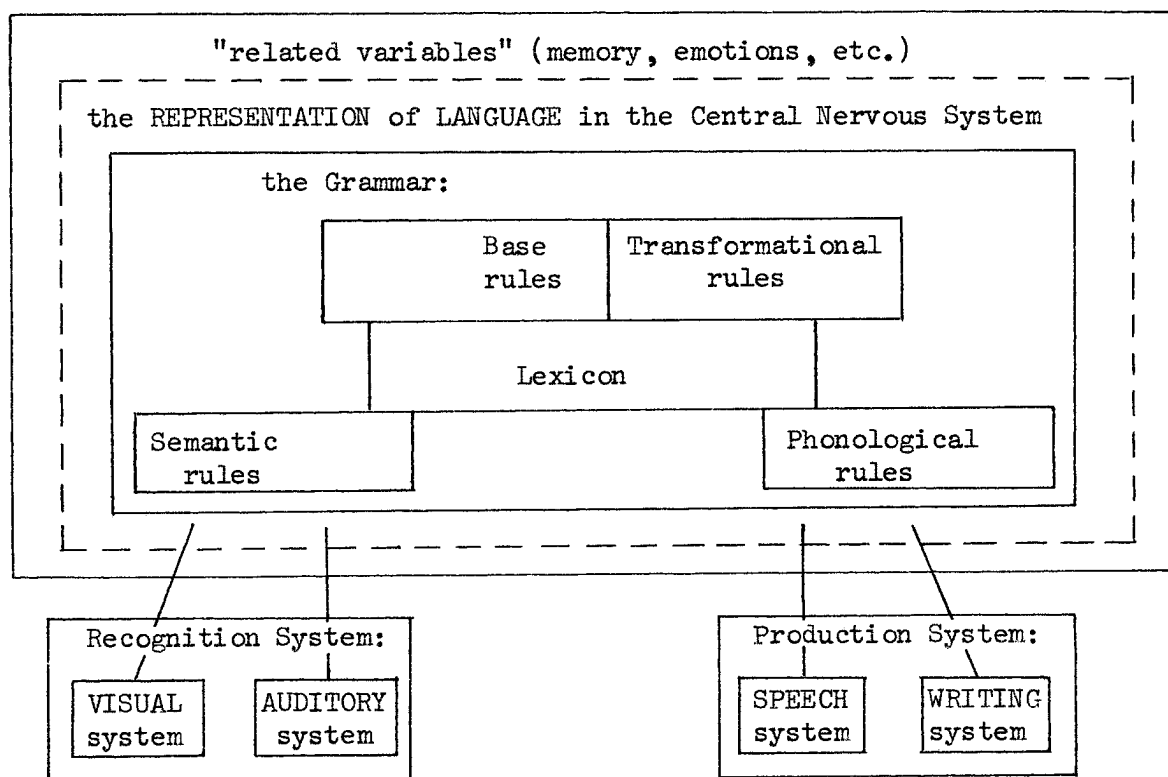


Figure 2. Schematicized Model of Linguistic Structure

The above schematic would claim that whereas we may not proceed through a set of rules in the process of uttering a sentence and whereas we may not proceed back up through the rules to understand a sentence, there *is* a point at which the production and recognition systems of the speaker-hearer make use of the abstract system of rules. The schematic would claim that competence -- the main subject matter of linguistics -- is concerned with the representation of language in the central nervous system; for if competence is a speaker's knowledge of his language and a speaker's knowledge of his language is essentially his *ability* to produce and recognize sentences in his language, then a 'full account of competence' includes the grammar and such other aspects of the representation of language which can be shown to constrain grammatical hypotheses. To repeat: the theoretical position which seems to be the most interesting, that is, the position which has the strongest empirical consequences, is that the competence model as outlined is in fact a direct representation of the center box in the model of linguistic structure. This is a claim that linguists are directly investigating the representation of language in the central nervous system. In this view, competence is not idealized performance, or what you would expect verbal behavior to be if there were no errors, memory limitations, etc.; it is one of the components underlying the use of language and as such can be directly investigated by, as noted above, intuitions about one's own dialect, by informal questioning of another person's dialect, from more structured field methods, or as I hope to demonstrate shortly, by considering the neurological bases of language structure and by the analysis of aphasic speech.

The above schematic would claim that performance, the actual use of language, would be involved minimally in the nature of the production and recognition systems which subserve competence. Thus performance will have neurological correlates, too, and additionally will be concerned about a number of phenomena or variables related to the use of language such as some aspects of memory, emotional states, context, etc.

There are to be sure some qualifications required in interpreting this diagram literally, which I won't attempt to develop here. A perhaps less obvious problem that ought to be at least mentioned has to do with the nature of the hypotheses which comprise the model. Obviously linguistic hypotheses are formal entities -- rules and the elements which enter into these rules -- which implies that the grammar ultimately will be a deductive system in the logical sense. This is nothing more than to say that the precise derivation of sentences (actually, structural descriptions) in the grammar is fully stated in the rules of the grammar and can be calculated without reference to anything else. What is not clear is whether there are any corresponding psychological or neurological mechanisms which are also rule-governed. It is commonly assumed that behavior is rule-governed but to my knowledge it is only assumed, not proven. It is just as feasible to assume that the brain operates with a set of strategies by which, for example, the production and recognition of language is achieved by close approximations rather than exact matchings.

Suppose that human beings stored whole words in the brain (which is suggested by cases of 'naming' aphasia) which were linked to each other by a system of neural re-coding that constitutes semantic association. Analyses of neuronal spike trains (see Perkel, Gerstein and Moore [1967]) indicate that several levels of coding is entirely feasible.

This is also observed by Bullock [1967] who distinguishes local from long-distance events in the nervous system; all but the neuronal spike trains are local. Bullock suggests the following about possible coding of spike trains:

But knowing these (integrative) properties does not tell us what code is used. There are several a priori possibilities, given all-or-none events of variable number and spacing. Note that this is in no sense a digital communication link, but a pulse-coded analog system. The intervals between spikes are not quantized but are continuously variable. Because only number and spacing can change, it has long been assumed that there are only two ways messages can differ -- in number and in frequency (spikes per unit time, arbitrarily chosen). Only recently has it begun to register that there are other ways, including the degree of variance of the intervals about the mean, the shape (symmetry, number of modes, etc.) of the distribution of intervals about the mean, the presence and sign or the absence of autocorrelation of successive intervals, the possibility of systematic temporal microstructure ("patterns") in impulse trains. Each of these has been found and is thereby a candidate code -- although not ipso facto a code, because we must show that the postsynaptic cell reads or discriminates the feature in question, when other features are invariant ...

The finding of broad general interest is that there is not one code but several. The average frequency is doubtless the code in some fibers. In others a number code is employed -- the number of spikes following each stimulus measures its intensity; the individual intervals do not systematically vary, and the average frequency is contaminated by the variable rate of recurrence of stimuli. A latency or phase code is found ... some fibers appear to be like doorbells and simply carry a presence or absence signal. [p. 351]

... the cell may be integrating spatially as well as temporally, cross-correlating the converging inputs and therefore changing the meaning of the signal in the output.  
[p. 352]

Although the *capacity* and probably certain *attributes* of nerve impulse propagation is innately determined (that is, genetically specified), the neural re-coding would be *learned*; it would be influenced

not only by the language of other speakers but by all the sensory modalities. This neural re-coding may comprise both 'meaning' of words and phrases and 'reference' to experience, although it is possible that the brain carries out these two operations separately. In many cases, frequently used re-coded patterns would be in turn codified into higher level *single units*, e.g. the commonly used phrases in ordinary speech such as idioms. The aphasic, for example, may have a fairly large stock of idiomatic expressions which he is able to utter clearly at a normal speech rate and yet be practically unable to use language creatively as a normal person; these idiomatic phrases have been referred to in the literature as "ictal speech automatisms" (see Chase [1967]) and are discussed in more detail in Chapter IV. So far, the postulated system is open-ended in at least two respects: more words may be added and more neural hook-ups may be formed. Minimally, these can be regarded as part of the memory capacity and part of the symbolizing capacity of man's brain, both of which deteriorate with age but have no *known* limits. Memory is, of course, presumed to be finite on theoretical grounds. One would expect that whatever re-coding takes place is paralleled by a re-coding of the connections to the motor cortex and hence the neural commands that activate the speech musculature; these articulatory commands, first associated with individual words and first overtly learned during language acquisition, by and large are unconscious for the adult speaker. This might be accounted for by assuming that a different type of coding is done by the motor cortex, one which analyzes and transmits the neural commands for words in terms of syllable types (with segmental and feature constituents) which become more or less fixed as neuromuscular habits for the specific language in question. Next, assume that there is a fixed number of stored brain *routines* for *assembling* the semantic units (the neural hook-ups) into a sequential order. In memory storage semantic units are not necessarily linearly arranged. These routines would be *strategies* for the production and recognition of speech, not rules in the formal sense. The distinction between strategy and rule is not an easy one to make, but could be approached as follows.

A rule-governed operation requires that all features of a given signal be computed in full with the result that a match is complete and in a one-one correspondence (where *match* may be either for production or recognition purposes, and implies the relationship between conceptual neural units and the neural signals associated with speaking or hearing). A strategy-governed operation requires only that the salient features of a signal are computed so that a decision (not a match) may be made as to the nature of the signal; minor details or perhaps even ambiguities are disregarded. For some additional views on rules and strategies see Moravcsik [1967] and Dreyfus [1965].

It can then be postulated that the strategies for assembling the semantic units worked bi-directionally in that they could associate the sequentially assembled semantic units either with the auditory cortex or the speech motor cortex, depending on whether the system is in the recognition or production mode of operation. As noted, the articulatory sub-system is largely an unconscious operation, comprised of neural

commands (habits) that have been worked out in a sophisticated way. It might be assumed that general speech production is 'monitored' through a *perceptual* feedback procedure which functions in time segments no smaller than the syllable (see Kozhevnikov and Chistovich [1966]) and probably in segments of word or morpheme size, although there is certainly a great deal of feedback control that is not computed but automatic (see MacNeilage [1968]). We may then reason that the only real conscious control over this system is in terms of the semantic units originally assembled. For example, if the time base for the lowest level *perceptual* feedback loop -- the speech motor control loop which involves processing of the acoustic signal of one's own speech -- is on the order of 150 msec, then there is sufficient time for the computation to be done at the cortical level, since acoustic signals travel from the cochlea to the auditory cortex well within this time period. It is unlikely that any cognitive control could be exerted within this time span, however, for we know that even conditioned reflex paths involving two modalities (e.g. vision and motor control of the hand) in monkeys, require nearly 100 msec of cortical *processing* time in addition to the input and output pathway times (see Evarts [1967]).

This seems to imply that the movements of the speech musculature are largely ballistic -- aimed at target configurations -- rather than controlled -- computed each time by direct sensory feedback (tactile) from the muscles themselves. MacNeilage [1968] arrives at a similar conclusion from a different line of arguments; interestingly enough he shows how such a model does not conflict with models which employ gamma-efferent feedback loops.

We could adduce one more capacity for this hypothecated language control system: a higher level cognitive mechanism for introspecting, or objectively looking at all the rest of the system (where such a mechanism might be located is completely unknown). This introspecting capacity could be responsible for notions about formal rules; it is called into use for very special linguistic tasks such as writing, or when the information contained in an incoming signal is not up to threshold (even for the strategies of perception), etc. Note, for example, the difficulty which aphasics have with the task of making a sentence given a set of cards with the appropriate words printed on them (discussed in more detail below in Chapter IV), even though they are quite capable of using the sentence in ordinary conversation; this could be explained by a breakdown in this higher level mechanism.

Finally, in order to argue such a system is possible, assume that two properties are genetically specified: (1) the general capacity to get a symbol system underway in the nervous system which can re-code neural signals hierarchically (up and down) and store such symbols and (2) the requisite neural architecture for linking up the non-limbic modalities to each other (see Geschwind [1966]). In fact, the pre-programming done by genetic specification seems to be a lot more than this, but it will suffice for the present exposition. We would expect this system



to have as normal output (and to accept as normal input) signals which suggested formal rule organization but in fact never quite conformed to it. We might further speculate that there is a sequential assembler mechanism, which is associated with though probably not the same as the introspecting mechanism above, which puts together the semantic/syntactic/phonological units of language under the direction of a cognitive system. Articulatory commands (i.e. neural commands to the speech musculature) could work on a principle of "tracking" (lexical) units, thus reflecting the intuitive distinction between the phonology and the rest of the grammar. The concept of "tracking" (originally suggested by G. Moore [personal communication] in connection with neural control circuits involving the cerebellum and red nucleus) is a potentially powerful explanatory hypothesis, although in this connection it is difficult to specify it neurologically. The conceptual units specified by the central language system must be transferred to neuromuscular specifications if we are to speak. What is intended by the notion of tracking is that there are two linguistic levels, conceptual and neuromuscular; the neuromuscular commands are computed by following or tracking the conceptual inputs.

On the perceptual side, the counterpart of tracking would be "guessing strategy"; thus the sequential assembler mechanism could be working ahead of the actual input, predicting the next set of speech stimuli in advance of their actual arrival at the auditory cortex. A number of interesting phenomena, such as certain types of 'errors' in speech production, could be discussed by considering the relationship between the sequential assembler routines and the tracking/guessing functions, e.g. spoonerisms. For some further remarks on this general idea and what it could entail for a model of linguistic competence and performance, see Whitaker [1968].

The consequence of taking an alternative view of competence than that just outlined seems to be to put linguistic theory back into the problems discussed in section B above and would therefore be tantamount to rejecting many possibilities for independently testing linguistic hypotheses. There is a systematic ambiguity in the conception of competence and performance outlined above which should be clarified; this is the distinction between *active* and *static* models.<sup>17</sup> Equating competence with the central language system and performance with the four peripheral language modalities does not imply that the former is static and the latter active. The better view is that both the competence and performance systems have active and static aspects, either of which may be modeled. Linguistic theory traditionally models the static aspects of language systems and this is what is intended in this research. In other characterizations of competence and performance, for example Chomsky's, the active-static distinction is often ambiguously presented, too. It could be argued that Chomsky appears to consider competence the static aspect and performance the active aspect of language. Such an interpretation does not affect the criticism of the former view of competence and performance nor does it affect the reasons given above for

offering the view presented here. In fact, there is a three-way distinction involved. There is the actual behavior and organism which models purport to describe and explain, there is an active model of the organism behaving and there is a static model of the organism and its behavior, the last of which is the usual model in linguistics and is abstracted from any real-time parameters. The position taken here is to distinguish static competence and performance models from each other; therefore, no considerations of real-time phenomena are incorporated into the hypotheses.

In conclusion, one might reason as follows about the subject-matter of linguistics: language is a product of man's nervous system -- literally -- and what goes on in the central nervous system underlies the overt expression of language with which we are familiar. In short, language has physical reality in the human brain, and the human brain alone. Our goal is an explanatory account of language, actually, the *structure* of language. It would be ridiculous if our proposed account, our grammar, could not be replicated or employed in any way by the human brain; it would be disappointing if our grammar *could* be utilized by the brain but in fact was not. Finally, it would be rather inconvenient if our model of linguistic structure did actually model neural substrates of language, but only by a very complex (unnecessary) and devious system of mappings.

Accordingly, it is of interest and it may be instructive, to get some idea of what neural mechanisms underlie language. This sort of inquiry is squarely in the domain of applied linguistics, by which is meant the extension and application of linguistic theories into a different realm of data -- the data of language pathology or aphasia. It is patently obvious, however, that aphasic language cannot be properly studied without some knowledge of the neuroanatomic substrates and the neurological bases of language, specifically some idea of the structure and function of the peripheral and central nervous systems of man which are correlated with language behavior.

## Notes to Chapter I

1. "Aphasiology" is a term used more for convenience than scrupulous academic delineation; it is intended to include those scholars from a wide variety of disciplines who are interested in aphasia.
2. Hebb's essay is found as the introduction to the unabridged and corrected [1963] republication of Lashley's 1929 monograph (by Dover).
3. I will not present a history of the writers on aphasia nor a history of linguistics; for a short and interesting treatment of these subjects, see the essay by Otto Marx "The History of the Biological Basis of Language" which is Appendix B in Lenneberg [1967]. Marx's essay concludes with a precis of Wundt's interests in the physiological psychology of language. Another book which considers these subjects is Esper [1968].
4. "Stimulus-response hypothesis" is, of course, a drastic simplification of behaviorism. Since Chomsky [1959] has already analyzed the principle hypotheses of behaviorism in great detail, it would be superfluous to repeat them here.
5. His 1929 monograph was not a refutation of physiologically based experimentation but a careful demonstration of how such experimentation should be done. His continued interest in the field is amply demonstrated by later articles, for example his 1942 article on the "autonomy of the visual cortex".
6. For a detailed discussion of Katz's performance model see Fromkin [1968].
7. Cf. Otto Marx [1967] referred to in note 3 above.
8. The work of Kozhevnikov and Chistovich in Leningrad, Öhman in Stockholm, Tatham and Morton in Colchester, Liberman, MacNeilage, Harris and others in New York City and Ladefoged, Fromkin and Ohala in Los Angeles, is well-known and need not be discussed here. (Cf. bibliography)
9. There are a number of reasons for considering linguistics, excluding certain obvious areas of course, rather than the more familiar discipline speech pathology, one of the neurosciences. The clinical side of speech pathology can be subsumed under medicine; the rest of speech pathology in fact is studying the structure of language and could benefit from linguistic theory as much as the latter will benefit from the broader empirical base. The notions 'normal' and 'pathological' make little sense unless juxtaposed, in which case it is difficult to see them as anything but two sides of the same issue. Since much the same reasoning

would apply to psychology, particularly psycholinguistics, there is little to be gained by an extended discussion of how all these fields should be subsumed under one field. The important point is that all these fields must be sensitive to each other -- theories and facts in any one are surely relevant to the central issue: the structure of language.

10. Behavioral representation is intended to include speech data whether spoken or written, linguistic hypotheses about the data such as rules, and psychological abilities in a general way.
11. Physical representation is intended to include the electrical, chemical and structural aspects of the brain; it would include the acoustics of the speech wave, too, although that is well beyond the scope of the present study.
12. Specifically, the constraints listed by Peters which are now employed in linguistic theory are: ambiguity, grammaticality, paraphrase, surface structure, coreferentiality, and the like. He argued that these represent our 'intuitions' about language and are insufficient in themselves to limit the power of transformational rules.
13. These assumptions are probably correct; although man's central nervous system is an evolutionary development from primate systems in some fashion (exactly how is not known), there is a qualitative distinction in many areas of the brain (see Geschwind in Carterette [1966], pp. 26-34). What seems to be a logical assumption -- that we can learn something about speech perception by studying auditory evoked cortical responses in primates -- may in fact not be. The question of whether anything interesting about speech production can be gained by animal studies is equally in doubt (for a contrary assumption see Lieberman, Klatt and Wilson [1969]). Notice though, that there must be some aspects of the brain, perhaps those that underlie the more specific neurological correlates of human behavior, common to all animals. A recently discovered protein, named S-100 (its function is not yet known), is specific to nervous tissue in animals. In many other respects, too esoteric to discuss here, brain tissue is quite distinct from all other tissues of organisms.
14. But perhaps one may be forgiven for assuming that membrane permeability is in fact related to negation; the problem is we don't know how.
15. To some extent this is being questioned as seen in current work on the nature of presuppositions (e.g. Lakoff [1968]). Chomsky [1968] argues that in large part these are performance matters, but the substance of this controversy is not crucial to the present discussion.
16. This view of the competence-performance was first mentioned by Kelley [1967]. Although he originally intended it ironically, there

seems to be a lot of truth in the observation.

17. There are a great many interesting and speculative questions on this topic which will not be considered in detail here, but in Chapters II, III and IV. For another view of the active-static model, see Morton and Broadbent [1967].

## Chapter II. The Neurology of Language

### A. Introduction

It is clear that brain function has both specific and non-specific neuroanatomical correlations; to some extent the degree of specificity decreases as behavior becomes more complex. Thus, functions such as attention-focusing, sleep/wakefulness controls, elementary visual and auditory perception, and certain limbic-system functions like approach/avoidance responses, all appear to be relatively localized in brain structures. Conversely, memory, thinking and such higher-level functions, do not seem to be properties of very well-defined brain structures in toto, although the best guess is that there are specific and non-specific aspects of these, too. As illustrative of this, see P. G. Nelson's [1967] survey of theories of memory and some views on the specific and non-specific aspects of memory.

There are two simplifications which pervade this research and just because of their pervasiveness it is impossible to note them at every relevant point. Consequently, they are presented now as caveats in the hope that they will be remembered when reading the following pages. The first pertains to the discussion of cerebral localization of the language system. In my attempts to point out the lack of value both empirically and theoretically of the pure holistic approach to brain, it is not always clear that my comments on areas of the brain subserving language are more based on clinical statistics than pure correlative or functional neuroanatomy. The debate over strict localization versus holism has been part of neurology since the mid nineteenth century; until more bridges are built between behavior and the electrical and chemical events in the brain, that debate is very likely to persist. The point of view which is adopted here is that the ultimate goal is an explanatory correlation between brain and behavior; efforts to discredit 'localizationism' are misguided regardless of whether or not a particular fact is unaccounted for. As a step toward that goal the following neurological model of language is proffered; when considering it, one must remember that the brain is not a simple set of parts but an integrated network.

The second simplification pertains to the discussion in Chapters III and IV on the nature of words as represented in the brain. Many early approaches to linguistic theory and certain aphasic symptoms, notably anomia, lead one inevitably to a model that assumes words to be isolated neurological units that can be categorically disrupted and, by extension, that must be the primes in the language system. Common sense and actual

aphasic language behavior as well as current linguistic theory argue against such a simple mechanism. Lesions of the brain do not eradicate, for example, a specific set of words in a patient's language because a word is not simply an isolated memory engram stored in one place in the brain. Words by themselves do not comprise language and it would be surprising indeed to discover that brain damage reduced a patient to a discrete set of words. Language is a system in which words constitute intuitively and theoretically isolatable parts; however, without the system -- the rules which interrelate words -- there would be no language of either a normal or an impaired sort. Similar arguments hold for linguistic notions such as semantic and syntactic features and for that matter, the rules themselves. To think of aphasia as destroying one particular semantic feature throughout the language system is not consistent with the limited knowledge we presently have. However, since it is necessary to discuss the linguistic implications of aphasia in such terms, it may in places appear that such an assumption has been made. Again, I can only provide the warning in advance and trust that the general intentions, data and results of this research will be apparent in spite of the limitations on the manner in which it is expressed.

For many reasons which will be amply demonstrated below, the most reasonable view of the neurology of language -- the neurophysiological and particularly the neuroanatomical substrates of the language system in the brain -- is a combination of specific and generalized structure and function. There appear to be well-defined centers in the cortex (secondary visual and secondary auditory cortices [association cortex] and Broca's and Exner's centers) which mediate the peripheral language systems: the visual and auditory recognition systems and the verbal and tactile production systems. The remaining cortical areas which are associated with language (probably including some sub-cortical inter- and intra- hemispheric association fiber tracts and possibly including some sub-cortical ganglia that are directly connected to the cortex), certain parts of the parietal and temporal lobes, do not seem to be differentiated like the four peripheral control centers; these generalized areas comprise the central language system of which little can be said neurologically beyond an identification of where they are located but much can be said behaviorally, as will be done in Chapter III.

The strict localizationist position, as for example Kleist [1962] and the strict generalizationist or 'whole-brain' position, as for example Lenneberg [1967] to be discussed below, are equally untenable. The strict localizationist position has been discarded in current studies of brain function and to reconsider its shortcomings belongs more in a history of the neurology of language than in a discussion of current knowledge. The whole-brain position -- i.e. that *all* parts of the brain are directly involved, or better, mediate, any behavior of the animal -- has not been completely discarded, at least with respect to language. This will be evident in the discussion of Lenneberg [1967] to follow. Furthermore, with respect to the central language system there are aspects of this position which are more or less correct. The problem

develops only when one assumes that the extremes of this dichotomy -- either localization (specificity) or non-localization (generalization) -- are the only possible theoretical alternatives to take.

Head [1963] is often thought of as a critic of 'localizationism', although in fact he should be considered a critic of unwarranted theorizing about localization of language function. What is overlooked apparently, is that his criticism of anatomical approaches to aphasia was largely based upon the fact that those who proposed diagrams of the language system in the brain generally failed to adequately characterize aspects of language in the first place and hence could not possibly find clinical evidence of a very significant nature (pp. 434-439). In his own words "we must first distinguish categorically the various defects in the use of language and then attempt as far as possible to determine their relation to the locality of the lesion in the left hemisphere." (p. 438) A good deal of progress has been made since 1926 on the problem of characterizing language and language disorders as well as their neurological correlates. Nevertheless, Head's cautious view of the question of localization of function is still valid:

That lesions situated in different localities of one hemisphere can produce specific changes in the power to employ language is one of the most remarkable facts which emerge from the study of aphasia. The material at my disposal is in no way sufficient to determine this relation with precision. Moreover, in all attempts to correlate the site of structural changes with defects of function it must never be forgotten that the severity and acuteness of the lesion exert an overwhelming effect on the manifestations.

But, in spite of these deficiencies, I think we are justified in drawing the following conclusions from the cases cited in this work. The more definitely the injury destroys the lower portion of the pre- and post- central convolutions and the parts which lie beneath them, the more likely are the defects of speech to assume a "verbal" form. A lesion in the neighbourhood of the upper convolutions of the temporal lobe tend to produce "syntactical" disorders. Destruction round about the region of the supra-marginal gyrus causes defects in the use of language which I have called "semantic"; whilst a lesion situated somewhat more posteriorly seems to disturb the power to discover and to understand names or other "nominal" expressions. [p. 441]

In this chapter I will try to show that the distinction between the peripheral language system (the four language modalities) and the central language system (the integrator of the four modalities) reflects those aspects of the neurology of language which can and cannot be localized, respectively. This distinction correlates with the distinction between performance and competence as these terms are defined in this dissertation, thus enabling us to make the larger and more important correlation between neurology and linguistic theory.



B. Critique of E. H. Lenneberg's *Biological Foundations of Language* [1967] (BFL)

The relationship between linguistic theory and the neurology of language has been outlined in Chapter I as well as some justification for the importance of the latter. It is unfortunate indeed, as Lenneberg himself noted in the preface to BFL, that there have been virtually no general and up-to-date treatments of the biology or neurology of language. The significance of BFL in partially filling this gap cannot be undervalued, despite some fairly important shortcomings in the text (to be considered in this chapter). Lenneberg must be credited with outlining the problem, with showing the relevance to linguistic theory and with suggesting a number of important results and hypotheses concerning the neurology of language. My criticisms will not be of the entire book for, as the title implies, Lenneberg comments on the nature of language from an extremely eclectic approach, a good deal of which is not relevant to this research. The parts that are relevant are Chapter 1 "The Conceptual Framework", Chapter 2 "Morphological Correlates", Chapter 3 "Some Physiological Correlates", Chapter 5 "Neurological Aspects of Speech and Language" and Chapter 9 "Toward a Biological Theory of Language Development". In view of the prominence this book has in linguistics, the importance of the subject matter, and the ostensible lack of any published competing views, I feel that an extended discussion of BFL is absolutely necessary before making any new proposals concerning the neurology of language. The clearest method for discussing this book is to first cite a list of the principal hypotheses presented in BFL which will be challenged and then to discuss each in turn in greater detail, presenting concurrently what I feel to be better substantiated hypotheses. At the end of this part of the criticism, the general statements of the theory which are presented in Chapter 9 of BFL will be considered.

A Restatement of the Nine Main Hypotheses about the  
Neurology of Language from *Biological Foundations of Language*  
(Chapters 2, 3 and 5)

1. The brain functions as a single unit such that there are no identifiable neuroanatomical correlates -- either topographical or histological -- of specific behavioral function (including language), except the visual cortex. Specifically, there is no unique histological correlate, i.e. one type of nerve cell or one type of nerve-cell network, which corresponds to the language function; the language 'areas' of the brain have a different cytoarchitecture.

2. Studies of brain lesions with concomitant aphasia support claim #1 because most brain lesions cannot be properly studied or correlated to language behavior for the following reasons:

(a) Cerebral vascular accidents (CVA) usually cause swelling (edema) which might disrupt function over a wide, diffuse area of the brain; furthermore, the infarcted area may receive collateral blood supply. A possible source of collateral blood supply is the posterior communicating artery. In general, the CVA lesion is too diffuse to be of use in localization studies.

(b) Tumors also create pressure over a wide area in the cranial vault, grow relatively slowly and may still permit nerve impulse transmission through them.

(c) Traumatic lesions may affect language function due to secondary disturbances located in other areas than the area of primary destruction, for example bleeding, degeneration, etc.

(d) Diseases such as Alzheimer's Disease are found to create lesions diffusely over the entire cortex thus affecting all aspects of behavior including language; this demonstrates that aphasia can be produced by a general reduction (prematurely) of the number of neurons in the brain.

3. Electro-stimulation of exposed cortex preparatory to surgery (as by Penfield and Roberts) does not indicate any well-defined areas specifically for language.

4. Notwithstanding the above, some lesions can cause more specific language disturbances; for example, lesions in the cerebellum cause "characteristic interference with articulation". Most of such specific lesions seem to be subcortical.

5. Subcortical structures participate in the language system and may have some specificity to language; for example the peri-aqueductal gray matter may be involved in the motor (articulatory) coordination of speech as evidenced by putative lesions in this area.

6. Language is entirely a supra-cellular phenomenon and therefore may not have any direct genetic foundation because genic action is intracellular.

7. Aphasia is interference with language skills because the aphasic is usually not reduced to a state of having no language whatsoever.

8. Aphasia is essentially a disorder of temporal integration.

9. A sequential chain model (associational) will not account for the physiological facts of speech articulation whereas a central plan model with hierarchical dominance will. This is demonstrated by the different lengths, different diameters and consequently different rates of nerve impulse propagation in the several cranial nerves associated with the speech musculature.

Consider first Hypothesis #1, the "whole-brain" or "non-structural"

hypothesis, which is expressed in the following quotes from Chapter 2 of BFL:

In the brain, on the other hand, there are no independent parts of autonomous accessories ...

In view of this we cannot expect to find any kind of new protuberance or morphological innovation which deals exclusively with a particular behavior. Any modification on the brain is a modification on the entire brain. Thus species-specific behavior never has a confined, unique neuroanatomic correlate, but always and necessarily must involve reorganization of processes that affect most of the central nervous system. [p. 53-54]

Yet the search for correlates in specific regions is not as futile as it may seem after these remarks ... The physiological and behavioral significance of the histological differences between areas is still unknown. There are only a few histologically and topographically unique areas to which we can assign corresponding unique motor or sensory roles. For instance, it is universally agreed that the striate area of the occipital pole is the area of primary visual projection. [p. 54-55]

A comparison of Figure 2.20 with Figure 2.22 [a "histological" map with a map of the loci of traumatic lesions which caused aphasia, respectively -- H. A. Whitaker] makes it clear that speech areas are made up of quite different cytoarchitectonic fields. On superficial inspection, Broca's area might have some unique characteristics, but even here a closer examination casts doubt on this. [p. 56]

There is no evidence for an "absolute" language area, but the language function may be localized in statistical terms. Although there is no one area which is necessarily and exclusively involved in language disturbances in all individuals, there are some regions which are never involved in either speech or language. [p. 61]

There is no clear-cut evidence that Broca's area is more specifically related to speech than areas adjacent to it. The language maps established on a statistical basis are not histologically homogenous. There is no cytoarchitectural peculiarity of the cortical areas involved in language. [p. 61]

In general it is not possible to assign any specific neuro-anatomic structure to the capacity for language.

However, this capacity may be due to structural innovations on a molecular level. Language is probably due to the peculiar way in which the various parts of the brain work together or, in other words, to its peculiar function. [p. 72]

From a reading of the text and from considering plates such as the one on page 55 which purports to illustrate histological maps of the lateral aspect of the brains of the Macaca monkey, the chimpanzee and man, it is not altogether clear that Lenneberg is using the terminology of neuroanatomy in the usual fashion. In fact these are rather poor *topological* maps, additionally distorted by being "re-drawn" to the same size. Whether or not they were originally histological maps is not clear from the plates. Topology generally refers to the gross anatomical formations such as the gyri and sulci of the brain. "Maps" which correlate the loci of lesions with behavioral abnormalities are usually topological, such as those of Russell & Espir [1961]. Histology generally refers to the cellular organization of tissue, determined by microscopic techniques. Molecular generally refers to the various chemical constituents of tissue, for example proteins, lipids, amino acids and the like, but can be used to refer to intracellular structures such as the axonal and dendritic processes, the cell nucleus, the glial cells which surround neurons, or various components of the cell such as Nissl bodies, ribosomes, membranes, mitochondria, etc.<sup>3</sup> Morphology generally refers to anything structural and is more or less equivalent to the term neuroanatomical when discussing brain; functions are usually discussed under the heading physiological. As noted before (Chapter I), the term neurological here may refer to either structure or function (where by 'function' is meant electrophysiological or biochemical event, and the like) and is opposed to behavioral (which refers to, e.g. linguistic or psychological hypotheses). Cytoarchitectonic is a specific term referring to the histology of cell networks and thus is subsumed under morphology.

With the terminology in mind it is simple to dispose of an implied confusion in the above quotations from BFL, namely that there is no "specific neuroanatomic structure" or unique cytoarchitectural aspect of language in the human brain. Obviously there is not. What is confusing is the suggestion that there should be or might be. Language behavior, in any terms one wishes to think of it, involves sensory (input--receptive) mechanisms, motor (output--production) mechanisms, and integrative (central--organizational) mechanisms. On these grounds alone there are a priori reasons for expecting to find different histological areas associated with the submechanisms of language. And empirically this expectation is verified. The histology of the parietal, temporal and frontal lobes of the cortex is quite different not to speak of the variations in subcortical tissue structures. As demonstrated by von Bonin [1949] in general, man's cortex has a greater number of axodendritic connections. Even in the grossest anatomical terms, there are noticeable differences between species in most cortical gyri and sulci. For example, the anterior border of Brodmann's area 44 (the operculum or Broca's (motor speech) area) is different in man than in any other primate, even

though in certain respects this cortical zone is found in all primates, i.e., the anatomical homology is that this zone has a distinctive layer of large motor cells called Betz cells -- a histological parallel between man and other primates.

Given the fact that different areas of the brain are histologically quite different although as yet we have an incomplete knowledge of the functional correlates in either neurological or psychological terms, it is nevertheless pure nonsense to deny a correlation as Lenneberg implies, or even to deny that we have some knowledge of a highly informative nature. For example, Evarts [1967] has studied the pyramidal motor system and shown that different neurons in the premotor cortex (an area adjacent to Broca's area and histologically similar) -- neurons which can be distinguished from each other by their structure -- are involved in at least two different motor functions: the maintenance of muscle tonicity and the control or innervation of voluntary muscular contraction. The latter function is the responsibility of the giant Betz cells of this area. The fact that Betz cells are found in cortical areas which are involved in motor control and not in either integration or sensory areas, hardly seems to be a coincidence and furthermore, provides little solace to the whole-brain hypothesis.

Lenneberg is quite correct in noting that localization studies are statistical in nature; as far as I know, virtually everything in the life sciences is statistical in nature. If one argues as he does that the localization or "structure-dependent" hypothesis is invalid because the evidence is statistical, then one must have a very narrow and rather uninteresting view of localization. Localizing the structures which are active in the brain during the use of language is by no means an exercise in fanciful theorizing. Particular, identifiable language deficits are one of the most powerful tools of the clinical neurologist who must know where a lesion is, its extent and its type. It is true that one cannot invariably "place" a lesion in the nervous system with exact coordinates; in the first place, no two brains are absolutely identical anymore than two people are and secondly, most evidence for localization is statistical in nature as just noted. But before one interprets "statistical" to mean "just better than chance" consider this: in a survey of over 800 patients with gunshot wounds, Luria [1962] determined that a "derangement of phonemic hearing" was localized in the superior temporal gyrus [Brodmann areas 52, 41 and 42], posterior region, in 94.7% of those patients injured in this area. This area is the lateral aspect of Heschl's gyrus which receives primary projection fibers from the medial geniculate body of the thalamus as the last level in the auditory system. Not all figures are that tidy, but this example does serve to illustrate the nature of many studies on cerebral localization of language mechanisms.

It is possible to criticize Lenneberg even more specifically. He claims that there is no clear-cut evidence that Broca's area is related to speech any more than other areas, by which one presumes he means the motor or production aspect of speech. This assertion is simply untrue.<sup>4</sup> The 'statistics' as it were are so overwhelming that it is incredible to

believe that such an assertion was knowingly made. As is well known it was this area of the brain, the foot (posterior third) of the second and third frontal convolutions, also called the operculum which Auburtin and Broca first investigated in the 1860's, which later became known as Broca's area and which is generally accepted now as a "motor speech center or analyzer" in the sense that lesions here usually cause apraxia of speech.<sup>5</sup> The evidence is so compelling that it seems pointless to pursue the matter further.

A more subtle argument in BFL is the suggestion that many areas of the cortex subserve the language function in an interdependent manner. The concept which, in my view, most seriously treats this fact is Luria's [1966] notion of the "graded localization of function." There are a number of centers for language in the brain and it is indisputable that in the normal brain they operate in conjunction with each other. It is true that disruption of virtually any of the centers will alter language behavior in many respects beyond that of the specific function of the area in question, as Luria [1966] has so carefully shown. However, it is clear that the disruption and the concomitant behavioral changes are quantitatively different and hence can be measured. In simple terms, disruption of a "hearing" center will have some effects on a "speaking" center<sup>6</sup> but the different effects can be sorted out from each other.

#### 1. Principal Cortical Structures in the Central and Peripheral Language Systems.

In the space of this chapter it would be impossible to synthesize the information of a century's work on the anatomical localization, and functional localization, of language -- only the barest outline will be presented below. The caveat which should be considered is basically that structural data only provides the context in which the major issues -- neurological function and behavioral correlates -- *must* be discussed. This is not to argue that neuroanatomy is "merely descriptive" for, even though the functional aspects are not perfectly known or perfectly matched to anatomical aspects, there is surely a match to be made. To assert that structure and function are independent is to entertain a philosophy of mind vs. body that is not only untenable but is also self-defeating. In fact, some of the functional correlates, in behavioral terms at least, are known; establishing a neurological connection should be considered a goal and not an impasse. The following outline covers the principal *cortical* structures which have been reasonably established as part of the language system in the brain; there are eight of these areas (discussion of the subcortical structures related to them is taken up later) which may be divided according to the model developed in Chapter I into four peripheral and four central language systems. The two peripheral production systems are Broca's Area (verbal or motor speech) and Exner's Center (writing); the two peripheral recognition systems are Heschl's Gyrus (auditory) and the Angular Gyrus (visual language). The four identifiable parts of the central language system are Wernicke's Area, the Naming Center, the Supramarginal Gyrus and the Arcuate Fasciculus (plus other

association fiber tracts of less well-known function). Since a detailed summary of these areas is not easy to locate in the literature, it is presented here.

(i) Broca's Area: This is also called the operculum; it is the foot of the third frontal gyrus ( inferior frontal gyrus) just above the Fissure of Sylvius and anterior to the precentral motor cortex (anterior central gyrus). Lesions here generally cause apraxia of speech, a disorder of the articulatory (motor) organization of language reflected in the patient's impairment in using the speech musculature for speaking. Apraxia of speech can be distinguished from dysarthria; the latter is associated with lesions of the subcortical parts of the motor system of cranial nerves. Dysarthria, unlike apraxia, is marked by weakness or paralysis of various speech muscles regardless of whether the patient is speaking or swallowing. Therefore dysarthric speech entails predictable distortions, usually in the direction of simplification of articulatory gestures, while apraxic speech entails unpredictable substitutions, confusions, etc., of phonemic organization. Apraxics are generally aware of their language deficit. A more detailed discussion is found in Johns [1968]. Foot of the Superior Frontal Gyrus: This is the supplementary motor cortex of Penfield and is also referred to as the supplementary speech area. Its functions are not too well understood; however Penfield and Roberts [1959] claim that electrical stimulation of this area produces "aphasic arrest" similar to stimulation of Broca's area. It may be noted that the cytoarchitecture of this area is quite similar to that of Broca's area.

The above areas are all anterior to the central sulcus (Fissure of Rolando); in general, lesions in these areas tend to affect the production or expression of language significantly more than recognition or reception. As has been indicated, these areas are 'in communication with' posterior cortical areas, most particularly the temporal, parietal and occipital lobes. Thus it is misleading to refer to them as language centers per the strict localizationist dogma; a more accurate notion is to consider them submechanisms of the speech and writing systems, submechanisms which are most involved in the phonological organization of language. As will be discussed below, the more posterior areas associated with language are more involved with syntactic and semantic organization and as would be expected, some of these subserve reading and hearing systems.

(ii) Exner's Center: This is the foot of the 2nd frontal convolution (Nielsen [1946]) and is at the part of the middle frontal gyrus just above Broca's Area. Lesions here may cause agraphia -- an impairment of writing ability. Although this area does not seem to receive long association fibers directly from temporal, parietal or occipital areas, it does receive association fibers from the operculum. It is posited by Nielsen [1946] that the writing system involves the angular gyrus, the operculum and Exner's writing center, since lesions of any of these areas can cause agraphic symptoms. This is discussed in some detail in Nielsen who reports on a patient with a lesion in Exner's writing center; the

patient's spelling parallels remarkably the kinds of errors made by apraxics, indicating some connection between the center and the operculum.

It is interesting that the so-called speech and writing centers -- or, more specifically, the cortical areas involved in the linguistic 'adaptation' of motor systems controlling head, neck and limb musculature -- seem to fall in the regions anterior to the anterior central gyrus (the precentral motor cortex) which is generally accepted as the control point for the pyramidal motor system in both man and primates. There is a possibility, then, that these cortical regions are only so adapted in man; cytoarchitectural uniqueness lends some support to this notion.

(iii) Heschl's Gyrus: The transverse gyri of Heschl are in the posterior portion of the superior temporal gyrus, in the part which forms the floor of the Fissure of Sylvius (fronto-temporal sulcus). This area is the primary auditory projection area; it receives sensory projection fibers from the medial geniculate bodies of the thalamus, forming the last 'level' of the auditory system. Lesions in Heschl's gyrus in the dominant hemisphere interfere with the auditory processing of speech, although the distinction between loss of hearing in general and loss of hearing for words (auditory verbal agnosia) has not been well-defined topologically. Cortical areas adjacent to Heschl's gyrus, e.g. Wernicke's Area, are probably auditory association areas (parallel or analogous to the visual association areas adjacent to the striate cortex) and it would be logical to assume that the processing of words as opposed to a general processing of acoustic signals, occurred in these areas.

(iv) Angular Gyrus: This is Brodmann's area 39 and is more or less the junction of the temporal, parietal and occipital lobes, situated at the extreme posterior portion of the sulcus separating the superior temporal gyrus from the middle temporal gyrus. It is undoubtedly an important language center for it is rich in association fibers connecting it with the temporal and frontal language areas as well as other parts of the parietal lobe and the occipital lobe. Lesions here can cause major impairments in reading ability (alexia) and also major impairments in writing ability (agraphia) the latter perhaps due to the former by virtue of the visual feedback operative in the writing process. This area is also implicated in some of the more diffuse aphasias discussed above under Wernicke's area, the supramarginal gyrus and the naming center. Geschwind, commenting on Roberts [1966], identifies this region as one specific to man and crucial in the establishment of non-limbic modal associations. He notes that the Angular Gyrus myelinates last, has fewer thalamo-cortical connections but more cortico-cortical connections and is located between the somesthetic, auditory, visual and speech areas of the cortex. The assumption that it is part of the peripheral language system may have to be revised when more is learned about it.

(v) Wernicke's Area: This is the central portion of the superior temporal gyrus, perhaps extending back to the supramarginal gyrus.



Lesions in this area vitally involve language function in many respects, although it is reasonable to argue that they predominantly affect the comprehension of spoken language (sensory or receptive aphasia, Wernicke's aphasia, etc., are some of the terms employed). Since it is very likely that the orderly production of speech requires acoustic feedback in order to maintain semantic and syntactic organization as well as phonological organization, lesions in Wernicke's area can often have serious effects on speech production, too. Specific functional claims about this area, as well as the areas to be discussed below, most be considered highly tentative. In the first place, these posterior areas of the cortex appear to be intimately involved with comprehension and integration and association of language; for this reason alone lesions are likely to cause widespread disturbance of function. In the second place, as was noted earlier, suitable linguistic models have not been applied yet to much of the semantic and syntactic aspects of aphasic language; this gap is most noticeable in the aphasias which result from posterior lesions. Thus on both neurological and linguistic grounds, there is much confusion.

(vi) Supramarginal gyrus: This is adjacent to Wernicke's area. Little more than the above remarks about Wernicke's area is known about this area, although some suggestions can be found in Nielsen [1946]. Lesions here obviously involve a major submechanism of language; the plethora of terms describing such syndromes is evidence enough that, from a linguistic point of view, several aspects of the grammar are dependent on this area.

(vii) Naming Center: This is the posterior portion of the inferior temporal gyrus. Nielsen [1946] argues that this is a "language formulation area", the locus of internal language. It has also been suggested that this area is essentially involved in the ability to name objects, etc.; however, again the data is somewhat speculative and probably not worth discussing in greater detail. Lesions here presumably cause amnesic aphasia, impairment in word finding, syntactic errors, and the like. Assuming these judgments to be accurate, it would appear that this area is not subject to a simple definition such as a "naming center". One can agree, however, that it is one of the areas subserving language in the brain and put off a further characterization until more data is produced.

(viii) Arcuate Fasciculus: This is part of the bundle of association fibers (axons) which connect parts of the frontal lobe, including the operculum, with parts of the temporal lobe, including the operculum, with parts of the temporal lobe, including the relevant portions of the superior temporal gyrus. Lesions here affect phonological organization during speech production, but can perhaps be distinguished from apraxia or lesions of the operculum, in that most errors are either clear-cut phonemic substitutions or simplifications of consonant clusters along well-marked parameters. (cf. Blumstein, [1968])

## 2. Cortical Areas and Linguistic Modalities

One can thus argue that posterior lesions affect reception or comprehension of speech and, as already noted, these cortical areas are less well differentiated anatomically and functionally. The major cortical language areas of the brain (see Figure 3) can be related to the four 'linguistic' modalities of language: speaking, hearing, reading and writing, or in neurological terms, the verbal, auditory, visual and tactile systems.<sup>7</sup> Heschl's Gyrus is, anatomically, a primary projection area in that sensory signals from the acoustic nerve reach this cortical area before any other. It is likely that Broca's Area is a primary radiation area anatomically in that motor signals to the speech musculature may go down through the extra-pyramidal tracts rather than through the mediation of the precentral motor cortex (which is known to be a primary radiation area for the pyramidal tract). This is discussed in greater detail below. Other than these two areas, it is likely that all other language centers or analyzers are association cortex; i.e. they receive signals from and send signals to other cortical areas during on-going language use, primarily. This neural exchange or coordination could take place, on anatomical grounds, in one of two ways (or even a combination of both): either through the association fibers which travel from one cortical area to another (both within and across hemispheres), the cortico-cortical association tracts, or through the thalamo-cortical system of projection fibers. In the latter case signals would be mediated by certain subcortical structures such as the thalamus. Penfield and Roberts [1959] and to some extent Lenneberg [1967], subscribe to the latter view which argues that subcortical integration, or use of the "centrencephalic system" (see Penfield and Roberts [1959], pp. 205-216) is more important than the cortico-cortical association tracts for the language system. This view argues that the pulvinar and lateral-posterior nuclei of the thalamus integrate the several language areas discussed above by virtue of the large numbers of fibers which connect them together. This putative subcortical -- cortical language system is outlined in Figure 4 taken from Penfield and Roberts [1959] p. 208. The inferences which can be drawn from the surgical procedures described by Penfield and Roberts do not settle the question of whether the cortico-cortical or the thalamo-cortical version of the language system is correct. Lenneberg's misinterpretation of this section is discussed below under the "subcortical hypothesis". One of the inferences which can be drawn, however, is the unusual and convincing demonstration of the cortical localization of language areas: even though the major part of the surrounding cerebral cortex is removed, as Penfield and Roberts describe, there is no permanent aphasia *if the speech areas are left intact*. Although the anatomy is fairly clear, it is also true that large bundles of association fibers, comprising the external capsule, connect the cortical language areas without passing through the thalamus. The arcuate

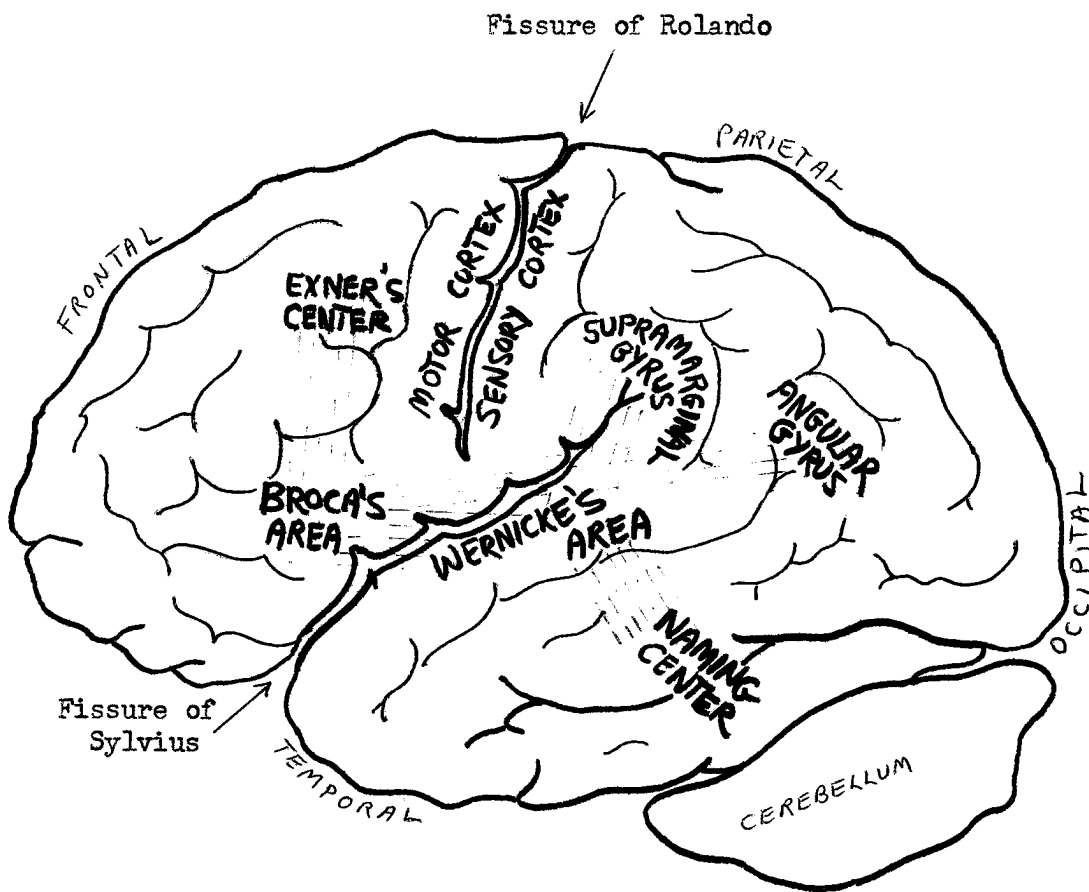


Figure 3. Lateral Surface of the Left Hemisphere

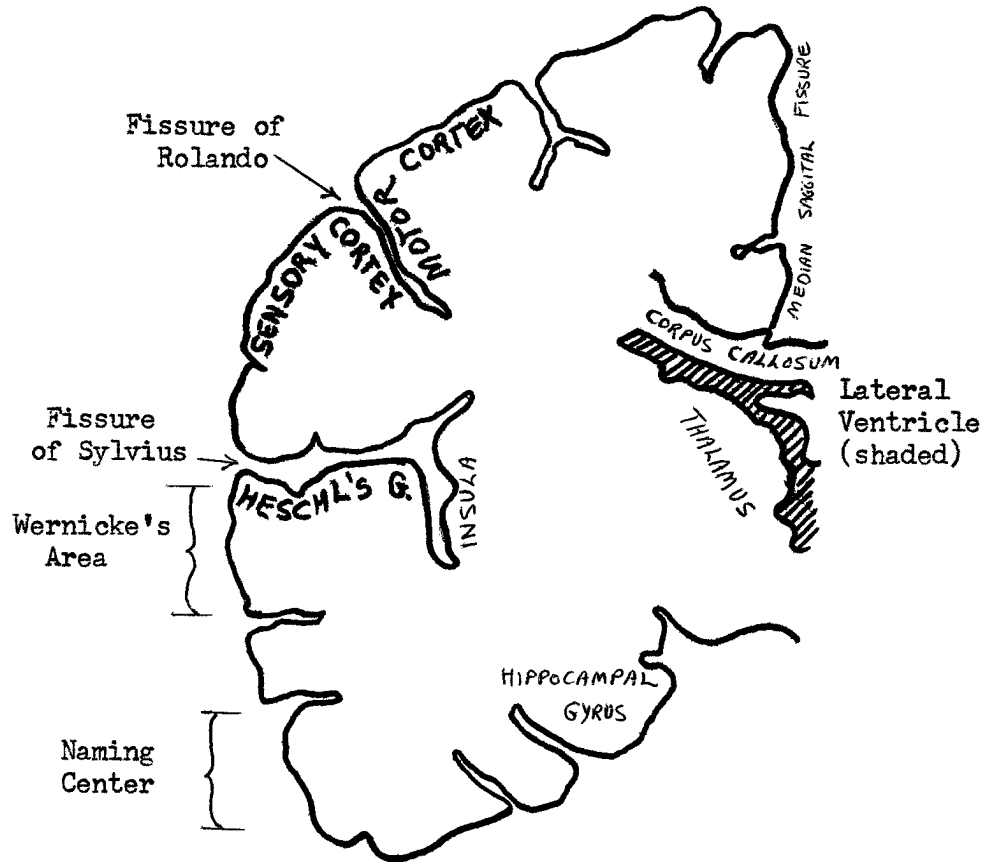


Figure 4. Coronal Section through Frontal Lobe of the Left Hemisphere

Drawing of cross-section of left hemisphere, looking down from the top--connections between cortical areas and thalamic nuclei and some cortico-cortical connections are indicated by straight and dotted lines.

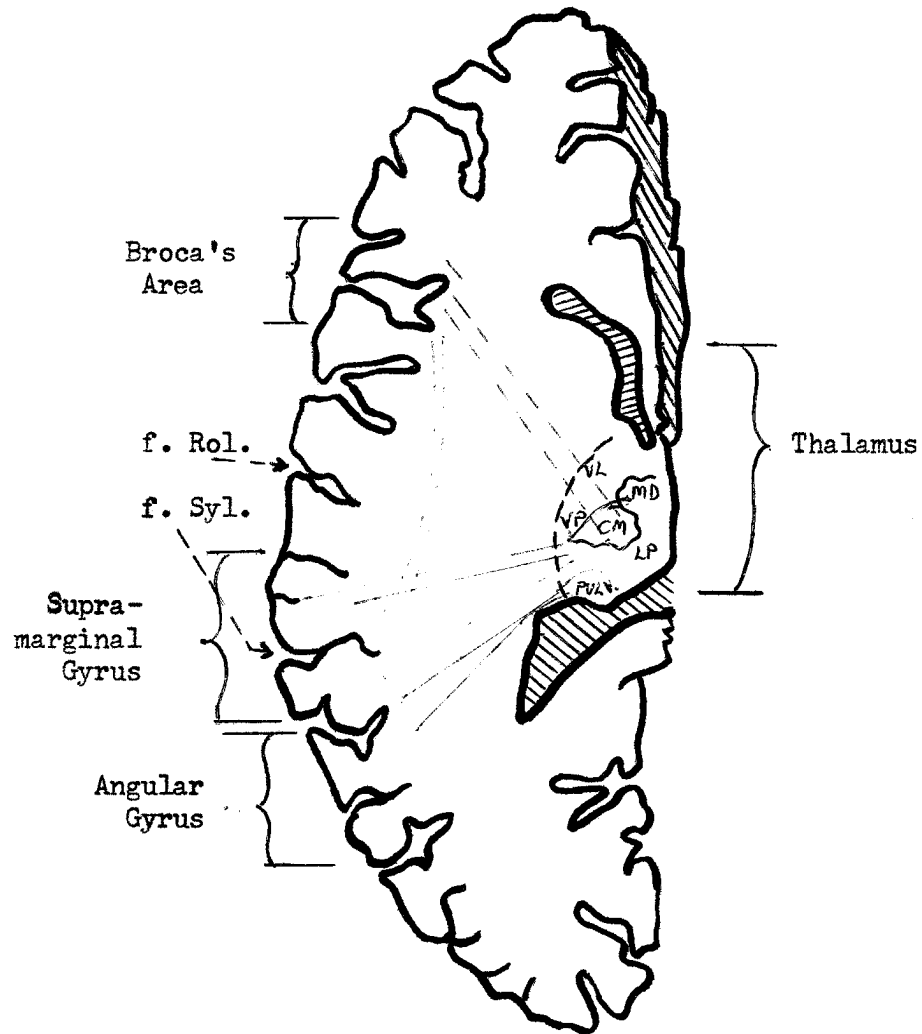


Figure 5. The Cortico-Thalamic Language System  
(after Penfield and Roberts [1959])

fasiculus is one of these fiber tracts. Aside from the studies of lesions in the arcuate fasiculus referred to above, there is little *functional* evidence yet which would indicate which notion of the language system is the correct one. For this reason the problem will not be pursued here.

Notice, however, that for certain aspects of subcortical language mechanisms there is data of both an anatomical and functional nature, namely the speech production and speech recognition subsystems involving the cranial nerves. These systems most certainly involve the thalamus as well as lower structures in the brain stem, etc., and can yield some interesting data on the "performance" system which subserves the central language system. The cortico-cortical vs. thalamo-cortical arguments concern the central language system and either view is open to debate. The lower-level sensory and motor aspects of language use, the peripheral nervous system mechanisms which tie in with the language system in the central nervous system, will be discussed below in the comments and criticism of Lenneberg's Hypothesis #5.

### 3. Brain Lesions and Functional Neurological Correlates.

Lenneberg's hypotheses #2 and #3 concerning the non-utility of studying brain lesions or the electrical stimulation of cortex, with a view toward characterizing language mechanisms in the brain, are quite understandable with reference to his first hypothesis. Since one cannot experimentally work on man's brain, inferences about human brain mechanisms such as those presented above depend almost exclusively on clinical and pathological studies following accidental injury or disease. Since these studies uniformly tend to refute hypothesis #1, one should consider the study of brain lesions most carefully. In fairness to Lenneberg's criticism it must be pointed out that brain lesions of any type are not always exactly circumscribed, are not always easily identified and are not always strictly and absolutely correlated with structural and hence functional language disorders; every neurologist is careful to note this fact. Nevertheless the statistical significance of the correlations is overwhelming and the failure to find a mathematical equation by no means obviates either the methodology or the results.

Lesions have a variety of natural etiologies -- interruption of the blood supply, degeneration, unnatural growth, disease or trauma. Their importance in understanding brain mechanisms is that usually they produce a functional change, loss or impairment, which of course provides direct evidence for the functions of such structures under normal conditions. Electrical stimulation of cortical areas is also a valuable method for it disrupts function, too, although only temporarily. 'Artificial' lesions as the result of surgery are comparable. If one considers the general experimental methodology of physiology, anatomy, biochemistry, etc., it is easy to see why so much emphasis is placed upon careful analysis of brain lesions in man. A large

part of the techniques of these disciplines is based upon methods which destroy structures -- surgical ablation, surgical destruction -- or which selectively blocks functions -- by the introduction of various chemicals into tissue.<sup>8</sup> Contrasting behavior under pathological conditions with normal behavior has led to many of the important discoveries in the life sciences. It is reasonable then to argue that the basic methodology -- studying brain lesions in man in order to study aspects of human behavior -- is the accepted paradigm. Its limitations are obvious; since lesions in man occur naturally, it is often very difficult to find lesions with a narrowly circumscribed area and consequently often more than one brain mechanism is involved. This places a greater burden of proof on the analysis itself, than in analogous animal studies; thus, as would be expected, the clinical procedures must be carefully studied and revised in the light of new theoretical evidence in order to maximally isolate the behavioral subsystem being investigated. With this note of caution in mind, Lenneberg's criticisms of the study of brain lesions for the purpose of discovering the submechanisms of language can be discussed in more detail.

With respect to cerebrovascular accidents (CVA) it should first be noted that the vast and complex arterial and venous systems in the brain are essential to maintain metabolism in brain tissue, not only in the general sense of the blood supply to the tissue but also in a particularly critical time domain. More than a few moments lack of blood to any part of the brain causes irreparable damage to that part. Therefore, the only sense in which Lenneberg's notion of "collateral blood supply" can have any meaning would be if blood flow were not completely occluded to a particular area and consequently the cells in that area did not cease to function. It is not the case, as Lenneberg seems to imply, that there are two vascular sources for each and every part of the brain such that, for example, if the middle cerebral artery were occluded that blood could reach Broca's Area by some other arterial source. The posterior communicating artery can, in a limited and very particular way, be a "collateral blood supply": it connects the basilar arterial system which supplies the posterior portions of the brain<sup>9</sup> to the carotid arterial system which supplies the anterior portions of the brain.<sup>10</sup> If a thrombus occurred in the internal carotid artery it is possible, in fact quite likely, that blood could reach the middle cerebral artery -- the crucial artery for most of the cortical areas subserving language -- through the basilar artery system; a thrombus or embolism in the middle cerebral artery itself, above the branch of the posterior communicating artery and above the branch of the anterior cerebral artery, would cause infarction of major cortical (language) areas which *could not* receive collateral blood supply from any known source.

More to the point however, is the fact that CVA's comprise a very wide range of lesions, both in extent as well as permanence. Those which block major arteries in the brain disrupt so many functions at

once that they are generally not useful for studying particular behavioral mechanisms such as language. There are frequently much less severe CVA's, caused by occlusions in smaller branches of the arterial system, which in fact produce very localized infarction and consequently are most useful in studying aspects of behavioral mechanisms. Segarra and Angelo [1968] argue that "the study of cerebrovascular disease is perhaps the best tool to investigate the correlation between function and structure in the nervous system of man". They note that CVA lesions may be very finely localized and describe in detail a case history of an infarct 2mm thick and 6mm wide at the thalamo-mesencephalic junction (disconnecting the thalamus from much of the sensory fibers ascending from the reticular formation). The syndrome (pure akinetic mutism) and the precise anatomical locus is not germane to this discussion but does illustrate dramatically the viability of the research paradigm which Lenneberg argues against. Segarra and Angelo note that:

The significance of this identity of lesions is fairly obvious: they cannot be the result of a random process but must correspond to an identifiable vascular territory related to a recognizable clinical syndrome. In other words, they constitute a true vascular syndrome in much the same manner as Wallenberg's syndrome, from posterior-inferior cerebellar artery occlusion or sylvian infarcts from middle cerebral. [p. 10]

Lenneberg also and quite properly notes that the secondary effects of many lesions may hinder an accurate structural-functional correlation; of particular concern to him is the swelling associated with edema. While his caution is commendable, he does not temper it with the judgment of good clinical-pathological analysis. Moosy [1968] remarks:

In spite of such obvious limitations as edema in the acute phase, cerebral infarcts and small hemorrhages may be especially appropriate for the study of anatomical-behavioral correlations. Cerebral infarcts lie in anatomically discrete vascular territories about which we have much scientific information and therefore helpful comparisons are possible...

When the edema of the acute phase has subsided, usually in a few weeks, cerebral infarcts may be regarded as static ablative lesions which will produce deficit effects or "negative" signs. [pp. 2-3]

The above illustrates reasonably well, I believe, the difference between an overly cautious pessimistic approach such as Lenneberg's and a more theoretical though still carefully empirical, approach which has led to the data and hypotheses already discussed. With respect to Lenneberg's Hypothesis #4 which suggests that there may be a specific relationship between some focal lesions and behavioral effects on language, he states:



Some diseases which show scattered lesions throughout the cerebrum at the time of death may begin with small destructions of tissue, for example, in the cerebellum or diencephalon where they produce characteristic interference with articulation. [p. 205]

What diseases are being referred to is not clear and the anatomical loci are even more confusing. The diencephalon is the name given to the lower structures in each cerebral hemisphere which surround the third ventricle -- the thalamus, epithalamus, subthalamus and hypothalamus. The cerebellum on the other hand is separated from the cerebral hemispheres by a layer of dura matter; it connects anatomically to the midbrain, below the level of the diencephalon. Lesions in these anatomically distinct areas do not cause similar dysfunctions. There may be characteristic articulatory disorders (taken in a very general sense) resulting from certain kinds of diencephalic lesions but there are none that I know of associated with diseases of the cerebellum. Cerebellar diseases usually affect the temporal or rhythmic organization of voluntary muscular activity in the body and in cases where there is an affect on speech it may result in "explosive" or "slurred" speech (see Chusid and McDonald [1967]) but does not seem to be of an articulatory nature. Brown [1968] indicated that cerebellar disease can interfere with the proper "phrasing" of speech. The diencephalic system, an intimate part of lower motor and sensory language mechanisms, will be discussed below under the Hypothesis #5 concerning subcortical mechanisms.

Hypothesis #5, the "subcortical" hypothesis of Lenneberg, to a large extent reflects a confusion between (a) a hypothesis about the nature of the central language system and (b) *subhemispheric* structures. The latter should be referred to as peripheral systems connected with the visual, auditory, verbal (speech) and tactile (writing) aspects of language use. Lenneberg implies (BFL pp. 62-63) that he accepts Penfield and Robert's [1959] hypothesis about the central language mechanism, an opinion which has been discussed above as the issue between cortico-cortical and thalamo-cortical systems. He goes on to adduce various sorts of evidence for the involvement of "subcortical structures" that play a role in "language and speech" (BFL, p. 64):

...a series of patients with speech and language disorders resulting from surgical diencephalic lesions made in the attempts to cure parkinsonism<sup>13</sup>

...one case of permanent total language arrest has come to the author's attention...

Motor speech disorders may result from either stimulation or lesions of the ventro-lateral nucleus of the thalamus. The most common symptom is either an acceleration or a slowing down of the rate of speech...

There is one other region in the mid-brain that may also be involved in motor coordination of speech. This is the gray matter that either surrounds or is adjacent to the ventral side of the aqueduct...Lesions in this area cause dysarthria in children more easily than in adults. The disturbance is one of articulatory coordination and not paralysis, because many of the patients so afflicted have no trouble chewing and swallowing or moving the articulatory organs, but they cannot control and coordinate the muscles to make them subservient to speech. It is conceivable that a congenital deformity in this region may result in developmental anarthria such as described by Lenneberg (1962).

The neurological terminology for language and speech disorders is remarkably varied and consequently largely unrevealing, as everyone knows. Nonetheless, 'dysarthria' or 'anarthria' has a reasonably consistent meaning, as employed by most neurologists. Darley (in Johns [1968]) states the general view:

The term dysarthria will refer to an impairment of motor control due to faulty innervation of speech musculatures...

the term apraxia will refer to difficulties in the voluntary control in the absence of paresis or incoordination.

Lenneberg is undoubtedly correct in arguing that lesions in parts of the mid-brain and thalamus can cause dysarthria. However, the clinical picture he describes -- being able to chew, swallow and move the speech musculature at will -- is not dysarthria, but apraxia of speech. Referring to Lenneberg's [1962] article clears up the problem. The patient he describes was *mute*, evidently due to a congenital defect; one could describe this as total apraxia of speech because the patient could apparently manipulate the speech muscles reflexively. Since no autopsy was performed, there is little point in using the clinical description given to theorize about where the lesion was located. It is also possible that the periaqueductal gray matter was involved, as Lenneberg suggests, in which case the functional loss may have been a variant of *dysarthria* -- the inability to vocalize -- or one of the syndromes associated with akinetic mutism. Segarra and Angelo [1968] note that their cases of pure akinetic mutism do show involvement of the periaqueductal gray matter, together with other structures at the thalamo-mid-brain junction. It will be noticed that the speech disorders which Lenneberg associates with subcortical lesions are quite limited; he mentions speech arrest, slowing of speech rate (hypokinetic dysarthria) and speeding up of speech rate (hyperkinetic dysarthria). Except for speech arrest about which less is known, these are common components of dysarthria. A more complete picture of dysarthria can be summarized as follows (from Aaronson [1968]):

<u>Disease</u>	<u>Type of Dysarthria</u>
1. Bulbar palsy (lower motor neuron disease)	flaccid
2. Pseudo-bulbar palsy (upper motor neuron disease)	spastic
3. Upper & Lower Motor Neurons	spastic/flaccid
4. Cerebellar disease	ataxic
5. Parkinsonism	Hyperkinetic
6. Dystonia & Chorea	Hypokinetic

Table 1: "Acoustically Distinct Types of Dysarthria"

Explanation: (per Aaronson [1968]) Flaccid dysarthria is marked by breathiness due to paralysis of the vocal cord muscles, hypernasality due to paralysis of the muscles of the soft palate and often a paralysis of the tongue which of course has predictable articulatory consequences. Spastic dysarthria is marked by a strain-strangle harshness to the voice, asynchrony of the muscles of the soft palate with respect to timing its movements with movements of other articulators, a weakness of tongue and lips, a slow rate of articulation, excessively low pitch and little variation in loudness of the voice. Ataxic dysarthria (also known as cerebellar ataxia) is marked by rhythmic disturbances, particularly irregular phrasing. Hyperkinetic dysarthria is marked by mono pitch, mono (and reduced) loudness and an accelerated rate of speaking, giving the impression of blurring or slurring. Hypokinetic dysarthria is marked by slow, distorted muscle gestures, long delays before a gesture can be initiated and noticeable deviations from the desired (target) gesture.

The importance in recapitulating Aaronson's review of dysarthria is to underscore its non-linguistic feature; the dysarthric symptoms may be present without any corresponding apraxia of speech or aphasia, they affect the speech musculature consistently whether one is speaking or not and they indicate a pathological involvement of the subcortical mechanisms associated with the motor control of these muscles. Apraxia of speech, on the other hand, which has been succinctly outlined by Darley [1968], Johns [1967] et alia (also called, as noted before, Broca's aphasia, motor aphasia, etc.) involves the cortical motor speech center -- the foot of the second frontal convolution. It is characterized by phonemic errors -- deletions and substitutions -- such as this example from Darley [1968]:

(1) /lEbərLEvEr/ for /rEvEr/ ("Revere")

An analysis of the phonetic aspects of dysarthria can be found in Lehiste [1965]. With characteristic thoroughness and attention to detail, she describes in both articulatory and acoustic terms the speech of ten dysarthric subjects and concludes with a chapter devoted to the major phonetic attributes of dysarthria, broken down into three categories: deviations due to insufficient control of the vocal folds, deviations due to lack of control of the velum and deviations of a general articulatory (neuromuscular) nature. Lehiste's study is of particular interest in terms of the indirect evidence it provides for the distinction between dysarthria and apraxia, both in terms of the etiology and locus of the brain damage as well as the phonetic and phonological characteristics. Keeping in mind the major attributes of dysarthria as outlined by Aaronson, one can make a careful study of Lehiste's descriptions of each subject's speech and demonstrate not only the differences between dysarthria and apraxia but actually make a reasonable guess as to the clinical diagnosis! Consider the description of the speech of her patient S2 which is marked by strong nasalization, aggravated by laryngealization, potential weakness of the lip musculature (shown by errors in voiceless bilabial and labiodental plosives and fricatives) and a tendency to systematically eliminate final consonants. One would predict that S2 had a bulbar palsy, which in fact is the case. Next consider patient S3. Lehiste notes that he had a tendency to insert retroflex segments where none were expected and made relatively frequent substitutions in the manner of articulation of consonants such as a fricative for a plosive; less frequently the point of articulation was changed from what one expected. Other aspects of S3's speech leads one to presume that the dysarthria was largely characteristic of a spastic paralysis, with an important qualification: many of the deviations (the retroflex segments, for example) are more typical of apraxia of speech than dysarthria. If the spastic paralysis is indicative of a pseudo-bulbar palsy, i.e. upper motor neuron lesions, then it is reasonable to infer that the apraxic characteristics indicate cortical involvement. Interestingly enough, S3's clinical diagnosis indicates a CVA, caused by embolism; it is quite likely that some cortical area is implicated in this lesion. Some of the theoretical implications of the data about subcortical language mechanisms will be considered in section C below.

#### 4. The General Neurological Model in BFL

The remaining four hypotheses in BFL can be commented upon very briefly. Hypothesis #6, the "supra-cellular" and "non-genetic" hypothesis, is stated as follows by Lenneberg:

...how could something like the capacity for language have a genetic foundation? The phenomenon is, after all, entirely supracellular or even more general, namely an interrelation of activities of complex assemblies of cells. [p. 239]

This is an extremely puzzling notion for several reasons. First, it could easily be interpreted as an argument against the "Innateness-hypothesis of language", which Lenneberg is at great pains to defend elsewhere in BFL. Second, it could be interpreted as disclaiming a genetic foundation for a property of the organism, in this case language; taken literally, that is simply an inconceivable assertion. This claim could also be interpreted as an attempt to deny any structural foundations of language in the brain; the preceding discussion has, I believe, refuted that claim. There is also much evidence, from pathological cases, for *cellular* aspects of language, e.g. diseases of the nervous system which attack the myelin processes of cells, the effects of certain drugs on the synaptic junctions and glial cells, and the like. Elsewhere in BFL Lenneberg mentions some of these himself. Since the structure of the brain is genetically specified, behavior which has clear structural correlates must have a genetic foundation; this of course is not to deny the relevance of environmental factors -- both must play a role in the development, as well as the use, of language.

Hypothesis #7, the "interference vs. loss in aphasia" hypothesis (cf. pp. 206-208 in BFL for an elaboration) is somewhat reasonable as it has been interpolated here, although the conclusions which Lenneberg draws from it are quite untenable; this will be discussed in detail in Chapter III, section A(1).

Hypothesis #8, the "time-base" hypothesis, is stated in the following:

Aphasic symptoms give no evidence of a fragmentation of behavior, that is, of dissolution of associatively linked "simpler percepts". Most of the symptomatology may be seen as disorder of temporal integration, of "lack of availability at the right time."

If this statement is only meant to impugn 'associationist theories' in psychology (cf. hypothesis #9 below) then there is nothing to the suggestion of a "time-base" for language beyond a discussion of those theories. However, it seems that Lenneberg wishes to subsume all aphasic language under the rubric 'linguistic performance' and to claim that the common feature of all such disorders is a disruption of the timing of events in the central nervous system. Some interesting hypotheses could be developed from this idea; e.g. that excitatory and inhibitory circuits for language were detached in varying degrees, affecting control such that elements of the language system either randomly or spontaneously were activated. Lenneberg does not speculate, however, and consequently the whole idea is vitiated, becoming little more than a circular argument on which nothing of substance depends.

Hypothesis #9, the "anti-associationism" hypothesis, is found in

Chapter 3 of BFL, "Some Physiological Correlates." Lenneberg refers to Lashley's 1951 paper on serial order and attempts to prove that there is a neurophysiological refutation of simple association or chain models of speech production. Specifically, the cranial nerves which innervate the speech musculature are of different lengths; therefore one would suspect that the neuronal firing order in the mid-brain (where the motor nuclei of the cranial nerves are located) is different from the order of muscular events in speech production. There seems to be a fallacy in this argument. The cranial nerve nuclei in the mid-brain area may or may not have a firing order different from the order of muscular events in speech, due either to the length of the nerves, inertia of the muscles, loci of the nuclei themselves, or all sorts of anticipatory or ex post facto contextual effects in phonological organization. The problem is that we already know that the axonal connections from the motor speech cortex down to the cranial nerve nuclei vary fantastically in size -- one to twenty microns -- which of course means that the speed at which spike trains are propagated varies fantastically, too -- from six to one hundred and twenty meters per second (data is from Evarts [1967]). It is possible that a 'slower' bottom end matches up with a 'faster' top end such that the sequence of neuronal events in the cortex is actually a straightforward articulatory phonetic representation. I don't know this to be true or false but the possibility is there and consequently Lenneberg's elaborate theorizing in this matter is quite premature.

The foregoing discussion covers most of the hypotheses in BFL relating to the neurological bases of language which seem to be either wrong or misleading. What I consider to be a more accurate viewpoint has been developed in each argument. In section C of this chapter a resume of the hypotheses proposed here will be given.

Lenneberg's final chapter attempts to propose a general theory of language. He begins with five premises (BFL, pp. 371 ff.):

- (1) Cognitive function is species-specific.
- (2) Specific properties of cognitive function are replicated in every member of the species.
- (3) Cognitive processes and capacities are differentiated spontaneously with maturation.
- (4) At birth, man is relatively immature; certain aspects of his behavior and cognitive function emerge only during infancy.
- (5) Certain social phenomena among animals come about by spontaneous adaptation of the behavior of the growing individual to the behavior of other individuals around him.

The first of these premises is meaningless insofar as it claims that different animal species are different, and trivially true insofar as it claims that the major substrates of behavior are species-specific. The second is trivially true: dogs are more like dogs than elephants. The third is either a redundant version of the second ("The frog's egg will develop into a frog and the minnow's egg into a minnow" BFL, p. 373) or a rather obscure way of noting the fact that genes do in fact specify genetically determined form and function. Premise number four is true -- certain aspects of behavior in man and in other animals have developmental characteristics or show maturation. Premise number five is undoubtedly true, also -- the environment does have some effect on behavioral development, although, like gene specificity, all the causal links are not yet known. Note however that the example which Lenneberg uses (p. 374) -- that the songs of certain bird species are environmentally determined -- is not a very accurate representation of the facts. There are 'social calls' and 'mating calls'. With many bird species the latter are genetically determined; birds raised in isolation from other members of the same species will develop the same mating calls (see Thorpe [1967]).

The theory itself in part seems to be a mere restatement of some of the premises, which might subject it to some formal criticism. His theoretical statements are (pp. 374 ff):

- (1) Language is the manifestation of species-specific cognitive propensities.
- (2) The cognitive function underlying language is an adaptation of the capacity to categorize and extract similarities.
- (3) Certain peripheral anatomical and physiological specialization account for some language universals, however cerebral function is now the determining factor for language.
- (4) The biological properties of human cognition limit the range of possibilities for variations in languages; however, within the limits set there are infinitely many variations possible.
- (9) The language potential and the latent structure are replicated in every healthy human being, hence universal grammar is a unique type common to all men.
- (10) Every child may learn any language with equal ease.
- (12) In a given state of maturation, exposure to adult language excites the actualization process in the child just like a certain frequency causes a resonator to vibrate; however, the energy required is supplied by the individual himself. Using the trigger analogy, the child unwinds himself.

(13) Even though biological constitution of the individual is essentially a replication of progenitors, there are individual variations. There may be variation on the level of latent structure, due to variations in the operation of cognitive processes or variations in the maturational course; there may also be variation on the level of realized structure, due to variations in peripheral function and structures. Variations on these two levels explain the main facts about language constancies, language change and language universals.

Statement #1 I agree with, and in the trivial sense of the above premise #1, it is true; explaining why and how it is true and understanding the mechanisms of it are, of course, the crucial issues not answered in BFL. Statement #2 refers to the psychological discussion in Chapter 8 of BFL; what neurological substrates are involved is not suggested and in fact are not known. If statement #3 means that linguistic ability is determined by cerebral functions (notwithstanding Lenneberg's arguments about subcortical structures in the earlier part of BFL), I agree with the hypothesis and have shown additional evidence for this view; if statement #3 means that because we use our speech musculature for speaking our speaking sounds like it was produced by our speech musculature, I do not understand the point even though I am forced to concur with the fact, that is, the phonetic linguistic universals in question are clearly restricted by the sounds human beings can and cannot make, which in turn is determined by anatomical specification. What linguistic universals are accounted for by physiological specialization are not suggested by Lenneberg and yet they are surely more important. Man is capable of producing many more sounds than he actually uses systematically in language; an explanation for this would be very interesting.

Statement #4 is an interesting claim; as far as I can understand this, Lenneberg is repeating in different words the accepted theoretical position of contemporary linguistics. Within linguistic theory, #4 is an empirical claim for which there is much evidence. However, one of the points of studying the neurology of language and aphasia is to corroborate this point, as well as others in linguistics. As Lenneberg states #4 it is not a biological hypothesis at all and hence cannot be proven or rejected in this form on any neurological grounds.

Statements #5 through #8 and #11 are not summarized above since they are a recapitulation of his remarks on language acquisition about which I have no comments. Statement #9 does not seem to differ from statements #1 and #4 and thus needs no further comment. Statement #10 appears to be true if one considers the average length of time children of all cultures take to acquire their native language; other than that parameter, nothing else is really known about the so-called 'ease of acquisition' hypothesis. Statement #12 seems to be the same as premise #5, only the behavior in question here is language; it is very difficult to make comments on the resonance-trigger analogy. In the discussion in BFL on growth and maturation of the brain he does not mention the analogy, making it difficult to interpret it beyond the simplistic notion that French children learn to speak French. Statement #13 is trivially true as a factual description



but not particularly interesting; I fail to see it as an explanation of either language change or language universals, as Lenneberg claims. Putting aside the inaccuracies as well as the tautologies and obvious truths, it is nevertheless difficult to interpret these statements as a theory of the biology or neurology of language. They do not lead in any obvious way to the construction of a model which might have empirical consequences. In spite of this, Lenneberg concludes Biological Foundations of Language<sup>18</sup> with some remarks intended to show the explanatory power of his theory. These are chiefly the claim that he has demonstrated language to be a species-specific behavior and therefore largely due to the genetic specification of innate human mechanisms (which is, incidentally, quite the opposite claim from hypothesis #6, p. 239, discussed above). It happens that I agree with this claim, although it is questionable indeed that Lenneberg's book is proof of it in view of the fact that he takes great pains both to demonstrate the lack of neuroanatomical and neurophysiological correlates of language and to demonstrate the profusion of neural homologies between man and other animals (excepting the peripheral anatomy). These last two points argue against his stated theoretical position, which seems to be a needless confusion since, as I have shown, there are specific neurological correlates in the central nervous system and the homologies are not strict.

### C. Summary and Proposals on the Neurology of Language

The importance of the preceding section is not so much as a resume of what is known about the neurology of language but as a synthesis of that knowledge for the purpose of establishing a neurological framework for the structure of language. The peripheral language modalities and the central language system were differentiated, not according to the familiar anatomical distinction between the central and peripheral nervous systems, but according to their functional roles in language behavior. This enables us to correlate the structural neurological model with the competence/performance distinction in linguistic theory.

It cannot reasonably be claimed that neurological correlates of language have precedence over linguistic hypotheses; however, it can and must be claimed that the two disciplines are intimately related to each other. Valid hypotheses from either perspective must fit those from the other, since the overall theory of language cannot tolerate conflicting evidence from any perspective if it is to be a satisfactory account. Unless such an approach is taken, it is difficult to see how linguistic theory can account for the deviancies found in aphasic speech, how it can make use of such deviancies as evidence about linguistic hypotheses and how certain analytical devices of linguistic theory may be adequately constrained. The grammar must be capable of predicting normal language behavior, as is generally agreed; it follows that it must be capable of predicting pathological language behavior, too, for the latter is no

more than a subset of the former. Since it turns out that aphasic deficits affect both competence and performance (as will be shown in Chapters III and IV), the neurological model must be able to explain that distinction as well as the linguistic model.

In the neurological model, the functional (from which the structural follows) distinction between competence (central) and performance (peripheral) is made on the following basis: a sub-system is central if it relates to the language system irrespective of modality and is peripheral if it relates only to the deployment of language in a specific modality. This distinction has very important concomitants in the analysis of aphasic language as will be discussed in following chapters. A performance factor can only be manifested in one modality; although a competence factor may be manifested accidentally in one modality, it is capable of being manifested in all four peripheral systems. With respect to the peripheral sub-systems it must be remembered that the verbal, tactile, visual and auditory systems do more than handle input-output language signals. As noted above, the language system appears to be superimposed upon other systems in man and nowhere is this more apparent than in the peripheral language sub-systems. This seems to imply an equivalence between the tactile-verbal-graphic-acoustic signal systems which is not quite correct. Language acquisition clearly proceeds from, e.g., acoustic signals to the graphic and therefore in this sense the latter signal systems are derivative. That the systems which are 'derivative' from the viewpoint of language acquisition are not derivative in the same sense for an adult speaker, is obvious from aphasics who can write but not speak.

Therefore, it should be quite obvious that performance data -- i.e. actual spoken or written language -- must be used as evidence for hypotheses with some care; the properties of the peripheral systems themselves must be separated from the use of these systems in a speech mode, and both of these must be separated from the functions of the central language system. These three distinctions can be made clear with the following hypothetical example. In order to write one's name, all three systems -- the central and peripheral language systems and the tactile system proper -- must be operative. If one cannot write his name, the cause could be in the motor innervation of the hand musculature (a non-language, tactile system deficit), in the motor innervation of the writing sub-system (a performance deficit), or in the central language system (a competence deficit). In the first case, we would expect the subject to be able to write his name with the other hand, or by holding a pencil in his teeth, etc. In the second case, we would expect the person to know his name if it was spoken out loud and to be able to speak it correctly himself (and perhaps be able to read it), but be unable to write it by any means or even to arrange alphabet blocks to spell it. In the last case, we would expect the person to be unable to use his name in any way correctly.

With respect to the cerebral localization of components in the central language system, much evidence was adduced which need not be repeated here. It can be theoretically summarized by the following five-step argument:

- (1) The separate parts, sub-mechanisms or sub-systems of language in the human brain
- (2) are connected with each other structurally and functionally (i.e. anatomically and physiologically)
- (3) but can be differentially disrupted such that
- (4) pathological language behavior can be used as evidence for (sometimes the only evidence) the locus of lesion and
- (5) as evidence for the functions of the particular sub-mechanism.

The above is an inductive argument; steps (4) and (5) are the stock-in-trade of the clinical neurologist. Step (2) is well-demonstrated anatomically and to a lesser degree functionally. Step (3) is well-documented by aphasiologists even though the terminology is not well established. Therefore, step (1) seems to be the most reasonable initial premise to the argument; it does not seem reasonable to adduce a "non-localization" or "undifferentiated brain" hypothesis in place of step (1).

Whether the language system is a special evolutionary development or merely a quantitative addition to homologous brain structures found in all animals, cannot be settled conclusively or convincingly as yet. However, all the evidence points to the former. The following remarks by Magoun (in Millikan and Darley [1967]) summarize the viewpoint accepted here and, I believe, that of most neurologists:

...man's communication by symbols, both vocal and written, appears to represent an entirely novel functional increment related to the acquisition of associational cortex in front of the face and hand parts of the motor area in the case of speaking and writing, and around the cortical sensory areas for audition and vision in the case of recognition of spoken and written language. Man's capacities for communicating by symbolic language are unique also in depending upon neural mechanisms which develop only in the dominant one of the two cerebral hemispheres, rather than bilaterally. One can conclude that there are two unrelated central neural mechanisms for vocal expression in vertebrates: one for nonverbal affective communication, widely present in the animal brain stem, and a second for verbal communication, present only in the lateral neocortex of the brain of man.  
[p. 18]

There are two aspects of man's behavior related to language which have not been discussed -- memory and intelligence. The neurological evidence pertaining to memory indicates that the hippocampal gyri are necessary structures for the use of long-term memory: bilateral lesions in these cortical structures generally impairs memory severely. So far the evidence indicates that *this* long-term memory (generally, memory for events and the

like) can be totally obliterated without any serious affect on language. Neurological work on short-term memory is virtually non-existent as far as I know, although there is an extensive psychological literature on the subject. Whatever "memory" is involved in language use evidently is different from the memory system which uses the hippocampal gyri and is apparently intimately a part of the sub-mechanisms of the language system which have already been discussed; in fact, not much can be gained by using the notion "memory" when referring to linguistic hypotheses. The usual sense of memory, long-term storage of information about previous perceptual events, etc., can be distinguished then from the central language system and need not concern us further. That aspect of short-term memory discussed in the psychological literature, i.e. being able to process 6 or 7 chunks of information at one time (cf. Miller [1956]) can be relegated to performance. In fact this is done in most of the linguistic literature.

Intelligence presents a more difficult problem. Again, except for the psychological literature, very little work has been done on the brain mechanisms underlying intelligence (or general cognitive ability, etc.). For theoretical reasons alone, one would want to separate the language system from the systems of intelligence (and memory) simply in order to circumscribe empirical investigation to a manageable limit. If, as has been suggested (cf. Luria [1962]) certain aspects of intelligence are mediated by frontal lobe structures, there may even be empirical grounds or justification for considering it separately from the language system.

One of the interesting problems in the neurology of language, briefly mentioned above in section C, pertains to the lateralization of higher brain functions including language and the role of subcortical structures in the language system. It is well-known that the pyramidal system controls both the cranial and spinal motor nerves which in turn control skeletal musculature (including the speech musculature). The cortico-spinal pyramidal motor tracts are generally depicted with a homunculus overlaid on the precentral gyrus (the motor cortex) to indicate where various regions of the body are controlled from, as in Figure 6.

The cortico-spinal tracts originate in both hemispheres, descend to the level of the medulla just above the spinal cord, where approximately 80% of them decussate (cross to the contra-lateral side) before proceeding down the spinal cord. The second motor system is the cortico-bulbar tract which innervates the cranial nerves. Except for the muscles in the pulmonary region, the speech musculature is controlled by the cranial nerves. The cortico-bulbar tract must decussate like the cortico-spinal tract, although this fact is not well-defined anatomically; the decussation of the cortico-spinal tract is easily located in the medullary region but this is removed from the loci of the cranial nerve motor nuclei and generally well below them. We know that a decussation occurs though, on physiological grounds. The affect of decussation is that the right side of the motor cortex controls the left side of the body and vice versa. The same bilateral control is noted for the speech musculature *qua muscles*, for the clinical picture of dysarthria is similarly bilateral; e.g. a paralysis may

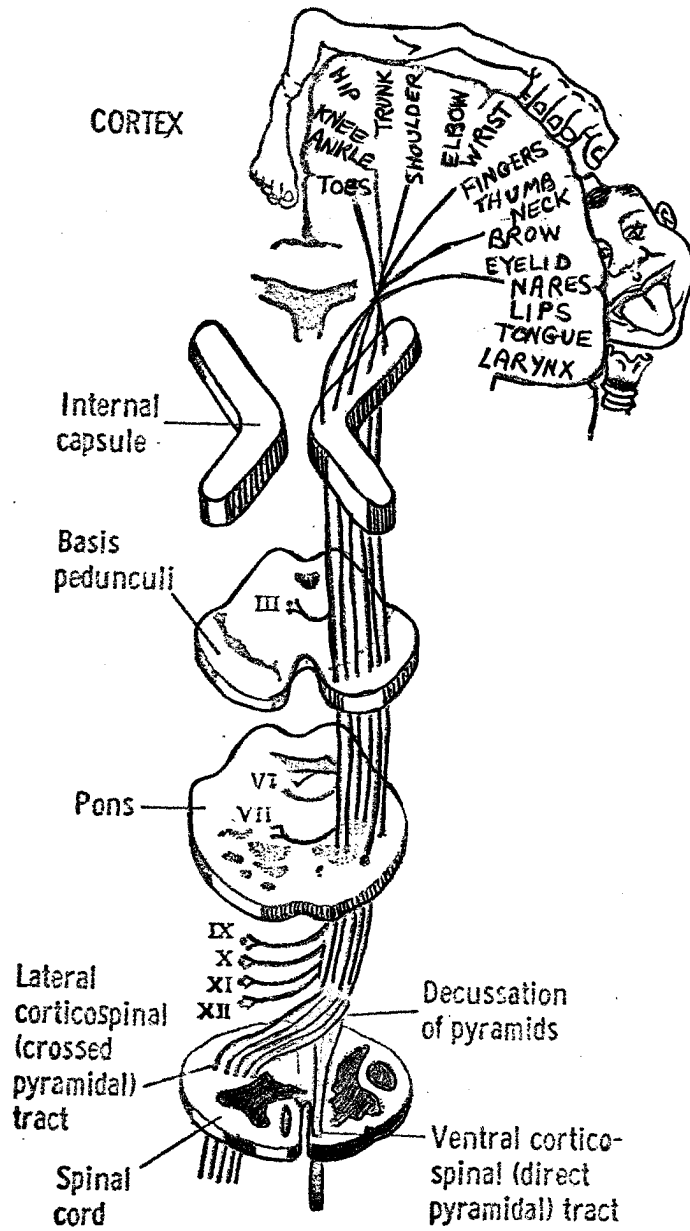


Figure 6. The Pyramidal Motor Tract

affect only one side of the face, tongue, etc.

It was believed that the motor speech cortex (Broca's Area) which is adjacent to the precentral gyrus (motor cortex) uses cortico-cortical association fibers connected to the latter and thereby 'modulates' the cortico-bulbar pyramidal tract when we are speaking. This of course would explain the fact that we have no little difficulty in speaking and eating at the same time. However, there are reasons for rejecting this assumption. If the production of speech were modulated through this system (the normal cortico-bulbar tract), one would expect to find lateralized articulatory deficits in patients with apraxia of speech or surgical division of the corpus callosum, which *is* the case for hand movements.<sup>12</sup> But in fact we do not. The language centers discussed above are active in only one hemisphere at a time -- i.e. language is lateralized, not bilateral, and usually lateralized in the left hemisphere. The most dramatic evidence for the cerebral dominance of language function is the fact that in the adult the non-dominant hemisphere may be surgically removed with virtually no permanent aphasic defects; this is also the case for division of the corpus callosum. Penfield and Roberts [1959] have shown that there are distinct and separate projection fiber systems from the precentral motor cortex (the pyramidal system) and from Broca's Area (an extra-pyramidal system) to the thalamus. Evidently the language system OUTPUTS directly to the thalamus without going through the cortical parts of the pyramidal system.<sup>13</sup> Since at the lower levels, from the mid-brain on down, there is patently only one set of motor nerves -- i.e. only the hypoglossal nerve innervates the tongue -- then it must be the case that there are two cortical centers for parts of the body's motor system. It is possible that the switching box or junction of these two control centers is in the diencephalon, perhaps specifically in the thalamus. In any event, the two controls -- voluntary motor activity for chewing, swallowing, etc., and voluntary speech activity -- are essentially different in that the motor activity is bilateral (right hemisphere-left side of body) and speech activity is unilateral (located in one hemisphere and not showing any bilateralization in either the speech musculature or in hand musculature when used for writing, etc.).

An interesting corollary to the lateralization of the language production system is the possible lateralization of the language recognition system as well, at least with respect to hearing. Some interesting experiments of Kimura [1961a, 1961b] and also Liberman et al. [1966], show that the right ear preferentially analyzes speech signals and the left ear, music, etc. The acoustic nerve decussates, too (and like the pyramidal tract, there are a few ipsilateral connections) such that a majority of the signals originating in the right cochlear nucleus will project, ultimately, on the left transverse gyrus of Heschl, i.e. in the language-dominant hemisphere. The optic nerve decussates in a similar manner but rather than the whole right eye relating to the left occipital lobe, there is a right and left visual field in each eye which respectively project upon the left and right occipital lobes. Each field is a half-circle

(vertically split). The lateralization effects on the reading aspect of the visual system is strikingly demonstrated in patients with surgical division of the corpus callosum.<sup>17</sup> Gazzaniga [1967] has found that generally such patients can read in a normal fashion only within the right visual field, indicating once again the language-dominance of the left hemisphere. Interestingly enough however, careful examination by Gazzaniga indicated that the non-language dominant hemisphere had some visual language ability; these patients could identify with their *left* hand concrete objects when the written name of the object was flashed only to the left visual field (thus, only the right hemisphere could have "seen" and "interpreted" the written word and it is this hemisphere which solely controls the left hand when the corpus callosum is cut). Attempts to identify anything other than concrete nouns (and, incidentally, they were common household objects) in the non-language dominant hemisphere have failed to date. There is already much evidence that the non-language dominant hemisphere is primarily used for spatial discrimination tasks and visual and tactile object identification tasks (see DeRenzi, Faglioni, and Scotti [1968] for case history studies) which may explain why the names of certain objects can be perceived by this hemisphere. Further research needs to be done before one might speculate on the linguistic implications of this function of the non-dominant hemisphere.

The evidence so far, and it is by no means complete, seems to indicate that the language function is neurologically quite distinct from any other brain function of man, in addition to being qualitatively distinct from animal communication systems (for some recent work on the nature of animal communication systems which supports this view, see Smith [1969]). Generally speaking, we may think of man's brain as a functional composite of two halves which under normal conditions equally participate in behavior; under certain pathological conditions in which one of the halves is abnormal, the bilateral nature of brain function is quite apparent. The language system seems to be unilateral however, and yet obviously uses the bilateral anatomical systems from the subcortical structures on down to the peripheral speech musculature. The traditional view that language is superimposed on structures which were originally employed for other functions seems to be valid then, at least with respect to subcortical structures. To conclude, one would guess that the evolutionary specialization which made language possible in man is to be found in the cerebral cortex.

The above is a description of the neurology of language as presently understood and with a minimum of speculation -- I alone am responsible for the synthesis which gives an impression of a unified descriptive theory and I alone am responsible for the 'weight' given to different facts and hypotheses used. What is missing in order to develop an explanatory theory is the (among others) relationships between electro-physiological events and chemo-physiological events which correlate with the above. There are three somewhat weak reasons for not considering these in any detail: (1) this is basically a linguistic approach, (2) my knowledge and (3) what seems to be known in the respective fields is very incomplete;

consequently I see no reason in simply taking up space with unfounded theorizing. When more is known about protein synthesis, membranes, electrical events associated with sensory and motor correlates of *higher* behavior, and the like, then one can speculate more.

However, there is data -- of a behavioral nature -- to complement the above discussion of the neurological bases of language; this is the language behavior of aphasics. In the literature many studies have correlated aphasic language behavior with the neurological hypotheses just proposed. This will not be done here or in the next chapter because my clinical experience did not include adequate medical histories to any extent. The data *can* be tied in with linguistic theories, though, which is at least one important aspect of the major problem -- the representation of language in the human brain.



## Notes to Chapter II

1. It should be pointed out that there have been a great number of articles discussing specific aspects of language in terms of biology, neurology, anatomy, physiology, psychology, etc. Those which bear directly upon the topics explored in this dissertation will be mentioned in the relevant sections and as they apply to the refutation of Lenneberg's arguments. Lenneberg's statement refers to book-length studies which attempt to synthesize the many facts and hypotheses presented over the last century.
2. In particular, the developmental studies in which the maturation of the brain is related to language acquisition -- BFL Chapters 4 and 7 -- and the remarks on genetics and evolution -- BFL Chapter 6 -- are beyond the scope of this dissertation. Chapter 8 "Language and cognition", at first glance would appear to be concerned with some of the problems to be developed in this chapter (II); in this section Lenneberg considers aspects of semantics such as reference, naming and categorization. However, the discussion is purely psychological theorizing and the only data presented is that of some experiments on color perception; therefore, this chapter will not be considered either.
3. Since the functions of many of the intracellular structures are not very well understood, they are identified generally on morphological criteria.
4. He may be equivocating on the notion "adjacent to" in which case one can only regret the careless style.
5. For an excellent historical review of the investigations of Broca's Area, see D. F. Johns [1968] "A systematic study of phonemic variability in apraxia of speech". As Johns notes, apraxia of speech has been variously called aphemias, motor aphasia, Broca's aphasia, loss of speech, anarthria, verbal aphasia, expressive aphasia, phonetic disintegration of speech, cortical dysarthria, articulatory dyspraxia, etc. The point that is consistently made by aphasiologists is that this syndrome affects the motor articulatory organization -- from mild disruptions up to total loss of speech -- without affecting comprehension or the basic substrates of language structure to any appreciable degree. There is good evidence that lesions in the arcuate fasciculus, a band of association fibers which lead from the posterior temporal lobe (Wernicke's area) to Broca's Area also affect the phonological organization of language (cf. the paper by Blumstein [1968] in this regard).
6. This is a well-known fact repeatedly mentioned in the literature; Luria [1966] has a good analysis of it, but virtually every writer on aphasia acknowledges it.

7. Since the goal is to relate linguistic to neurological hypotheses, there seems to be little point in keeping any terminological distinctions of this sort, particularly when no confusion can result by using the terms interchangeably.
8. One should not minimize the importance of other techniques such as monitoring electrical activity of cells and cell-networks (EEG), quantitative studies of the biochemical composition of structures, etc. The point here is that ablation studies on animals, for example, parallel studies of brain lesions in man. The difference, obviously, is that the latter have a measure of uncontrollability not found in the former, which makes them more difficult to interpret.
9. The basilar artery and its branches provide blood to the cerebellum, pons, acoustic nerve, medial and inferior portions of the temporal lobe and the occipital lobe. These structures are not directly associated with the language systems.
10. The carotid artery has two branches: the anterior cerebral artery which supplies the frontal lobes and the middle cerebral artery which supplies most parts of the brain associated with the language mechanisms.
11. Although not a terribly critical point, it should be mentioned that surgery for Parkinson's disease is not intended as a cure as Lenneberg states (the causes are not known for this disease); surgery is only to relieve certain symptoms and usually involves destruction of the globus pallidus or the ventrolateral nucleus of the thalamus.
12. Note that "hand movements" is not to be equated with writing gestures. In agraphia the patient cannot write with either hand although if the 'pyramid' hypothesis were true, one would expect the loss of writing ability in only one hand. Gazzaniga notes that callosal-sectioned patients can only write with *one* hand; since the only motor nerves which go to the hand go through the spinal part of the pyramidal system, which does decussate, this is to be expected.
13. Penfield and Roberts [1959] find that surgical excision of most of the precentral motor cortex only causes a mild postoperative aphasia which soon disappears.
14. The corpus callosum is a large mass of interhemispheric associational fibers which enable the right and left hemispheres to communicate with each other. The occipital lobes in each hemisphere employ the most posterior portion of the corpus callosum, which is referred to as the splenium.
15. In the final pages of BFL, Lenneberg discusses the distinction between competence and performance. Since this is a central issue in the criticisms of Weigl and Bierwisch in the next chapter, and since it

does not affect the current discussion, the few comments on Lenneberg's remarks will be deferred to Chapter III.

### Chapter III. The Linguistic Analysis of Aphasia

#### A. Literature Survey

Studying aphasic language for the express purpose of evaluating linguistic theory has been done only in the work of Weigl and Bierwisch [1968], to my knowledge. There has of course been a number of descriptive studies of grammatical characteristics of aphasia, some of which have interesting and relevant data and conclusions: Goodglass [1962], Goodglass [1968], Goodglass, Klein, Carey and Jones [1966], Jones and Wepman [1967], Wepman, Jones, Bock and Van Pelt [1960] and Osgood and Miron [1964]; these will be briefly discussed below. For an excellent review of other work on the grammatical aspects of aphasia, see Goodglass [1968].

Osgood and Miron [1964] present a resume of a conference held in Boston in 1958; it is chiefly of interest in that linguists and aphasiologists, for apparently the first time, sat down together to discuss how their respective fields and interests were interrelated. Chapter 4, "Linguistics and Aphasia", maps out some of the ways in which linguistic theory can be applied to the study of aphasia, with particular discussion centering on Jakobson's [1956] proposals that aphasia could be characterized as either a similarity disorder (paradigmatic) or contiguity disorder (syntagmatic). It was noted that this theoretical dichotomy is not sufficient for categorizing the complexity of aphasic disturbances. In fact, as Chomsky pointed out at this conference, neither does it explain the complexity of natural language. In a rough way however, it seems to correspond to a general dichotomy of aphasic types -- the agrammatic and non-agrammatic, or in other terms, the classic distinction between motor and sensory aphasia.

Wepman, Jones, Bock and Van Pelt's [1960] short paper is of interest here in that they propose a model for language in the central nervous system which distinguishes perception-transmission disorders, the agnosias, from production-transmission disorders, the apraxias, and from integrative disorders which are the true aphasias. *Formally*, their model parallels the peripheral and central language systems delineated in this research. It should be noted though that their model is based on aphasia syndromes and as such only places these in a very general theoretical framework; they do not attempt to isolate either the functional or the anatomical aspects of their model.

Jones and Wepman [1967], acknowledging the relevance of current linguistic theory to aphasia research, approach it from the statistical analysis of certain "grammatical indicants of speaking style." They propose 23 indicants, or variables, ranging from common and uncommon nouns, verbs and adjectives (in the frequency-of-use sense), to the frequency of use of various pronouns and other so-called function words. Their data contrasts scores of aphasic and normal speakers in free speech elicited as a response to the cards of the Thematic Apperception Test. They found two major aphasic groups: one which had difficulty using substantive words which they label as "semantic aphasia" and another which could use the substantive words easily but was "unable to embed these words in a grammatical structure (176)," which they label "syntactic aphasia". Jones and Wepman suggest that there may be a third aphasic category determinable by these measures, "pragmatic aphasia", which is marked by a patient's inability to "associate incoming signals with appropriate concepts and his speech conveys little meaning to a listener (177)." One patient studied showed a shift in usage over a period of 18 months from the "pragmatic" stage to the "semantic" stage; of some interest is the apparent fact that the use of adverbs and auxiliaries changed like the verbs and pronouns, while prepositions changed like nouns. This paper is a good example of the statistical approach to aphasia which assumes the universality of certain linguistic constructs and bases the analysis on frequency of occurrence of those constructs. Since the present research is more concerned with the neurological reality of the underlying constructs themselves, no other statistical analyses will be reviewed.

Goodglass' earlier [1962] paper is an interesting discussion of the agrammatism typical of Broca's aphasics (motor or non-fluent aphasics) which produces a "telegraphic" style of speaking. Goodglass challenges the usual view of telegraphic style which suggests that the patient, having difficulty speaking, uses all his energy just to produce the important or information-bearing words of the intended utterance and thereby omits the grammatical or function words.

He proposes instead:

the distinctive feature of the agrammatic speech defect is an abnormally high threshold for initiating speech sequences -- either after a silence or as a continuation of sequences already in progress; that in order to produce any speech, the patient with this disorder must find the *salient point* in his intended utterance -- ordinarily the significant noun or verb. As a result, his speech issues in short bursts, each centering about a salient verbal element, with rarely more than one unstressed morpheme before or after it. The normal melodic intonation and rhythm are thus destroyed...The prominence (salience) may be based on stress, on phonological distinctiveness, on affective value, or on informational significance. The small relational words of grammar, which usually lack salience of any kind, are particularly vulnerable... [p. 110]

His study contrasts agrammatic aphasics with non-agrammatic aphasics (the latter are also referred to as fluent aphasics, (see Howes and Geschwind [1964]) in terms of substitution and omission errors on the initial stressed word of a sentence, on the number of words in a typical phonological phrase, and on substitution and omission errors of the grammatical formatives /-S/ (the plural noun marker, the possessive marker and the 3rd person singular verb marker) and /-D/ (the simple past tense verb marker). Goodglass's conclusions show that:

omissions increase with severity of aphasia only in the case of the short-phrase (agrammatic) group while substitutions increase with severity only for the long-phrase group. Comparisons of the two groups with respect to errors on opening stressed words shows no significant difference. [p. 113]

Errors made by ... non-agrammatic aphasics are overwhelmingly more frequent with the extra-syllable form of the final /-S/ and final /-D/ ... The ratio for the agrammatic subjects is markedly different, with omissions of the simpler but less salient form of the inflection (i.e. the non-syllabic form of these bound morphemes--H.W.) occurring just as often as errors with the complex form.

Goodglass [1968] suggests seven general grammatical losses in aphasia, which can be paraphrased as follows:

1. Omission of articles, prepositions, personal pronouns
2. Interchangeability of articles, prepositions, personal pronouns
3. Substitution of verb stem of infinitive for inflected verb forms
4. Loss of coordinating and subordinating syntactic constructions
5. Loss of intonation as an indicator of grammatical units (phrases)
6. Loss of comprehension of the meaning of grammatical formatives
7. Use of incomplete sentences and the mixing of grammatically incompatible sequences. [p. 179-180]

These losses are not all readily amenable to interpretation in linguistic theory, although much of the data and conclusions in the main part of his study are; they are instructive, however, in that Goodglass raises the question of their relationship to linguistic competence and performance [p. 184]. He makes the point that competence can be considered modality-free, or conversely that there could be an expressive competence and a receptive competence if it could be shown that with respect to particular grammatical losses there was no interdependence between the production and recognition of language. Linguistic theory, of course, opts for the modality-free conception of competence; this is the position taken in this research also. Goodglass' results, based upon testing of both the recognition and production of grammatical characteristics such as passive/active, verb tenses, singular/plural, etc., in populations of motor and sensory aphasics with brain-injured non-aphasic controls, are as follows:

all our groups demonstrated essentially the same sequence of difficulty with the ten grammatical forms presented, this sequence was the same for expressive and receptive modalities of discrimination ... With minor exceptions ... Broca's aphasics and fluent aphasics followed the same pattern ... Considering the question of capacity for understanding grammatical discriminatins, we found that whether we split our aphasics along lines of impairment in expression or impairment in comprehension or into diagnostic subgroups, no significant differences in adequacy were produced. Although we had restricted the severity range of our aphasics, the lack of significant differences is still a remarkable result. This finding further indicates that there is a fairly stable hierarchy of difficulty of grammatical tasks that is unrelated to the so-called grammatical difficulties of aphasics. [p. 193]

...there is no evidence that any particular grammatical rules are harder for agrammatic aphasics than for fluent aphasics. A large number of grammatical tasks arrange themselves in a hierarchical order of difficulty that is essentially standard for all aphasics. [p. 205]

These results are in complete accord with the theoretical position of the present research as well as with the experimental evidence adduced below. They are, however, at variance with the theoretical position of Weigl and Bierwisch [1968] who claim, with Lenneberg [1967] and Tikofsky [1968], that aphasia only affects linguistic performance. This issue is important not only in its own right but also for the evaluation of aphasic language data. If aphasia affects only performance, then aphasic speech is of little value as evidence for or against linguistic hypotheses about a particular grammar, although it would, of course, be of interest in discussing performance models; therefore, a more extended discussion of the paper by Weigl and Bierwisch [1968] is necessary since they have the only detailed arguments supporting this position.

#### 1. Critique of Weigl and Bierwisch's "Neuropsychology and Linguistics: Topics of Common Research"

Weigl and Bierwisch begin with the assumption (pp. 3-5) that speech performance is a system of components through which language is used; some of these are: lip-reading, auditory perception, spontaneous speech, repetition speech, reading and the like. Each component has a set of steps for analysis or actualization. For the auditory perception component, for example, there is a "preliminary auditory analysis, identification of the underlying phonemic pattern, actualization of the connected meaning and organization according to the syntactic rules (3)".

Each component which can be disturbed in a specific way such that other components are not disturbed, is considered an autonomous functional unit. Linguistic competence is assumed to underlie these components of performance; all the performance components are connected to competence in specific (although unnamed) ways. Competence is said to be one of several factors which "govern" the "system of performance strategies (5)". The data which they obtained, cited below following this discussion of their theoretical framework, is clearly performance data (as is some of the data obtained in this research as noted in Chapter IV). The fallacy in Weigl and Bierwisch's position is first to assume that all aphasic speech data is performance data (i.e. that all aphasics are alike) and second to assume that performance data is evidence for the notion that competence cannot be disrupted. As evidence about linguistic performance, specifically about the nature of the peripheral language modalities, Weigl and Bierwisch's paper in no way conflicts with the theoretical position taken in this dissertation and in fact supports that position. What they fail to observe, it seems to me, is that aphasia can affect competence. Part of the reason for this failure is to be found in the theoretical assumptions which they make about linguistic competence.

In a special section "Competence, performance and aphasia" (pp. 5-8), Weigl and Bierwisch make the following assumption:

Implicit in our research program is the hypothesis that aphasic syndromes in general are to be understood as disturbances of complexes of components or subcomponents of the system of performance, while the underlying competence remains intact. [p. 5]

It is of interest to note what is in part their definition of the neurological correlate of competence:

Some of the earlier kinds of linguistic treatment of aphasic phenomena might give the impression that aphasia consists in a partial loss of linguistic competence, i.e. in a reduction of the long term storage of language. Instead of this we would like to consider aphasia as a disturbance of the access to the knowledge of language still preserved. [p. 5]

Weigl and Bierwisch present three arguments in support of the claim that aphasia affects only performance, paraphrased here:

(1) In aphasia usually one or several components of speech performance are disturbed, while others are not. If aphasia was a loss of competence, then each performance component that might be impaired would have to be connected to its own competence, forcing one to postulate a separate competence for each language modality which is an absurd conclusion.



(2) There is a day-to-day variation in the performance of an aphasic in the specific component affected most; e.g. a patient may name an object today that he could not name yesterday. If competence were lost in aphasia such a fluctuation would not occur. Any occurrence of a performance skill, no matter how random, proves the underlying competence is still intact.

(3) De-blocking, which is the transference of a performance skill from a still-intact component to a disturbed one, would be unexplainable if the underlying competence were lost.

This set of arguments concludes with the following caveat:

This claim does not mean, however, that aphasic disturbance of performance cannot be bound to particular components or aspects of competence. Rather we will illustrate below very strong connections between aphasic phenomena and quite special aspects of components of competence. It is for this reason that certain aphasic phenomena can be classified in terms of the structure of competence, i.e. in terms of grammatical and lexical structure. This then seems to be the rationale behind such treatments which deal with aphasia in terms of competence. [p. 7]

Weigl and Bierwisch also recognize two types of aphasia which *can* be interpreted as loss or partial loss of linguistic competence: global aphasia (in which all language functions are disturbed) and expressive/receptive agrammatism (in which the production and recognition of speech are reduced to single words). Their arguments against interpreting these syndromes as loss of competence are quite weak. With respect to global aphasia they simply state that when all language functions are disturbed it is impossible to decide whether competence is lost or not. With respect to agrammatism they suggest that the disorder could be interpreted as a deficit in the strategy for serial ordering, which they relate to Jakobson's [1966] notion of "contiguity disorders". This obviously is related to Lenneberg's [1967] notion of the disorder of temporal integration, briefly discussed as his hypothesis #8 in Chapter II. It should be noted first that this is a misrepresentation of the usual clinical picture of agrammatism. The agrammatic does speak and comprehend in single words, but these usually are nouns, verbs, adjectives and adverbs which are *ordered* serially as in normal speech. What are missing (and what are not perceived) are the so-called 'function' words: articles, auxiliaries, prepositions and the like. The agrammatic will communicate with one or two words, although often faultily; to explain this away with a notion of temporal ordering strategies surely misses the linguistic relevance of such a disorder. In summary, the position taken by Weigl and Bierwisch is that if an aphasic has any recognizable speech behavior retained, his competence is presumed intact, and if he has no recognizable speech behavior, one cannot decide whether competence

is intact or not. Stated this way, it is apparent that competence in their view is not a property of the human brain; even in cases of nearly total language loss they suggest that competence is still, somehow, intact. It is difficult to know what is meant by "intact" in this context, other than the tautology that brain lesions putatively cannot disrupt competence. There is no question but that one can maintain this theoretical dualism -- competence thus is a linguistic concept and performance is a neurological concept -- but the consequences are unacceptable. In such a view one must reject the relationship between linguistic and neurological models of language and one must reject the possibility of investigating competence by analysis of aphasia. Curiously, Weigl and Bierwisch suggest that one *can* investigate competence by analysis of aphasia, because as quoted above, performance components are "bound to" specific aspects of competence. This turns out to be a very puzzling suggestion. If an aphasic deficit can be bound to a specific aspect of competence, it is impossible to distinguish between a *block* of that aspect of competence (what Weigl and Bierwisch call a performance deficit) and a *loss* of that aspect of competence (which would presumably be a competence deficit), since all the behavioral effects would be identical! This seems to be a purely terminological point which is no argument against the main theoretical issue at stake: does aphasia affect competence or not?

If we consider in detail the three arguments which presumably establish the fact that aphasia affects only performance, the fallacies of such a model become more apparent. Argument #3 quite obviously applies only to a particular class of aphasics, namely those who have performance deficits. It cannot be disputed that if a deficit manifests itself in only one modality and not in others, the competence underlying the function in question is intact. It is just for this reason that the bi-modality technique was employed in the research presented in the next chapter. This technique is the analog of de-blocking. Tests are given to aphasics in both recognition modalities and the responses are solicited in both production modalities. When a deficit appears in all four, i.e. when it is impossible to de-block the deficit, there is no other conclusion but that competence is disrupted. Unless, of course, one does not consider competence to be a property of the brain, a view which has no interest whatsoever to me. As will be shown in the next chapter, and has already been observed by Goodglass as noted in the preceding section, an aphasic deficit can and often does cut across modalities. The patients studied by Weigl and Bierwisch apparently did not have deficits of this sort, but that is hardly reason to conclude that all aphasia is the same.

Argument #2 is correct, but notice that what is at issue is a performance strategy -- object-naming ability. If a patient still has some ability to name objects, it is possible that he may name 'ashtray' today and not be able to do so tomorrow. If a patient has lost the ability to name objects however, such a fluctuation does not occur *unless and until* a partial recovery is achieved, which may be a matter of years. There are other problems with argument #2. Considering performance variations, one could be led to the fallacious assumption that there are

no permanent aphasic deficits, which is patently not true. The matter of fluctuation *per se*, does raise a theoretical problem: namely, does a deficit have to be absolute in order to count as evidence for functional loss? I would suggest that it does not, but the data discussed in Chapter IV provides a better means for answering this question than any speculations here. In the first place, linguistic performance always fluctuates even in normals. One of the main reasons for proposing a model of competence is to be able to make linguistic generalizations which do not have to account for fluctuations. Therefore, as a warning, argument #2 is applicable not only to the study of aphasia but to the study of language in general. The linguistic implications of any language behavior must take fluctuation into account; it is slightly more problematic with aphasics, particularly when a function is only partially disturbed, but it is still the same problem.

A closer examination of argument #1 reveals some interesting but apparently unfounded assumptions. Competence is considered by Weigl and Bierwisch to be a unitary, all-or-none phenomenon which cannot be affected by brain damage. This view forces one into claiming that competence has no neurological correlates or substrates, a view that is reminiscent of Lenneberg's [1967] (p. 239) argument that language is supra-cellular and therefore has no genetic basis. Note that the position maintaining that competence has no neurological basis is quite different from claiming that competence is or is not localized in one specific part of the brain. If one accepts the position that competence has no neurological basis, it seems inescapable that competence cannot be investigated empirically and that assertions about psychological reality become totally vacuous. Linguistic theoreticians do not maintain such a crude kind of dualism and it is difficult to understand why anyone in psychology or neurology would desire to remove their theoretical constructs from the ordinary venues of science. Weigl and Bierwisch's assumptions require that aphasia disrupt *either* competence *or* performance; the alternative which is proposed in this research, that brain lesions can affect both competence and performance (or either one), is not considered. The view that brain damage can affect both competence and performance (discussed above in Chapter II) which seems to be the only plausible alternative, can be briefly recapitulated using a different example. Consider the case of a brain lesion which affects the visual system. If the patient has a right or left hemianopia, it may be the case that he does not *see* half of a written word or sentence, given any point of visual focus. This of course is not a linguistic deficit but a pure visual deficit -- partial blindness. If the patient has alexia, a deficit in reading ability not vision (although a pure visual deficit may be present at the same time) there may be either performance deficits or both competence and performance deficits. For example, Marshall [1968] reported on a patient who could not read prepositions but did use them correctly when speaking and appeared to be able to comprehend them normally. One of the patients I have worked with, K. T., is virtually unable to read personal pronouns and furthermore he cannot write them

correctly and has great difficulty using them properly when speaking. He does not seem to be able to understand them properly when spoken to him, either. In this case it is necessary to claim that K.T.'s deficit has affected his central language system or linguistic competence (the details are presented in Chapter IV). The distinction between a pure modality (e.g. visual agnosia), a pure performance feature (e.g. alexia) and a competence feature is, I believe, quite clear.

Good reasons for rejecting the theoretical position of Weigl and Bierwisch have been given. Apparently they were more interested in speech production and perception processes than in direct consideration of the central language system, which might account for the unclear theorizing just discussed. They do, however, present four conclusions from their research which are valuable contributions to our understanding of the representation of language in the brain. The theoretical position they take prevents one from relating these conclusions clearly to competence or performance. In some cases, e.g. with respect to conclusion (3) below on the semantic fields, it would be interesting to learn whether the deficit was manifested in all modalities or not; without this information not much more than a summary of their results can be given. These are (my paraphrase):

(1) Investigation of the correspondences between the auditory phonemic perception and the perception of graphemic structure and labio-lexic structure (lipreading), indicates that there is only one abstract underlying lexical representation of words in the brain and that these various subsystems are interrelated to each other at a 'lower' level. Of major interest here is their conclusion that the phonological component is neurologically distinct from both the semantic and syntactic components of the language system:

A correct phonemic or graphemic identification of given signals does not necessarily presuppose a syntactic and semantic analysis... the actualization of the external articulation patterns also can proceed without any participation of semantic and syntactic processes... [p. 9]

Weigl and Bierwisch further provide evidence for the view that both visual and auditory speech perception *need not* depend upon the lower-level articulatory patterns, somewhat contrary to the motor theory of speech perception (cf. Liberman et al [1963]), which of course does not rule out this model as a supporting component in normal language behavior; it does rule it out as a necessary component, however. In sum Weigl and Bierwisch argue that the articulatory patterns and the abstract lexical representation are neurological *levels* or components distinct (but interdependent, of course) from the semantic and syntactic components, a view which I strongly agree with and which will be examined in greater detail in Chapter IV.

(2) Certain cases of alexia which at first seemed to demonstrate the reality of the [abstract/concrete] feature distinction actually seem to be correlated with the difference between lexical and grammatical formatives in Weigl and Bierwisch's analysis. This is shown by the different deficits among the German patients investigated for nouns that have prefixes and those which do not, the former being derived from verbs in German. It is argued that alexia in these patients affects abstract, prefixed nouns (in terms of the ease of comprehension) significantly more than concrete, non-prefixed nouns. Related to this it was observed that a patient could grasp the syntactic information without getting either the semantic or phonological information correct; in the case of pronouns, the patient could comprehend that it was a pronoun but did not know which one. This is an important hypothesis which incidentally is supported by my own research; some data obtained from K.T. parallels Weigl and Bierwisch's results exactly and will be discussed in Chapter IV. Weigl and Bierwisch also noted that for some patients errors in reading pronouns consisted of reading another but incorrect pronoun rather than some random word, a finding which my data also confirms.

(3) Related to the suggestion in (2) is the postulate that lexical structure in the brain is organized in terms of semantic fields. Weigl and Bierwisch have data showing comprehension errors which are substitutions of nouns within the same semantic field. I.e. a patient will read *trousers* instead of *blouse*, *tie* instead of *cuff*, *bodice* instead of *cardigan*, *sandals* instead of *socks*, *peaches* instead of *oranges*, *bananas* instead of *figs*, *potatoes* instead of *vegetables*, etc.

(4) Weigl and Bierwisch claim to have evidence for the psychological reality of transformational rules. This was shown by a patient's ability to take dictation of a set of syntactically related sentences (i.e. differences were between the kernel sentence, the WH-question form, the yes-no question form, and the like) which the patient formerly could not repeat, either verbally or in writing; the patient was first "taught" (by the de-blocking technique) to transcribe the kernel sentence, and it was found that she could then transcribe all the transforms without further training. The data which Weigl and Bierwisch present in this section does not pertain to questions of whether specific rules are part of the central language system or whether sets of rules for neurological units, or some other possibility. Their data does suggest that transformational rules in some fashion are separable from both semantic and phonological aspects of the language system. As stated by Weigl and Bierwisch, "transformationally controlled operations constitute a separate subcomponent of performance that may or may not be retained if connected subcomponents are disturbed." With due allowance for the different interpretation that I put on this data -- I consider it a matter of competence and not performance -- I would agree with the conclusions drawn. As well be shown in Chapter IV, I think that evidence of a much more specific kind can be gleaned from aphasic language behavior, however,

## B. Aims of the Study

It should be reiterated that I am interested in establishing independent empirical justification for linguistic hypotheses; in fact, of course, my research touches on only a few aspects of the grammar. The pertinent question in any aspect of applied linguistics is: what constructs in theoretical linguistics are meaningful or of significance (substantive) to the empirical domain being investigated? But applying linguistics to the domain of neurology has, I think, a special significance in a number of important ways. A linguistic hypothesis that is descriptively adequate for some neurological phenomenon in a meaningful way can be said to represent part of the language system in the human brain. Depending upon the nature of the neurological phenomenon and the particular linguistic construct in question, this description may bear upon either linguistic performance or linguistic competence [= peripheral or central language systems], and further may bear upon language universals or facets of a particular language. What is of concern in this area of applied linguistics then is an explication of how well linguistic theory is actually a model of neurological reality at some level of abstraction.

Some of the general aspects of the grammar are evident from the consideration of the gross functional neuroanatomy of the language system as was developed in Chapter II above. Thus there seems to be good reason to consider the phonological component and the lexicon as distinct parts of the system. There is also reason to consider the four language modalities (visual, auditory, tactile and verbal) as subserving the central language system, thus providing a rough distinction between competence and performance in the sense that the four modalities represent an actualization or realization of the central language system. The significance of this neurological model may be better appreciated when it is recalled that even performance errors pattern along the lines of the central language system. In even the most obscure jargon aphasia, the individual syllables are those of English not some other language; and in even the most severe dysarthria in which all utterances are little more than a nasal sound, intonation patterns for words and sentences may be intuitively discerned. It is also evident that there are several levels of complexity or abstraction in these performance systems -- the system itself, the linguistic use of the peripheral system and the cortical control areas for them, etc. And it was also noted that a speech deficit could be due to either a problem in the subsystem qua subsystem, a breakdown in the subsystem qua linguistic performance modality or a breakdown in the central language system itself; the last, of course, having the potential of manifesting a deficit in all four modalities and being the deficit of major interest here.

But these are only rough outlines, approximating the rough 'black-box' models of the linguistic system as sketched in Chapter I. Given the theoretical *and* the empirical separation of the lexicon, for example,

what we can find out about the specific nature of a lexical item, in neurological terms correlated with linguistic notions, is even more important. It must be emphasized that, for specific questions of this sort, there is virtually *no* neurological data (of the sort in Chapter II); there *is* behavioral data, however -- the language behavior of aphasics -- to which the remainder of the dissertation is devoted.

It is important to understand first exactly how aphasic language behavior constitutes empirical evidence so that this evidence is neither taken for more than it is nor discounted as valid empirical data in linguistic research. As studied here, aphasic language behavior is not approached statistically. Rather it is approached in virtually the same manner as other current linguistic investigations -- on a single informant basis. By and large statistical approaches perforce measure performance variables for the simple reason that if a 100% correlation were established for any factor (i.e. if there is no inherent variation such as is found in performance factors) there would be no meaning to a statistical analysis. For example, word frequency studies are statistical because different people have different frequencies of word usage; it would make no sense to do a statistical analysis of the occurrence of noun phrases -- *except* as a frequency study -- or a statistical analysis of the features [male/female] per se, since these are universal aspects of the language system itself. To put this another way, statistical analyses of language are predicated upon or assume the universal existence of the categories being measured: a frequency study of word usage presumes the universal existence of the category *word* (or even the existence of a particular word), to consider this simplistically. The question posed in this research deals with these underlying categories and can be phrased thus: given a linguistic construct such as the distinctive features [male/female], can it be shown that these are meaningful aspects of the representation of language in the human brain by virtue of evidence that these features can be lost due to brain lesions?

### C. Methodology

There are three methodological aspects of the research which should be made clear: (1) how loss constitutes evidence for the original, normal presence of a construct, (2) the aphasic patients which were studied and the controls used, and (3) the elicitation and test techniques used to obtain the data.

A standard research paradigm for studying function in physiological psychology, neuroanatomy, biochemistry and physiology is to ablate tissue or otherwise block or inhibit the normal functioning of the tissue by electric shock or by chemical reaction. The assumption is that a comparison of pathologic with normal behavior under such controlled

conditions, reveals the function of the tissue so manipulated. Virtually the same assumption underlies the neurological investigation of aphasia and other human pathologies resulting from brain damage, as has been repeatedly referred to in Chapter II. The difference is that in the human brain one cannot experiment as with animals, excepting in the limited manner employed by Penfield and Roberts [1959] which has already been discussed, and consequently one must carefully study a great number of patients and extrapolate from such studies. One of the better examples of such study and extrapolation is the work of Russell and Espir [1961].

Loss or impairment due to brain lesion thus provides positive evidence for function under the conditions just noted. A similar situation but with quite different inferential potential, exists when a brain lesion affects function 'A' when function 'B' is the one being investigated. In this case the evidence is negative for only one of potentially several (unknown) functions can be ruled out. For example, if one is looking for evidence that subcortical structures such as the basal ganglia or thalamus do not play a significant role in the central language system, *negative* evidence is provided by noting that lesions in these structures have the functional concomitant, dysarthria; one might speculate that a vote is cast in favor of the view that cortical structures only are utilized for the phonological component of the language system since to date the evidence shows that lesions in Broca's Area produces apraxia of speech. There is an additional aspect to this research paradigm which is quite problematic: the distinction between loss and impairment. This is of particular importance in the discussion of apraxia of speech in Chapter IV, Section A below. The loss of a particular part of speech behavior can be considered as either loss of the part itself or loss of access to it. As noted in the preceding discussion, these may be empirically equivalent in *any one modality* (the data would be the same regardless of which explanation might be correct), although these may be distinguished from each other with bi-modality data. If a loss is involved, it will be manifested in all modalities; if an access problem is involved, it will be only in the modality affected.

If we find evidence of variable impairment, such as the case in which a patient knows a word today which he did not yesterday and may not tomorrow, it is obvious that there are different possible neurological explanations. It may be that the function itself is impaired or it may be that the actualization of the function is impaired; that is, the representation of a word may be variably impaired or the ability to associate the representation with the appropriate neuromuscular commands may be variably impaired. The latter is the "tracking" function mentioned in Chapter I; it's counterpart is the "guessing" strategy. This introduces a different and much more obscure theoretical issue. It is thus conceivable that a *competence* deficit may be variable, i.e. may fluctuate in the sense meant by Weigl and Bierwisch [1968] like a performance deficit. If the deficit is manifest in all four modalities, it must be a competence deficit even if there is a



fluctuation in its effects. Exactly how this would affect the model proposed in this research is not clear and is not likely to be resolved until our understanding of the neurological basis of competence is significantly better.

Notice however, that under any of these conditions we may infer the neurological reality of the function in question. This is the reason why Weigl and Bierwisch were able to claim that they had verified certain aspects of the competence model and at the same time dispute that aphasia affected competence. It is not necessary to invoke an additional function in the brain, as do Weigl and Bierwisch with the notion of a performance function being "bound" to a competence function. Regardless of whether a deficit is part of performance or competence, the behavior which results from it can provide direct evidence for aspects of linguistic structure as long as that data is consistently related to the aspect in question. To use a simple example, aphasic speech behavior marked by a lack of words (as in agrammatism) may be due to the loss of part of the lexicon, part of the syntactic structure or to the impairment of a lexical selection strategy. In all cases though *words* are the linguistic unit involved and such speech behavior constitutes evidence for their status as neurological units in the language system. Even in a trivial example such as this it is easy to see that the notion of "binding" a selection strategy to the competence unit "word" is little more than terminological superstructure. Isolating the strategy itself, of course, is another matter of no little interest to models of speech performance.

The data discussed below was obtained primarily from patients who suffered at least cortical damage (exceptions are noted in Appendix A.3), but since the clinical descriptions were not checked against further medical information, it is not known to what extent there was subcortical damage. Consequently, this data is of no value in further support of the hypotheses presented in Chapter II. This is not a serious problem for two reasons: first, the literature cited in Chapter II provides ample evidence for the neurological model proposed, and second, the main purpose of this research is to seek data that establishes the neurological functions and correlates of linguistic hypotheses. For this purpose, precise anatomical descriptions are not as important as the behavioral data. Once certain linguistic hypotheses can be neurologically established, future research projects can consider the task of making the correlations between the *grammar* and the corresponding neuroanatomy.

#### D. Transcription System

In order to simplify reading the data presented below, the "quasi-phonetic" spelling system proposed by Chomsky and Halle [1968](p. 28)

will be used whenever the data is not directly interpretable in standard English; such transcriptions will be indicated by slanted lines /-----/. Otherwise, standard English orthography will be employed. The symbols are shown in Table 2, with examples indicating their phonetic values.

<u>Symbol</u>	<u>Example</u>	<u>Symbol</u>	<u>Example</u>
/p/	pat, tap	/i/	bit
/b/	bat, tab	/I/	bite
/m/	mit, Tim	/E/	beet
/t/	tap, pat	/e/	bet
/d/	dip, pad	/A/	bait
/n/	nick, kin	/æ/	bat
/k/	cad, pack	/u/	put
/g/	gad, dig	/U/	unite
/ŋ/	sing	/O/	boat, flow
/f/	fat, wife	/ɔ/	bought
/v/	vat, wives	/a/	pot
/θ/	thin, lath	/ə/	but, Rosa
/ð/	the, lathes	/aw/	bout
/s/	seal, lease	/uw/	boot
/z/	zeal, Liz	/ɔy/	boy
/ʃ/	sure, rush		
/ʒ/	azure		
/tʃ/	church		
/dʒ/	judge		
/r/	red, deer		
/l/	leaf, feel		
/y/	yes		
/w/	west		
/h/	head		
/ʔ/	uh_uh [ʌʔʌ]		

(\* /ə/ is being used for both the stressed vowel in words like but, etc., and the unstressed reduced vowel as in cutter /kətər/)

Table 2. Phonetic Transcription System

### E. Subjects

Thirty-eight patients from the Long Beach Veterans Administration Hospital's Aphasia Clinic provided the aphasic data for this study. The Clinic is under the direction of Mrs. Milfred McKeown. I worked with twelve of the thirty-eight subjects personally, directly administering the tests described below and eliciting data through free conversation. These sessions were recorded on a Uher 4000 Report-L tape recorder. In addition, tape recordings of interviews and tests conducted by the LBVAH Aphasia Clinic staff with twenty-six were analyzed.

In the discussion below illustrative material from some of these patients is presented. Data obtained from the twelve patients with whom I personally worked is identified by two initials preceding the data. Data provided by the LBVAH tape recordings is identified with the letters LBVAH. For example, data from W.L. was obtained in a direct interview situation and data from LBVAH-C.S. comes from a borrowed tape recording made by one of the staff members.

The clinical diagnosis of thirteen of the twenty-six patients interviewed by LBVAH was on the tape, i.e. the patient was identified as a Broca aphasic, a Wernicke aphasic, an Ammesic aphasic or a Global aphasic. Since the classification of aphasia is often idiosyncratic, Appendix A is an outline chart showing the impaired language functions for each of these three general types of aphasia, as currently analyzed and defined by the LBVAH Aphasia Clinic. Appendix A.3 states the clinical diagnosis of the patients actually cited in the following analysis; for those personally interviewed, the etiology of the brain damage is also given.

Eleven subjects comprised the control group: a nine year old boy, five speech therapists at LBVAH, and five graduate students at UCLA. All were native English speakers, free of obvious speech defects or peculiarities. However, one of the graduate students had had severe transient aphasia as the result of an automobile accident just under two years ago. All the control subjects, including the former aphasic, could do the tests without difficulty or noticeable error. The data from the control subjects is not presented since it is indistinguishable from what one would expect of normal speakers of English. It was considered necessary to use a control group (including a nine year old) to make sure that the tasks were understandable and could be carried out by non-aphasic subjects.

#### F. Description of tests

Six tests were specifically designed for this study. It was necessary to develop new testing procedures since the clinical tests ordinarily used in either diagnosis or training of aphasia patients cannot provide answers to the particular questions being asked in this study. These tests are reproduced in Appendix B.

Test #1 consists of five pairs of sentences which may be combined either by coordination or subordination.

Test #2 consists of a narrative passage containing underlined Noun Phrases (NP's) to be pronominalized. This was designed to check on the ability of the patients to recognize or utilize those semantic features relevant to pronominalization.

Test #3 consists of ten partial sentences paired with full sentences which can become the complements to the partials. This was primarily designed to check complement structures of the infinitive, ING and THAT-clause types.

Test #4 consists of fifteen pairs of complete sentences. The subjects were asked to combine these into single coordinate sentences.

Test #5 is in two parts. In part one, ten sentences have blank places, which the subjects had to fill with lexical items; this was designed to check the subjects' use of different syntactic categories. Part two consists of ten sentences some of which are semantically deviant by virtue of violation of selectional features; this was designed to check judgments of acceptability.

Test #6 consists of 45 words which were to be used in a sentence or defined by the patient, or both. It was designed to test sentence construction ability and other aspects of the grammar.

#### G. Interview procedures

For those patients personally studied a rather informal interview procedure was used. Through the courtesy of the LBVAH Aphasia Clinic staff and the personal assistance of Mrs. McKeown, it was always possible to meet each patient individually in a comfortable room away from others in the clinic. The six formal language tests, (Appendix B) were used for the core material. These were supplemented by conversations recorded on tape and a few other kinds of tests developed on the spur-of-the-moment when a particular aspect of a patient's language abilities seemed worth exploring in more detail.

An important aspect of these testing and interview procedures is what might be termed the "multi-modality" approach. This approach attempts to circumvent the influence of one of the peripheral subsystems of language by using input stimuli in both input modalities (visual and auditory) and eliciting output data in both output modalities (verbal and tactile). For example, in test #4, the coordination test, the two sentences are given on paper and read aloud to the patient; at the same time the patient's answer is solicited both as a spoken and as a written response. On most occasions the response data was the same; however, some interesting exceptions were noted and will be discussed below in the appropriate section. The theoretical assumption underlying this test-interview procedure is that the use of two modalities *requires* a computation in the central language system and any abnormal language data is thus a reflection of deficit in that system and not attributable to a deficit in one of the peripheral systems. This seems to be comparable to the "de-blocking" technique used by Weigl and Bierwisch [1968], insofar as the effects are concerned.

To my knowledge no other researchers have made use of this technique in studying aphasia.

#### G. The Linguistic Framework

Current views of aphasia as only disruptions of performance (Weigl and Bierwisch [1968]) or disruptions of temporal sequencing (Lenneberg [1967]) and the concomitant view that somehow the "essential" aspects of language (competence) remain intact in persons suffering brain damage, are considered inadequate. The arguments and facts which have been presented already and the data which follows clearly show that damage to the central and peripheral nervous systems of man can affect the particular system or modality itself, can affect the performance or modality-bound aspects of language or can affect the central language system. It seems clear that there are at least three approaches to the data, all of which fall within the scope of the preceding arguments: (1) a discussion of aphasic language behavior in terms of linguistics with a view toward presenting an adequate linguistic classification of such syndromes, (2) a discussion of linguistic hypotheses in terms of aphasic language behavior with a view toward empirical verification and thus affirmation of the substantive (as opposed to formal or theoretical) nature of such hypotheses and (3) the correlation of linguistic hypotheses and aphasic language behavior so classified (i.e., both (1) and (2)) with their neurological substrates, anatomical loci and ultimately the precise nature of the physical and functional defects.

Although I believe the first to be a desirable and very useful project, it falls more within the domain of clinical application. The third is, of course, a goal to be aspired to but unfortunately is considerably beyond the level of this study or present neurological information. The second is a possible project and is what the remainder of this dissertation is devoted to. The basic question is this: do the constructs of linguistic theory, as exemplified in current proposals about the structure of English, have substantive validity such that aphasic language deficits are predictable consequences of those constructs? From this question it is easily seen that there are three possibilities: (A) the data does fall out in this fashion; in such a case the relevant linguistic hypothesis can be said to be empirically confirmed. (B) the data does not fall out in this fashion and thus predictable consequences are not in fact discovered; in such a case one has neither confirmed nor disconfirmed a linguistic hypothesis. (C) the data indicates that there are substantive units and operations in the language system for which there appear to be no currently proposed linguistic hypotheses; the last case of course is the familiar problem in science of discovering data which is not within the domain of the current paradigm or model. As one would expect, there is data of all three types, although my major interest in this research has been possibility (A) and most of the discussion will be devoted to it.

Current linguistic theory divides the structure of language, the grammar, broadly into semantic, syntactic and phonological aspects. Various levels of the grammar have been proposed; these approximately define "components" of the grammar which were depicted in Chapter I Figure 2: the *semantic component*, composed of the rules for the combination of elements of meaning into 'readings' for phrases and sentences, the *syntactic component*, comprised of the level of deep structure (the set of rules or relations common to all languages from which a subset of base rules is taken for English), the level of transformations (a set of operations which manipulate deep structures which are the product of the first level; again, there is presumably a universal set from which the particular transformations for English are taken), and the level of the lexicon (the set of 'words' for a language, represented by a subset of semantic features, a subset of syntactic features and a subset of phonological features for each lexical item), and finally, the *phonological component*, composed of a set of rules that operate on a phonological representation (this representation includes the subset of phonological features of each lexical item together with the syntactic information provided by the product of the syntactic component) to convert it into a phonetic representation (which is a specification of a sentence that putatively correlates directly with the acoustic signal); the phonological (or systematic phonemic) representation and the phonetic (systematic phonetic) representation constitute the two levels of the phonological component.

Earlier versions of linguistic theory considered the semantic component to be analogous to the phonological component in that semantic rules were said to be interpretive, operating on structures formed by syntactic rules -- in this case those of the deep structure not the transformations. Some current versions dispute this conception and propose that the syntactic component does not contain a level of deep structure that represents all the necessary information for the specification of meaning followed by the transformational level; rather, it is suggested that semantic and syntactic rules jointly form a component, with no separate levels, and that the universal base rules (or deep structural level) are very abstract and contain virtually no language-specific elements. There are many theoretical arguments on both sides of this question which will not be reviewed here; for discussion see Chomsky [1969] and McCawley [1968].

It should be made clear once again that the linguistic theory just discussed above upon which the research in this dissertation is predicated is that of transformational grammar. There are, of course, other linguistic theories such as stratificational grammar or tagmemic grammar which, for the reasons outlined in Chapter I, will not be discussed. The earlier version of transformational theory, the so-called "standard theory" (Chomsky [1969] p. 5) has been diagrammed by Postal [1968] p. 204; his model of the grammar, the *competence model* is shown in Figure 7:

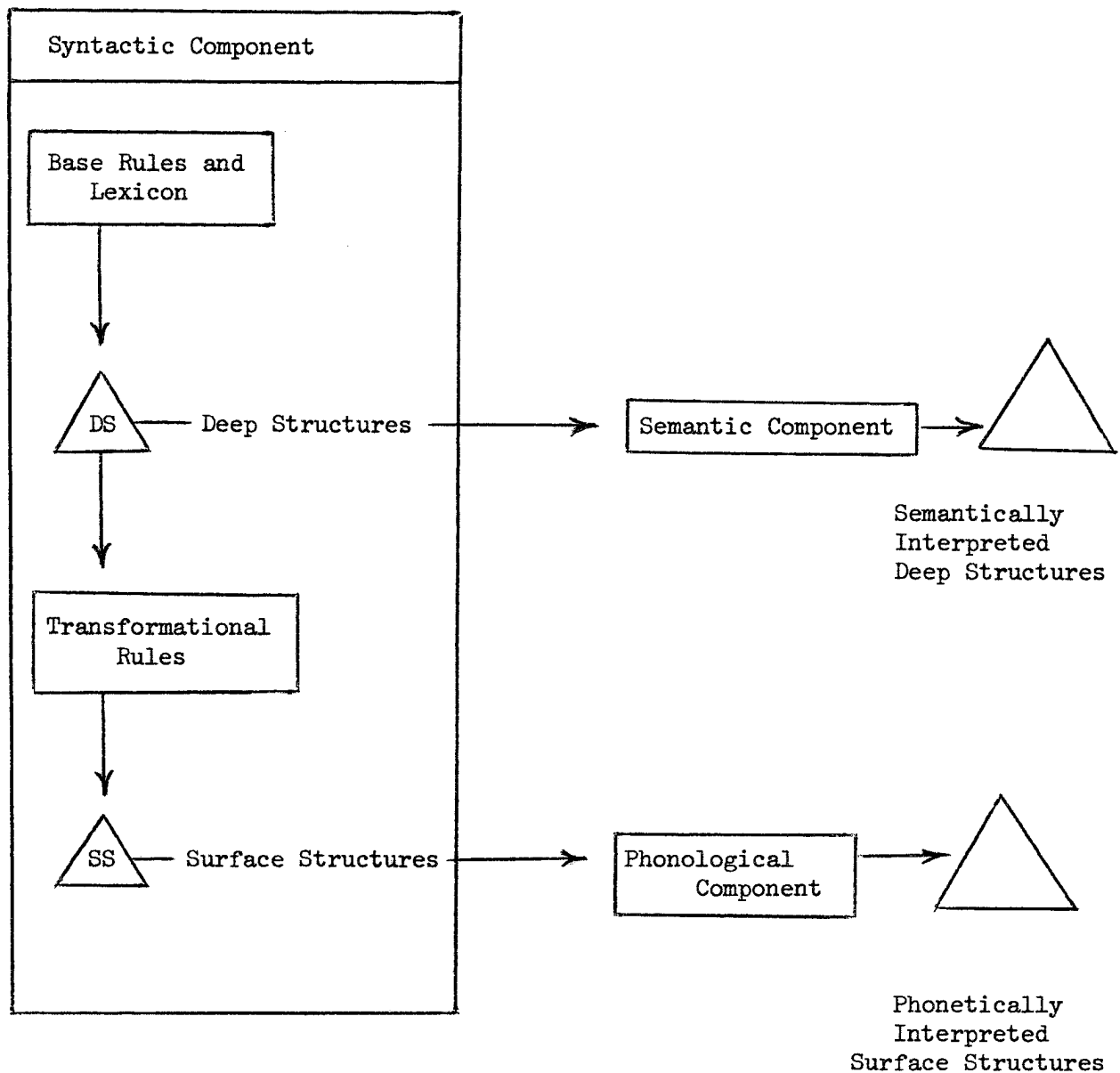


Figure 7. Postal's Model of Competence

Some current versions of transformational theory (McCawley [1968]) would combine the "Semantic Component" and the "Base Rules" into one component (presumably retaining the lexicon as a separate component), thereby obliterating the *level* of deep structure. This variation of the standard theory, called "Generative Semantics" or "Semantax", will be considered in more detail shortly. The competence model just diagrammed was not intended to be a schematic of the speaker/hearer but to be an abstract

characterization of language structure. The arrows do not represent a sequential order of neurological events, an order presumably characterized by a performance model in the standard theory. As discussed in Chapter I however, I do not believe the performance-competence distinction is actually an opposition in terms of the grammar itself, if one examines the matter carefully. The grammar *in toto* is intended to reflect the real underlying structure of the language system and therefore must be a model of the speaker/hearer, albeit an abstract one. That it is not intended to be a *working* model is beside the point. In the earlier abstract model the semantic rules are interpretive (on the analogy of the phonological rules), i.e. they are dependent upon (formally) syntactic deep structures; in generative semantics the deep structures develop the semantic information and the syntactic structure together. If we consider the very plausible notion that in actually speaking we must organize our thoughts prior to determining the linguistic structure to be used in expressing them, we find that this notion still does not legislate for one or the other model. The reason is that all the semantic rules, separate from syntax or not, that have ever been proposed, to my knowledge bear little if any relationship to thinking processes, organizing one's thoughts to speak or even the most general aspects of cognition. Consequently the assertion that we think first and then speak (or conversely, hear the utterance and then comprehend it) is without empirical consequence in this debate, regardless of whether or not it is true. If we assume for the moment the following conception of the total grammar, then I believe the serious linguistic issues can be made more clear. Let us limit the notion 'Grammar' to such aspects of language which either follow or precede *thinking*, on the pragmatic grounds that thinking is for the moment not amenable to linguistic analysis. Thus, what one does in deciding what to speak or in contemplating what was said, let us call this the Cognitive System, can be ruled out of the grammar by definition. We can then think of the grammar as dependent upon the Cognitive System; its relationship to the Cognitive System is a matter to discuss in a working model of the speaker/hearer and therefore the issue about the semantic and syntactic aspects of the grammar proper is no longer a question of dependence but of autonomy. There are then two possible box diagrams which characterize how the grammar might be organized, depending on whether or not there is evidence for the separation of semantic and syntactic aspects of language. (The question of this autonomy is considered again in detail in Chapter IV.)



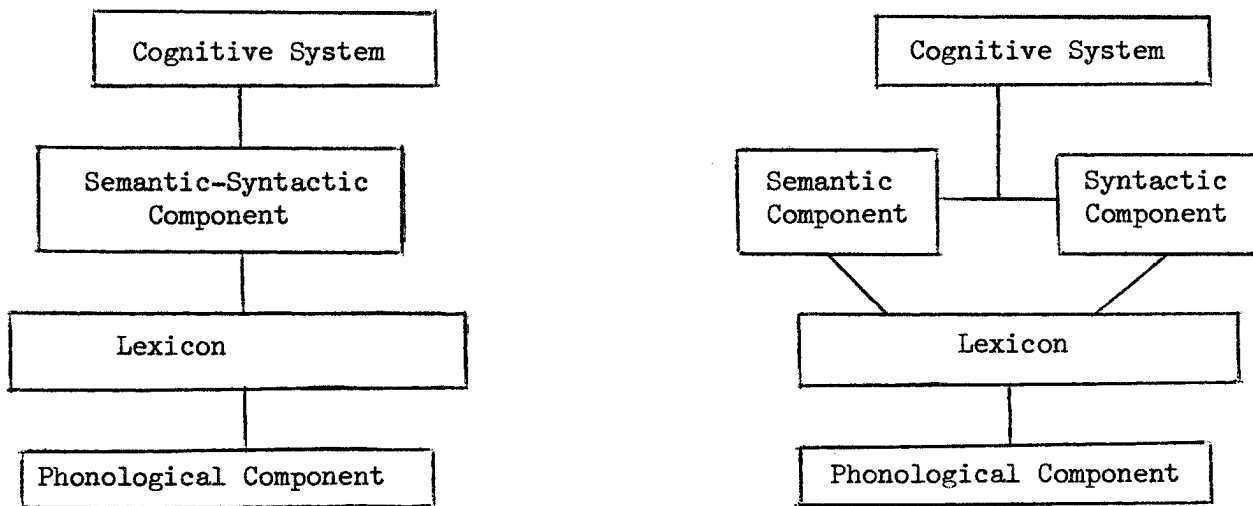


Figure 8. Organization of the Grammar

Within the linguistic model outlined in the preceding pages there are a number of theoretical issues; the research data in Chapter IV is directed to three of these issues: (1) Levels in the grammar and particularly the autonomy of syntax and semantics, (2) some questions about the organization of the lexicon, and (3) one problem concerning the underlying structure of noun phrases. The general manner in which aphasic language behavior counts as empirical evidence was discussed above in sections A-C. In Chapter IV the three issues will be described in fuller detail, including the specific hypotheses which will be argued for. What is important to bear in mind is how the data relates to theoretical hypotheses in linguistics. This data is indicative of substantive units or constructs which are part of the representation of language in the brain and as such provides facts which must be represented in the grammar, if the grammar is to be psychologically or neurologically adequate. There are two kinds of theoretical hypotheses which are *not* relevant to this data. The most obvious one is the case in which two hypotheses adequately account for the empirical facts, or employ the correct substantive units, but have different theoretical (not empirical) implications. A less obvious one is the case in which the theoretical hypothesis is simply not intended to account for such data; this is of course tantamount to claiming that the hypothesis has no neurological reality, or as it might actually be expressed, that the hypothesis is very abstract and only related to the neurological facts by some unstated or unknown function(s). The latter position is in a sense analogous to many aspects of mathematical theory in which the equations and models are self-contained; any relationship to things other than mathematical entities is fortuitous. Although linguistic theories can be abstract in this sense, as such they are not of interest

to me. I am expressly concerned with linguistic theories which have predictable and therefore verifiable consequences for all aspects of language structure and behavior and the neurological substrates of that behavior.

The above discussion outlines the linguistic framework for this research and the particular grammatical constructs and hypotheses which I will investigate. It will become very apparent that the data from the aphasics cited in the next chapter is linguistically much richer than I will have overtly commented upon. This is no more surprising than the fact that the above remarks barely touch upon the number of arguments, hypotheses and facts within linguistic theory. An aphasic's linguistic ability is still a reflection of, or a product of, the underlying system even though that system has been disrupted, impaired and in part lost. A total loss or disruption would not be empirically measurable for the simple reason that there would be no speech output or input. At the same time, trying to isolate specific linguistic hypotheses from even a partially inactive language system creates certain theoretical and methodological problems; however, these are not much greater than those attending linguistic investigation of a normally functioning language system. The major difference is that it is difficult to confirm a piece of data in the case of aphasics; with a normal person one can sometimes ask, or in the case of the linguist himself, there are intuitions. This is why many of the tests were presented in more than one modality and whenever possible, a result was checked in more than one test. In the final section of the next chapter, a few other linguistic hypotheses will be briefly considered and some further research problems will be noted.

## Chapter IV. The Linguistic Issues: Evidence from Aphasia

### A. Levels in the Grammar

The question of levels in the grammar and particularly whether or not there is a level of deep syntactic structure can be considered from several points of view, as noted by Bedell [1969]: as formal mathematical objects, as crucial points in the rules and as a psychologically real part of the grammar. It is the notion of level as it reflects a neurologically distinct aspect of the central language system (broadly equivalent to psychological reality) that is of interest here. Even this is a very large set of problems of course and therefore only certain points will be explored. What will be considered is the autonomy of: a (physical phonetic) level of neuromuscular commands, surface structure (phonological representations), deep structure (the basic grammatical relations) and semantic features (principally those which play a role in selectional restrictions). Evidence will be given indicating that each of these aspects of the grammar can be impaired or disrupted in isolation which lends strong support to considering them functionally independent.

The preceding three chapters have outlined a general linguistic model of competence and performance, a neurological model of the central and peripheral language mechanisms in the brain which correspond to the linguistic model, and many of the details of the linguistic model of competence. In this final chapter the language behavior of aphasics is considered in terms of the linguistic model. Even though the particular hypotheses to be discussed were outlined in the preceding chapter in terms of transformational theory, the following research is not to be construed as confirmation or refutation of the transformational model of grammar; it is merely the linguistic model I happen to be making use of. Any linguistic theory will be accountable for such facts about the neurology of language that are posited here.

The notions component and level suggest the possibility that either different brain mechanisms or different neuroanatomical substrates might be involved in the language system. One would expect to find, if such notions are valid, language deficits corresponding in some way to the disruption of one level and the preservation of others. A simple example illustrates this principle clearly. The physical phonetic representation of an utterance -- the output of the grammar so to speak -- constitutes a functional neurological division in the language system (a level) inasmuch as the deficits associated with dysarthria clearly

demonstrate that only the neuromuscular actualization of the utterance is affected. Speech disorganization in dysarthria is contingent upon the speech musculature which happens to be affected (or the musculature of the arm in motor agraphia) and not upon the phonological organization which is its precursor in production -- the latter is affected in apraxia of speech (or writing). Put another way, dysarthria is a defect of the verbal modality itself -- e.g. all uses of the tongue might be affected -- whereas apraxia of speech affects only the linguistic use of the modality.

Two of the patients whom I studied, F.Z. and J.W. were clearly dysarthrics; their ability to handle the language tests (Appendix B) is normal and thus they also serve in one sense as control subjects. They are of some interest here since each had one of the production modalities and its corresponding input modality virtually inoperable: F.Z. could not write and J.W. could not speak (except in an indistinct nasal mumble). F.Z.'s reading ability seems to have been severely impaired, too, although it is difficult to tell since his control of the postural muscles of the head and neck and his control of the arm and hand muscles was so poor that he found it difficult to get into the physical position to read. J.W.'s auditory comprehension seems to have been severely impaired, but again it is difficult to say; since he did not respond to spoken speech it was not clear to what degree spoken speech made sense to him. Consequently, the tests were given to them as follows: for F.Z. I read aloud the tests and I wrote down his responses. When a response was not clear I asked him to repeat it several times and I repeated it back to him for confirmation; incidentally, if I repeated it back incorrectly, i.e. not as he had given it to me, he immediately perceived the error and corrected me. For J.W. I wrote the instructions to the test, gave him the test on paper and he wrote his responses. Some sample responses to the Coord./Subord. #S# Test #6 (forming one sentence from two source sentences) and the Word-to-#S# Test #8 (forming a sentence which either uses or defines a stimulus word) are reproduced here; the other tests given them (#7 to J.W. and #1 to F.Z.) are again indistinguishable from normal language ability.

(2) Test Word (#8)

F.Z.'s Response (spoken)

- a. expect
- b. remember
- c. arrive
- d. perform
- e. challenge

My wife expects a baby pretty soon.  
 I don't remember nothing.  
 My mom and dad will arrive soon.  
 The clown performed a trick for us.  
 I was challenged to a fight.

- | (3) Test Item (#6)                  | F.Z.'s Response (spoken)                       |
|-------------------------------------|--|
| a. i. Mike is a happy man.          |  |
| a. ii. Peter is a happy man.        | a. Mike and Peter are both happy.              |
| b. i. John gave his wife a present. |  |
| b. ii. John gave his son a present. | b. John gave his wife and son a present.       |
| c. i. The tiger caught the rabbit.  |  |
| c. ii. The tiger ate the rabbit.    | c. The tiger who was hungry caught the rabbit. |

- | (4) Test Item Word (#8) | J.W.'s Response (written)   |
|-------------------------|---|
| a. perform              | He was to perform the play well.  |
| b. conceal              | I tried to conceal the gun in my coat pocket.                               |
| c. persuade             | I tried to persuade Bill not to go home.                                    |
| d. amaze                | The trick I knew would amaze anyone who saw it.                             |
| e. forgive              | Sam did not forgive me for the insult.                                      |
| f. expect               | I tried to tell the man not to expect the dog,<br>unless it was a good day. |
| g. require              | I did not know if they would require him to<br>shoot the men quickly.       |

I think these few responses clearly indicate the intact nature of the central language system, despite the nearly complete disruption of the peripheral modalities, particularly in J.W.'s case, for he could carry on a conversation by exclusive use of written language. The conclusion drawn from these cases is that there is a functional level separating each peripheral language modality from the physical system subserving it, an interface between performance and the organism itself. In the verbal and tactile modalities it is a level of neuromuscular motor signals; in the auditory and visual modalities it is the analogous sensory signals. Strictly speaking, the parallel does not hold in the visual modality for F.Z., for in fact he is not blind but only unable to read -- a syndrome referred to in the classical literature as visual agnosia which is an inability to comprehend written language (see Nielsen [1946] pp. 251 ff.). This deficit is extremely hard to study and, since my interests lie more in central language system deficits, I did not explore it in depth with any of the patients.

## 1. Phonological levels and rules

In considering the phonology of a language, the present model of transformational grammar (specifically, generative phonology) includes a number of levels and different sets of rules which relate to the sound system. There are those rules which are called either Morpheme Structure (MS) Rules or Conditions which specify the segmental and sequential redundancies in a language. For example, in English, for nasal consonants, voicing is a redundant feature, since there are no oppositions between voiced and voiceless nasals. It is also true that in English there are no 'words' or 'morphemes' which start with a velar nasal. Thus, \*[ŋa] is not a 'possible' English word. Neither of these facts about English is due to universal articulatory constraints since such words do exist in other languages. If we assume that these rules are real aspects of the brain's language system, one may easily surmise what would happen to speech output if this section of phonological rules or constraints were disrupted. To my knowledge one does not find aphasics whose utterances fit the sound structure of some other language, or the graphemic patterns either; one does find utterances that are in fact non-occurring in English, but only in the fortuitous sense -- they could be English words (i.e. they obey the set of Phonological Redundancy or Morpheme Structure rules). We find, in other words, that in the utterances produced by some aphasics (who have *no musculature deficits*) the 'words' are no longer identifiable as English vocabulary but the syllables of English and the junctures indicating word boundaries remain. In these cases it is obvious that the phonological representations (P-reps) of lexical items are disrupted, although whether it is the P-rep itself or the neural actualization of it (the "tracking" function discussed in Chapter I) is open to question. What needs explanation however, is the status of the intact MS rules -- are they an aspect of the central (competence) or peripheral (performance) language systems. If we assume the MS rules are part of competence we have then a situation in which one level of competence, the P-rep, is disrupted and another level, the MS rules, which is *contingent* upon the former, is intact. This is conceivable but not very plausible. On the other hand, if we assume the MS rules are part of performance -- specifically, a set of neural-muscular 'habits' associated with a specific language -- the aphasic speech in question can be very simply explained. A competence level -- the P-reps -- is disrupted but the neural-muscular habits associated with English are intact; thus the output obeys the phonological constraints of English although it is in fact non-occurring.

Research on other forms of skilled behavior indicates that in the process of learning skills a set of neuromuscular habits are formed. It seems quite obvious that the difficulties encountered by adults in learning the sounds of another language are related to the contrast between the set of n-m habits acquired when learning the native language and the n-m gestures required to imitate the foreign sounds. Notice, too, that such a view as just expressed (placing the MS rules in the performance system) makes it unnecessary to consider any relationships

between innate properties of the brain and MS rules; however, if one considers MS rules part of competence, then it is necessary to distinguish innate competence and learned or acquired competence on still another level. Innate and acquired competence will probably have to be distinguished, but at the moment both are quite vaguely understood. The argument here is that at least with respect to MS rules, one need not invoke unknown theoretical superstructure to account for the facts. Minimally, the above considerations raise questions regarding the inclusion of MS rules as part of competence. There seem to be intricate connections between competence and performance and often the two are difficult to distinguish. What is clear is that whichever explanation turns out to be correct, the aphasic data clearly supports a linguistic model which incorporates such a set of segmental and sequential morpheme structure constraints.

Besides these MS rules, the Phonological Component in a grammar consists of a set of Phonological Rules (P-rules) which convert a lexical representation to its phonetic specification. These include both (a) more abstract rules (e.g. a rule which specifies for a speaker of English when a /g/ is pronounced, as in *resignation*, and when such an 'underlying' /g/ is deleted as in *resign*; or a rule which determines the proper assignment of stress in English words) and (b) low level phonetic rules (e.g. a rule which states that in English initial voiceless stop consonants are aspirated before stressed vowels as in *pit* [p<sup>h</sup>it], but not in *spit* [spit]). These are also language dependent rules. The low-level phonetic rules seem to be similar to the MS rules discussed above, as can be illustrated by the difficulty of attempting to teach an English speaking adult to produce initial non-aspirated voiceless stops before stressed vowels in French. Very often the physical ability to control the onset of voicing after the closure of the consonant is impaired, and while they may become aware of what they are doing wrong they are unable to correct the production errors. Again, non-dysarthric aphasics do not make such 'errors' they do not produce unaspirated voiceless stops where they should be aspirated, nor do they aspirate stops after /s/; they nasalize vowels before nasal consonants (as in *can* [kæn]) etc. That these rules seem to be non-disruptable where muscular control remains unimpaired but other, higher-level phonological disruption occurs, is again indicative of the complex interrelationship between competence and performance rules and argues against the position that it is only performance which is disrupted in aphasia.

On the other hand, there are phonological rules which seem easily impaired in aphasics, such as those for stress assignment. It is not difficult to find aphasic speech which would be explained by the loss of the ability to assign stress properly.

We have so far referred to the set of MS rules and P-rules, and have pointed out that while phonological disruption does occur (see below) the linguistic constraints on sequences and phonetic features of utterances

remain intact. This of course depends upon the fact that the patient has no dysarthria and no deafness. The lack of dysarthria or deafness implies that the motor and sensory neural signals are not affected by deficits in the peripheral modalities concerned.

What, then, accounts for the kinds of phonological disruption which can be found in aphasic speech? Phonological rules operate on strings of formatives with given phonological representations (i.e. the surface structure of a sentence). The individual items are, according to this theory, stored with their phonological or lexical representations specified.<sup>1</sup> The difference between lexical and phonological representation is not of concern here, and is specified by Chomsky and Halle [1968] primarily in relation to grammatical readjustment rules which are not under discussion. I shall use the term phonological representation to refer to the lexical specification as well as the representation in the surface structure. Linguistic analyses of apraxia of speech (Johns [1967], Blumstein [1968] and to some extent Lehiste [1965]) suggest that this phonological representation of utterances or words can be impaired; the work of Weigl and Bierwisch [1968] is also suggestive along these lines. Assuming that the cognitive system and the semantic and syntactic components were still intact (regarding this assumption, more will be said below), we would expect that a patient with a general disruption of phonological representations would appear to be responding to questions, carrying on a conversation and otherwise behaving normally psychologically. We would not expect him to be markedly upset by the fact that his speech was often unintelligible because the deficit would be manifest in perception as well as production of speech; this is in contrast to the usual Broca's aphasic (ordinary apraxia of speech) who becomes very upset with his inability to speak adequately because he knows what the phonological representations, and hence the phonetic output, ought to be. This latter deficit will be discussed below, too. The following interview with LBVAH-G.C. illustrates these predictions:

(5)

- MM        Good morning, George.
- G.C.        yes
- MM        How are you today?
- G.C.        well
- MM        Feeling fine today?
- G.C.        oh, it's /šer men gAðer, mI sen wən kAkər/
- MM        How long have you been in the hospital, George?
- G.C.        Oh, /mI sak šur rA ten lat ten/





Apraxia of speech and auditory verbal agnosia more frequently have less severe concomitants in language use. In speech production for example, it is usually the case that an utterance will contain some normal speech and then a word or two will be garbled. With the neuromuscular level patently intact (which can be checked by neurological examination, of course) there are two possible linguistic explanations for this deficit. Clearly, the entire level of phonological representations cannot be disrupted or there would not be a significant context of normal speech. Either the level of lexical representations is disrupted or the tracking mechanism for relating these representations to the neuromuscular commands is disrupted. (This was discussed in Chapter I above; lexical representations may now be equated with the "conceptual units" discussed in that chapter.) In some cases we may be able to distinguish these alternatives by observing whether speech comprehension is disrupted for the word in question in addition to speech production. If the word is affected in both modalities, it is more likely that the lexical representation itself is disrupted; if the word is only affected in speech production, it is more likely that the transfer (tracking) to the level of neuromuscular commands is disrupted. *In either case*, the level of lexical representations is established as a functional neurological unit as the data below shows. Consider, for example, an attempt by LBVAH-L to pronounce the word *California*, she said: /kandes:wanEyes/. Even though the number of syllables is correct (which *could* be fortuitous) and one might speculate that the initial segment and part of the final vowel cluster is correct, either the lexical representation or its n-m specification is not intact. In a test to identify pictures (in color) LBVAH-W responded as follows (I have not shown his correct responses here. The following is about one third of the test responses, the others were correct):

Picture	LBVAH-W's Response word (spoken)
(6)	
a. raspberries	red b <u>l</u> erries
b. a hamburger	/bærpegE/ # /maembergør/
c. a cucumber	/kæmør/ # /kUber/
d. a watermelon	watermen, mettle, watermettle
e. a dictionary	/ʃinjinerE/
f. fruit cocktail	fruit tail

On a repetition task, LBVAH-W responded as follows to the stimulus word:

Word	LBVAH-W's Repeated Word
(7)	
a. October	/aktE/ # /akter/ # /aktiber/
b. November	number
c. February	/feber/

In view of the fact that most of W's responses were correct and it is easy to see in the above examples that a good number of the syllables of the correct word are retained, there can be little doubt that the

semantic features are intact and only the phonological features of the lexical item are impaired. On the other hand, since some of the responses were correct, and as will be seen below, often only specific word classes are disrupted this way, it is hard to imagine how the phonological rules could be impaired for one word and not for another, unless one wished to add a lot of special functions and mechanisms to the central language system.

A disruption of the neuromuscular specification of a lexical representation can apparently be confined to particular classes of formatives, at least it is possible to argue that in some instances only nouns and perhaps even a subset of them are affected. This syndrome is referred to in the literature as anomia and could be characterized as the loss of the lexical representations of nouns, particularly [+proper] nouns. Consider the following interview; it will be noticed that the lexical representations are disrupted or inaccessible in both spontaneous speech as well as in repetition speech but in at least one instance can be realized by access through the visual modality. The difference between a disruption and a block (i.e. being unable to retrieve the lexical representation will be discussed below again; it has been discussed above in the remarks on Weigl and Bierwisch's [1968] paper. The interview is with patient LBVAH-E.J.

(8)

CH Studebaker! Say it.  
 E.J. /jægwɪn/ # /čikwər/ # /čikitwər/  
 CH Studebaker  
 E.J. stick it, sticker, sticker  
 CH Studebaker  
 E.J. sticker-maker  
 CH Studebaker  
 E.J. studemaker! studemaker! [See footnote #1]  
 E.J. Your name, I trying to remember yours so damn long I don't  
 get it which is real bad for you, uh, Jack damnit, no it's  
 not Jack, Jack.  
 CH Clark  
 E.J. Jack Clarker  
 CH Clark  
 E.J. /klar/  
 CH Clark  
 E.J. Clark! Clark Clark Clark, Clark with 'K' on the end of it, yea, Clark.  
 CH Harada [Note: stress on 2nd syllable]  
 E.J. Clark /jagə/  
 CH Harada  
 E.J. /garadə/  
 CH Harada  
 E.J. /gard/ # /gard/  
 CH /h:/  
 E.J. /gard/

CH /ha:/  
 E.J. /hard/  
 CH Harada  
 E.J. /harar/ # /harəd/  
 CH Harada  
 E.J. /'jægər/ # /harharəd/ aw, I got it written down some place  
 right here, find the damn right see [reading from paper]:  
 Clark Harada [but placed stress on 1st syllable]

In view of E.J.'s ability to recover the interviewer's name by reading it, which as noted above is probably a deficit in tracking the P-rep with the neural-muscular commands, it is interesting to contrast a patient whose access to the P-rep was disrupted in the visual modality as well as the auditory. In an interview conducted by V. Fromkin and myself, R.H. was asked to read from a primer; the stimulus sentence was on the left page and a picture representing some aspect of the sentence was on the right side. The sentence in question was *Davy wakes up early* and R.H. was asked to read it aloud:

(9)

VF Can you read his name?  
 R.H. /pæpə/ # /perə/ # but it's right here and that's early.  
 VF And what did he do early?  
 R.H. To go to bed, early in the morning, very early in the morning  
 to go to sleep.

On another sentence, *He combs and brushes his hair*, the word *hair* was isolated for R.H. and he was asked to read it alone aloud.

(10)

R.H. /hɪzəs/ #... /hæd/ ...  
 VF What is it that you have that's red? What is this? [pointing  
 to hair]  
 R.H. Well, your head, your head right here  
 VF Hair?  
 R.H. Your /hæd/ or your head  
 VF Hair!!  
 R.H. Your /hɪ/ right, your /hɪ/, I meant, I can't put it out,  
 it's /hɪ/

R.H.'s language deficits obviously include much more than a disrupted lexical representation, but for now let us just consider that. In the former case of E.J. the small bit of evidence that a written word could be associated with its lexical representation but the spoken ones could not (recall that he did speak the interviewer's name when reading it but could not otherwise) indicates that the lexical representation itself was probably intact but dissociated from the peripheral auditory language modality. This is comparable to much of the data presented by Weigl and Bierwisch [1968] and one would want to argue that the visual stimuli

deblocked the auditory modality. Quite properly this should be regarded as a performance deficit rather than a competence impairment. By contrast, R.H.'s disruption of the lexical representation is a central one. Not only was he unable to locate the lexical representation through the visual modality, he also could not locate it through the auditory modality, as seen in the example of some repetition tasks:

	Spoken Word	R.H.'s Response (spoken)
(11)		
a.	red	red
b.	Vicki	/laefitE/ # /laefitE/ # /tE/
c.	Pittsburgh	Pittsburgh
d.	Purdue	Purdue
e.	Fort Wayne	/perfaen/
f.	Glasses	/kəlaes/ # /kəlaesən/ # /gəlaes/ glasses, to glean

Three of his correct responses were cited (in this case the majority of responses in both the reading and repetition tasks, as well as in spontaneous speech, were incorrect) both to demonstrate that an impairment does not need to affect *all* instances of the level in question in order to substantiate the reality of the level and to demonstrate that certain words and phrases become more stable in some sense than others due to non-linguistic associations. *Red* is R.H.'s nickname, *Pittsburgh* is his birthplace and *Purdue* is the university he attended.

What is important is that all instances of the disruption are correlated with the level in question, which is the case in the data cited above. Notice that the performance (modality-bound) deficit of E.J. still provides concrete evidence for the existence of a level of lexical representation in that it is precisely such a level that is blocked for one modality and accessible to another. This was noted by Weigl and Bierwisch [1968], too, as noted in Chapter III. For comparison to the above, consider the following part of an interview with a typical Broca's aphasic; his speech is non-fluent and there is little evidence of apractic distortions of words. L.P.'s auditory comprehension was much better preserved than his ability to speak, making him very much aware of his deficit.<sup>2</sup>

(2)

- L.P. You know, I know but I can't seem to communicate. This just burns me something terrible, the brain, the brain.....I know, I know it. But I can't seem to get going. Now this is the problem. I. You know all of these facets but you can't seem to. It's an amazing thing.
- HW You can't pull the bits and pieces, or do you have--
- L.P. brain over brawn
- HW Well, how would you describe it?
- L.P. All of these things take time, all of these things take time. That's about the extent of it. You, you.
- HW You remember you were telling me about your ship?
- L.P. Well, that's, that's.

One of the interesting things about L.P.'s speech is his ability to utter common phrases or cliches in a fluent and perfectly normal manner; this, of course, includes swear words. Such phrase units are called ictal speech automatisms and must be considered separately from the central language system, regardless of how 'normal' they appear to be, because there is convincing evidence that such phrases are separately stored as whole units in the brain and can be retained in spite of a nearly complete dissolution of the verbal modality. For a detailed neurological discussion of ictal speech automatisms and some theoretical arguments proposing that they are an "open-loop" speech mode in contrast to normal language, see Chase et alia [1967]. Speech automatisms are not idioms such as "kick the bucket" but are phrases frequently used in day-to-day conversation such as "How are you?" and the like. It follows that a repertoire of automatisms is largely idiosyncratic, depending on an individual's own language habits. They are more of interest to the neurological model of language than to a linguistic model because they vary from speaker to speaker and because they do not reveal underlying units and rules. From the viewpoint of testing the linguistic model against aphasic data they are important only as a class of exceptions from which we cannot infer loss or retention of aspects of the grammar. For example, when L.P. was first admitted to LBVAH, approximately 1 1/2 years before my interview with him, he used the following phrases in an interview with the clinician:

(13)

- |                                       |   |
|---------------------------------------|---|
| a. pretty fine                        | h. I'm sorry                                |
| b. I'm getting it                     | i. Whatever comes along I go get it         |
| c. gradually                          | j. it just depends on whether I do or don't |
| d. I was born in #                    | k. I got a bum deal                         |
| e. by God                             | l. I don't know what the guy's #            |
| f. It's been about six or seven weeks |   |
| g. you can get 'em across the thing   |   |

Aside from these, many of which were repeated several times, L.P. was virtually unable to express himself. The following phrases, excerpted from my interview with him again are the only aspects of expressive speech which are normal and fluent:

(14)

- |                                     |   |
|-------------------------------------|---|
| a. God love it                      | h. my God   |
| b. it's a long time ago             | i. my whole life I've been brought up<br>to get going |
| c. mind over matter                 |   |
| d. give or take a #                 | j. brain over brawn                                   |
| e. it just burns me up the yingyang | k. more or less                                       |
| f. can't seem to connect it         | l. we'll see what we see                              |
| g. oh, it wouldn't make a #         | m. What seems to be my problem here?                  |

L.P. cannot carry on a conversation or do the most elementary language tasks such as identifying simple familiar objects, even though he does seem to understand a few things said to him, although only in a very rudimentary fashion like a particular word or idea. A more extreme but by no means a typical case of non-fluent aphasia with retention of ictal speech automatisms is L.S. He responded to nearly every question or attempt at conversation on my part with the utterance:

(15) What'cha gonna do right now? yea yea!

This was said with perfectly normal, albeit excited, articulation and intonation.

In view of these considerations it is exceedingly difficult to argue one way or another about central language processes in terms of ictal speech automatisms. For example, on the basis of one utterance, it would be rather far-fetched to assume that L.S. had retained the WH-question transformation and correct pronominal references (for the 2nd person singular personal pronoun), regardless of the fact that this one utterance would be so analyzed linguistically. It is only in cases where aphasic language behavior is not of the ictal speech variety that one can use it as data bearing on linguistic hypotheses and further, it is only when such data is obtained in more than one modality that it can be considered to bear on the central language system.

## 2. Separation of Syntax and Semantics

So far we have indicated that the following aspects of the grammar have a functional neurological validity: (a) a level of neural signals which serve as the input/output interface between the central and four peripheral language systems, (b) a general level of phonological representation of utterances with the important sub-level or component, (c) the lexical representations of words. A fuller discussion of the lexicon will be presented in the next section.

The question of interest then, is whether the remainder of the central language system -- the semantic and syntactic aspects -- are also separable entities or a more or less unified component. If the latter is the case, we would not expect to find brain damage resulting in, e.g. syntactic disruption while the lexical and semantic aspects of language remained intact. An utterance does not have separate syntactic and semantic units comparable to separate words, of course; these distinctions are inferred from the meaning of the utterance and from the grammaticality of the utterance. Furthermore, an utterance in one sense does not have words *and* semantic-syntactic relations, since without the sounds that we identify as words there is no utterance to begin with. These very obvious and simply-minded facts are mentioned only to remind one that the data is not physically quantifiable in the sense of, say, a biochemical assay or the firing rate of a neuron.

What one is looking for is systematic deviance in aphasic speech that indicates a loss or impairment of a semantic, syntactic, lexical or phonological nature. As we have seen above, evidence for a predominantly phonological deficit is reasonably straightforward. Before turning to the more complicated syntactic and semantic data, a brief recapitulation of the kind of evidence for which we are looking is helpful. Syntax and semantics have a familiar and intuitive distinction which, however, is often difficult to state with any precision. Transformational grammar (in the earlier versions at least) formalized the distinction in a clear manner. Chomsky [1965](pp. 75-77 and pp. 148-153 ff) noted that the difference between strict subcategorization features and selectional features corresponds to a natural distinction in language use that is related to the boundary between syntax and semantics. In particular, strings which violate strict subcategorization features such as transitivity or complement structure, as in (examples from Chomsky [1965]):

[1. (i)] John found \*sad

[1. (v)] John persuaded great authority to Bill

and strings which violate selectional features, such as animacy or concreteness, as

[2. (iii)] The boy may frighten sincerity

[2. (v)] they perform their leisure with diligence

deviate from normal sentences of English in different ways. Those of (2) can often be interpreted metaphorically by analogy to sentences that do not violate the selectional features in question but those of (1) cannot and are therefore in some sense more ungrammatical. The question which Chomsky raised -- whether selectional features were part of the syntactic or semantic components -- is no longer that simple an issue in linguistics; selectional features such as [animate], [concrete], etc. are clearly semantic in origin regardless of whether syntactic rules (such as relativization for example) depend upon their correct specification. The current issue in transformational theory is whether semantic and syntactic aspects of the grammar should be distinguished by separate components or not as was noted above in Chapter III. There are a number of theoretical points on this question which will not be examined here; instead, the question will be considered from the perspective of Chomsky's original distinction between syntactic deviations as in [1] and semantic deviations as in [2]. If the evidence from aphasia indicates that semantic and syntactic aspects of language are separate then any linguistic theory must specify a grammar that accounts for the facts; how the facts are actually represented in the grammar is not determinable from such data although as before, the simpler representation would seem to be preferable. The following phrases were excerpted from an interview with LBVAH-W.H. Since normal conversation is rarely a succession of



grammatically complete sentences, the "clause" nature of these utterances is to be expected. On the other hand, normal conversation does maintain a great degree of semantic and syntactic integrity within clauses. W.H.'s utterances are not generally standard in this regard, although on some occasions they are semantically and syntactically normal (see entire interview which is reproduced as Appendix C). It is interesting to observe that syntactic irregularities occur less frequently than semantic ones in this conversation. They can be distinguished in that the semantically deviant utterances maintain correct syntactic categorization comparable to Chomsky's examples (2) above. When syntactic categorization is violated, it is of course impossible to ascertain whether the semantic representations are right or wrong, as in examples (1) above. We can assume that W.H. on occasion had access to correct semantic representations as seen in the context of the entire interview. It is also interesting to observe once again that deficits in aphasia are not "complete" over the whole range of one particular grammatical phenomenon; semantic violations can occur without syntactic violations, as just noted syntactic violations are strikingly different even though it is an open question whether the semantic representation is correct, and sometimes there are no deviancies. In addition to these deficits, W.H. also shows apraxia of speech deficits which are indicated by the phonetic transcriptions between slanted lines; for convenience in separating the semantic and syntactic violations, the latter are italicized and considered first. In this interview, the speech therapist was showing W.H. a series of color photographs of famous people; the person whose picture W.H. is looking at and answering questions about is indicated before each utterance. Following each example is a brief and tentative comment on the deficit illustrated; in most cases a putative "correct" version of the example is given in order to better illustrate the deficit and not to argue that W.H. actually had such a version in mind.

[W.H. was shown a picture of General Douglas MacArthur]  
 LBVAH-W.H. (16) ... and his *appeared up* with all medals for returning every receded, and his name is General /mae/, his next one what's his called there is,

In (16) the incorrect particle *up* following *appeared* could have been, for example, the adverb *there*; we might assume that *his* is the incorrect phonetic specification of *he*, or that an appropriate noun was deleted. Since W.H. had just remarked on the fact of MacArthur's being ordered out of the Philippines, it is plausible that the syntactically incorrect phrase *every receded* could have been a time adverbial such as *then*, or *every time*. The participle *receded* of course cannot be modified by *every*.

[W.H. was shown a picture of Harry S. Truman]  
 LBVAH-W.H. (17) ... and till *in the as a poison plesident*, now he is a /fərsən/ should be,

In (17) the underlined words begin as a prepositional phrase and finish as a comparative construction, including the semantically anomalous noun-noun structure *poison plesident* (the /l/ instead of /r/ is

probably due to apraxia); noun-noun units do occur in English -- e.g. house carpenter -- but it is possible that this is a syntactic error, too, in that *poison* could have been an adjective. The clause between the commas is syntactically deviant; assuming that /fərsən/ should have been *person*, it would either be the predicate nominal of *is* or the subject of *should be*, but not both. As indicated by commas, there was no pause within the last part of the utterance.

[W.H. was shown a picture of John F. Kennedy]

LBVAH-W.H. (18) ... after the can these into the present mister, oh what's his name now I've forgotten,

(19) ... to fill his a fishing ...

(20) Well, he was killed by a /ə/ accident in a almost about lung, down into,

In (18) it is not possible to decide whether *can* should have been the noun (a semantic error in that the noun would have to be specified as a place or time following the preposition *after*) or as the modal verb (a syntactic category error, of course). In (19) *a fishing* is not a syntactic phrase -- it could have been an NP associated with the possessive *his*; it is difficult to associate it in any form with the preceding verb. In (20) note that the only hesitation pause is after *lung*. It is possible that *down into* was a partial utterance common in conversational speech; however, *in a almost about lung*, if intended as an allocative adverbial expression, as it seems, is syntactically deviant. It is interesting to note that it can almost be corrected by re-arranging the order: *almost about in a lung*.

[W.H. was shown a picture of Fidel Castro]

LBVAH-W.H. (21) ... to see joy one oh seven, and that was a fond of day of theirs.

(22) Oh, it, he's got some in /kə/ Cuba and he's so badly in bomb just what he stands that there are long in Paris.

In (21) the collocation of the article *a* and the adjective *fond*: (of) is wrong and the adjective could perhaps be either a noun or an expletive. Notice also that *day* should have an article in front of it, assuming that *day* was actually the noun intended, or a mass noun of some sort could have been used like *time*. In (22) the adverb *long* is categorially incorrect; it could have been an adjective such as *many*. *Bomb* in (22) is the same syntactic mistake as *day* in (21).

[W.H. was asked the question: "Your wife is a former teacher, is that right?"]

LBVAH-W.H. (23) I'd have to look that peg to get it, but as of right now she's twenty-eight.

In (23) the word *peg*, presumably a noun but possibly the verb, could have been the particle *up*. *About twenty-eight* is semantically anomalous in this context, although it is conceivable that W.H. was referring to her age rather than occupation.

The above examples do have some semantic violations as noted; however, generally there is a word or phrase indicating what W.H. was trying to say, thus, he noted the fact of MacArthur's career in the Philippines and his return -- actually, W.H. served in the Army under MacArthur as he stated elsewhere (see full interview in the Appendix), he noted that Truman was a former president -- in fact, *poison* might have been intended for *former* although there is hardly any phonetic similarity, he noted the assassination of Kennedy and finally the fact that Castro is in Cuba. It is quite reasonable to assume that W.H.'s cognitive system is largely intact and that some aspects of the semantic system are operating, but that in these examples, there is a relatively severe impairment of basic syntactic relations within phrases and sentences. In contrast, the following examples from the same interview with W.H. illustrate predominantly semantic deviations.

[W.H. was asked: "Can you tell us your name?"]

LBVAH-W.H. (24) That's my only phone, and my mother's /AdEwI/, and then the bath, that's where the three vegetables are wreathed, no calendar there /kaetaek/ you know what I mean?

Most of the semantic anomalies in (24) are patently obvious, but the sentential nominal *where the three vegetables are wreathed* requires some comment. Presumably this noun phrase would be derived (ignoring details) from a sentence *someone wreathed the three vegetables (WH-)there*. Notice that the verb *wreathe* is not transitive and thus the original (putative) sentence is syntactically wrong unless of course some other verb was intended; but, the passivization and movement to the front of the locative adverb *where*, are correct for placing the sentence in the predicate nominal position following *that is*. [Note: The full details of the derivation of this sentence are ignored simply because I know of no way at present to relate the requisite operations to this data. Some comments on rules will be made later in the chapter.]

[W.H. was shown a picture of Jimmy Durante]

LBVAH-W.H. (25) And this is a predator ... very /təces/, /kəntəčəd/ boy and an electric soul

(26) Well they /trɔiv/ and very /nɔtiv/ and makes good loyal buildings stink or /rɛrɛrdz/ anyway you want to look at it but he is a good /krugər/.

In (25) it will be noted that *predator* is not ordinarily marked [+human] in English (except possibly in the mass sense, as 'The Romans were predators') and of course the NP *electric soul* violates the

selectional feature [+animate] as is undoubtedly known by the contemporary singing group of that name.

[W.H. was shown a picture of General Douglas MacArthur]

LBVAH-W.H. (27) half of the olive and the navy and all other people,  
you either do it the way he says or they don't pay,

(28) ... why his, his /s|E/ wouldn't, overalls wouldn't look  
like it overly paid a little bit too much, you know?  
Let's go down and, uh, they'll say they never ...

In (27) it appears as though *olive* should have been *army* (at least the quality of the first vowels is alike) which is an error of virtually all semantic features except [concrete]. In (28) one would first reconstruct the utterance by allowing for the false start /s|E/ to read: *why his overalls wouldn't look like ... etc.* in which case the pronoun *it* violates the [mass/count] distinction referring back to *overalls* and the verb *paid* does not match the subject *his overalls* in that the selectional feature [+human] is violated.

The discussion of these examples is, obviously, quite simplified as was that for the syntactic examples because the importance of this data is only to indicate that semantic and syntactic aspects of language may be disrupted in a manner parallel to Chomsky's original insight into how they may be distinguished. In examples (24-28), except for the transitivity of *wreathe*, there are no syntactic category mistakes such as using a noun where a particle should be as in (23), in examples (16-23) the errors are primarily syntactic, although there are semantic deviancies too in addition to the fact that a syntactic category error perforce results in anomalous semantic specification. A consideration of the whole interview with W.H. also confirms the fact that he appears to have access to a phonetically complete lexicon (this assumption deliberately minimizes the apractic symptoms which seems justified in view of their relative infrequency) of nouns, verbs, adjectives, etc. as well as grammatical formatives, but that semantic and syntactic aspects of deep structure are on many occasions quite confused. Before considering some data which is much more specific and detailed than the above, the claims being made as to what the data demonstrates, should be made quite clear again.

So far the following facts about the cited aphasic language data have been established: the phonetic representation of a lexical item may be disrupted when the semantic and syntactic representation is intact, the syntactic categorization of lexical items may be incorrect in terms of what is required by the phrasal context when there is no phonetic disruption and unknown semantic disruption, and finally the semantic representation of lexical items may be incorrect with respect to selectional features when there is no syntactic deviance and no phonetic disruption. That I have chosen to discuss this data in terms of transformational grammar is not to be construed as empirical verification for such a model, or more particularly, for any of the several

proposals within transformational theory. On the other hand, the transformational model does provide a relatively straight-forward way of accounting for this data, which is certainly important. The data does provide evidence against a linguistic theory which fails to distinguish semantic and syntactic aspects of language (although I know of no such theory in the literal sense) and more significantly argues against a theory which considers semantic and syntactic aspects of language to be inseparable parts of one component of the grammar (the generative semantics proposal) for the following reason. If brain damage can differentially affect semantic and syntactic features and rules, and more evidence in support of this will be discussed shortly, then we must recognize that in aphasia these aspects of language are functionally independent or autonomous. If it is proposed that normally they are indistinguishable but as a result of brain damage they become separate, we are then put in the curious and untenable position of asserting that brain lesions create or add brain functions not found in the intact human brain.

One of the more interesting tests in terms of the type of data obtained was #8. The test procedure was as follows: a stimulus word from the list was presented to the patient, both verbally and printed on test sheets in most instances except as noted above with F.Z. and J.W.; the patient was asked to define the word and/or use it in a sentence, in most cases both a verbal and written response was solicited. Consider first some of the strategies and abilities that might be involved in this task; the patient would have to have knowledge of both the possible structure and possible content of the entity, *sentence*; this might specifically take the form of the following:

(a) knowledge and ability to use the major grammatical units of the sentence -- the syntactic phrases -- so that he could pick a context for the stimulus word and a context for the phrase in which that word was to be placed, which implies that he has syntactic category information of the stimulus word as well as any other words he might use, in order to do anything with (a),

(b) ability to determine the semantic features of the stimulus word and be able to determine the semantic representation of other words in order to construct a meaningful and appropriate phrase and sentence,

(c) and have a correct phonological representation to correlate with an intact phonetic comprehension and production (or graphemic), in order to successfully do the task. It is arguable that the patient would *not* need to have knowledge or ability to use some of the major transformational rules like Negation, Relativization, Conjunction, etc., in order to do this task, although he *would*

(d) have to have knowledge and be able to use many of the obligatory rules like number agreement between the surface subject and verb. This last is an important point since we can assume that a patient with

brain damage that affects the language system will not find *linguistic* tasks of any type particularly easy and it would therefore be reasonable to expect him to avoid or disregard inessential grammatical operations, in the task of constructing a sentence from a given stimulus word. In fact this seems to be the case and this of course makes the responses from F.Z. and J.W. above on this task even more interesting. Since (c) above pertains to the apractics and dysarthrics which have already been discussed, we can concentrate on the other strategies and abilities.<sup>3</sup> There are then four putative linguistic factors (excluding the phonological ones) involved in the word-to-sentence task, which can be re-stated in terms of the model of the grammar, linguistic competence, as follows:

(1) Information about which collocations of phrases (NP, VP, etc.) constitute sentences (#S#); this may be both semantic and syntactic information.

(2) Syntactic information of the constituents of the phrases (Det, N, V, Prep, etc.)

(3) Semantic information of the lexical features which determine meaning and selectional restrictions between the phrases of (1)

(4) Syntactic information of obligatory grammatical rules, e.g. T-agreement.

If these four assumptions are correct, i.e. if these are in fact separate parts to the central language system employed in the word-to-sentence task, then it should be possible to predict the consequences for speech behavior if any one of them has been disrupted by brain damage. Consider first what would happen if necessary semantic information were lost. We would expect semantically anomalous sentences to be produced in response to the test word; in particular, the selectional restrictions between syntactic phrases would not be adhered to even though the syntactic relations would be maintained. The following data seems to demonstrate just such a loss:

Stimulus Word	W.L.'s Response
(29)	
a. under	*there is under a horse a new side saddle
b. behind	*it is behind the end
c. next	*next to me is a new return
d. out of	*out of the end is the middle
e. in	*in girls we see many happy days

It will be noted that in responses (29) the syntactic constraints on the prepositions are acceptable but W.L. apparently does not know their meaning. To support this idea he was given another task in which he was asked to tell me whether an object, a lighter, was *behind*, *in front of*, *under*, *on top of*, etc., other objects such as an ashtray or a

table. It is interesting that W.L. always used a locative preposition in trying to give me the answer but he almost invariably used the wrong one; this indicates minimally that he has in fact lost the semantic features of the locatives but apparently not the grammatical category feature [preposition], or the major semantic feature [+locative]. Next consider what the consequences would be if syntactic category information of the stimulus word were lost. In particular, if the stimulus word was a verb such as *force* which requires a complement sentence and that syntactic knowledge was unavailable, we would predict that an error would be made like the following:

Stimulus Word	W.L.'s Response
(30) force	*you forced a better foot

Or, again, if a verb which required that the object be marked [animate] and that semantic feature was lost we would get responses as:

Stimulus Word	W.L.'s Response
(31)	
a. challenge	*I'll challenge a new bike
b. grow	*grow a new method of eating
c. surprise	*I surprise no new glamour

The semantic feature [number], specified as to [singular] or [plural] syntactically, (or as 1st, 2nd and 3rd person with respect to pronouns and verb affixes), if lost would have predictable effects on the low-level syntactic rule for agreement. This loss is demonstrated in many of the responses of another patient, K.T. In this case, the test itself is irrelevant since the only deviancy is number agreement. Consider these sentences of K.T.'s:

- (32)
- well, like door-to-door salesmans...
  - Is there three chairs in this room?
  - Is cigarettes bad for you?
  - Is cigarettes smoking hazardous for your health?

It is interesting to note, and also is further confirmation of the loss of the feature [number], that K.T. cannot use the agreement rule properly after conjunction. In this case the plurality is derived from the coordinate noun phrases, but is still unavailable to him for correct verb agreement:

- (33)
- multiplication and division is simple # is easy
  - Mike and Peter is happy
  - Bill and John likes hamburgers, Bill and John likes hotdogs
  - The pencil and the pen is, would be, in the pocket
  - Green and black is my best colors

It should be pointed out that K.T. did not use the singular verb form invariably. That is, his deficit is not merely a matter of having only singular verb forms at his disposal. A number of his utterances used plural verbs correctly, as in examples (34), and of course a number of them used singular verbs correctly, too. The impression one gets is that singular and plural forms are more or less randomly selected. [Note: As an aside, it should be mentioned that K.T. was able to count normally and could add and subtract with little difficulty; no other arithmetic skills were tested.]

(34)

- a. John and Mike are doctors.
- b. Peter and Barbara do not like each other.

It was suggested above that the task of constructing an appropriate sentence for a given stimulus word probably did not involve the major transformational rules in syntax with which sentence complexity and variety is achieved -- complementation, negation, coordination, question, relativization<sup>4</sup> and the like -- because a brain-injured patient would find such tasks hard to do and therefore seek the simplest means for doing them. In other words, we would expect that an aphasic would employ a very limited number of syntactic frames in responding to this test, assuming that the requisite knowledge of syntactic deep structure were still intact. This does seem to be the case as is suggested by the data noted in this chapter. In fact, characteristically the patient responded to test #8 by using the stimulus word first, or near the beginning of the utterance. For nouns this was generally an easy task. For verbs, however, problems arose as would be expected. Often an imperative form was used (as in (35) below) or an infinitive form as a subject nominal and on many occasions the verb was nominalized, possibly in order to use it at the beginning of the response more easily. A better explanation of this phenomenon is probably that nouns were handled more easily by the patients than verbs; this problem is taken up in more detail in section B below. The lack of a wide variety of sentence structures in test #8 does not necessarily mean that such transformational rules are lost. Consider patient K.T., for example; he characteristically responded to verb test words (*when* he analyzed them as V; he often analyzed them as N, an interesting problem taken up below in the section on derivational morphology) with a simply subject-predicate sentence or an imperative sentence structure:

Test Word	K.T.'s Response
(35)	
a. skate	I skate back and forth in the snow
b. throw	Throw the ball, please.

Less frequently he employed a complement structure:

(36) expect	I expect I'm going home
-------------	-------------------------



And on the complement-making test, he often made mistakes such as failing to alter the complement verb to conform to the complementizer:

Test Strings	K.T.'s Response Sentence
(37)	
i. Michael wanted _____	
ii. John bought the car.	Michael wanted John to bought the car

In other tests K.T. was able to convert declaratives into Yes-no Questions, to negate affirmative sentences, to make a coordinate sentence from two conjunct sentences (with many errors of [Number] however), and to do pronoun replacement (with many feature errors discussed below), to name some of the syntactic skills tested. These are discussed again in section D.

The data discussed so far provides good evidence that phonological representations, semantic representations and syntactic categorization are functionally distinct aspects of the language system. Of these three aspects, only the syntactic categorization data is directly involved with the base rules of the grammar as opposed to features of lexical items which are more related to the data on semantics and phonology. It should be clear then that this data bears on semantic amalgamation rules and major syntactic transformations in only an indirect manner, even though these rules surely must depend upon the features in question. These problems are taken up in more detail in section D below.

The last evidence to be considered on the matter of the separation of syntax and semantics even more clearly pertains to lexical items. Consider the type of knowledge required to successfully complete the pronoun substitution test (in Appendix B, No. 2). Both semantic information -- e.g. the features of [Person, Number, Gender] -- and syntactic information -- e.g. the grammatical category feature [Pronoun] and [case] agreement -- are required to make correct substitutions. Considering this, it is interesting to note the errors made by K.T. on this test, which involved replacing underlined NP's with the appropriate pronouns in a short narrative passage. K.T. was unable to do this task successfully as shown by both his spoken and written responses:

[The identification numbers were not part of the original test.]

John and Susan are married. When John came home, *John*(1) asked Susan if Susan(2) wanted to take a trip. Susan(3) said 'yes'. So *John and Susan*(4) went to San Francisco. It was raining when *John and Susan*(5) arrived. John did not bring *John's*(6) raincoat; *John*(7) had left the *raincoat*(8) at home. However, Susan remembered to bring *Susan's*(9). While *John and Susan*(10) were in San Francisco, *John and Susan*(11) met Paul. Paul was an old friend who knew *John and Susan*(12) back in high school. *John, Susan, and Paul*(13) all enjoyed the vacation.

(38)

	Correct Pronoun Response	K.T.'s Spoken Response	Feature Error	K.T.'s Written Response	Feature Error
(1)	he	I	[Prs]	I	[Prs]
(2)	she	she	[Prs, Gen]	her	[Prs, Cs]
(3)	she	she	-	her	[Cs]
(4)	they	we	[Prs]	I	[Prs, Num]
(5)	they	we	[Prs]	I	[Prs, Num]
(6)	his	our	[Prs, Num]	I	[Prs, Cs]
(7)	he	---	---	---	---
(8)	it	I	[Prs, Gen, Cs]	I	[Prs, Gen, Cs]
(9)	hers	him	[Gen, Cs]	her	[Cs]
(10)	they	we	[Prs]	I	[Prs, Num]
(11)	they	we	[Prs]	he	[Num]
(12)	them	---	---	---	---
(13)	they	we	[Prs]	I	[Prs, Num]

[K.T. attempted (7) and (12) but finally gave up and said, "I don't know."]  
 Note that even in a test with thirteen items, K.T. has managed to make errors in all four semantic features of the personal pronouns. Not surprisingly, he makes similar errors with pronouns in conversational speech and on many of the other tests which were given to him; most noticeable in conversational speech are errors of [Gender]. It was observed above that K.T. had difficulty with the late syntactic rule for [number] agreement; from the responses in (38) it is obvious that he also has difficulty with the late syntactic rule for [case] agreement. This is suggestive of a general impairment that involves comparing phrases in the same sentence, however this was not explored in any detail.

In a test to convert declarative sentences into questions, the pronoun confusion of K.T. is quite apparent:

(39)

HW I have been to Germany  
 K.T. Have you been to Germany?

Since it is possible that my declarative was taken as a matter of fact rather than as a purely linguistic exercise, the personal pronoun was changed as in the next example; obviously it made no difference.

(40)

HW You have been to Germany.  
 K.T. Have you been there to Germany?

In fact it appears that for questions involving personal pronouns, K.T. only uses *you* in his response; on one occasion he even used it when *John* was the subject of the declarative (note also the switch in tense):

(41)

HW            John hit the ball.  
K.T.         Are you hitting the ball?

With ordinary inanimate-object nouns this confusion did not appear, of course:

(42)

HW            This is a tape recorder.  
K.T.         Is this a tape recorder?

The remainder of this test is of some additional interest for it appears that K.T. was more or less aware of his deficit even though he could not correct it.

(43)

HW            We are happy.  
K.T.         Are you happy? Well, that's not actually right.  
HW            O.K. tell me the right one.  
K.T.         Are you happy? No, that's still not right. I am happy.  
              I am happy, you are happy, everyone is happy.  
HW            We are happy.  
K.T.         You are happy. Are you happy?  
HW            They are happy. (no response) They are happy.  
K.T.         We are happy. We are happy. They are happy. Are you happy?  
              It's still not exactly right.  
HW            Do you know what's wrong with it?  
K.T.         Yea it's a, there should be one more letter, whether it's a three letter word or a four letter word or, it's, it's just one letter; is it one letter? or what's the word for it?  
HW            Well, it's one of the little words, yea -- the pronoun.  
K.T.         The pronoun, yea that's what I wanted to say.

It is reasonably clear that the features [person, number, gender] as specifications of [animate, human] noun phrases are quite disrupted in K.T.'s language system; on the other hand, he clearly does identify [animate, human] nouns, particularly the subset of proper names, and even more importantly, identifies the grammatical category [pronoun]. This is one of the clearest examples of the separate disruption of semantic and syntactic features in this research.

To summarize the evidence and conclusions presented in this section, it has been argued that phonological, syntactic and semantic aspects of the language system are distinct and in particular it is most plausible

to consider lexical items (the major lexical formatives -- nouns, verbs, adjectives and adverbs) as being comprised of three sets of features which reflect these differences. We may further speculate that the major division in the lexicon is between the phonological representation and the semantic/syntactic representations, that there is a further split between the latter two and that the three representations or sets of features do not have equal status. This does not seem too surprising if we remember that the semantic and syntactic features of the lexicon are absolutely required for all four language modalities -- visual, auditory, tactile and verbal -- whereas the phonological representation might only be needed for the verbal and auditory systems with a perhaps derivative graphemic representation employed by the tactile and visual systems. The phenomenon of knowing what the word is but being unable to say it, common in the syndrome of anomia<sup>5</sup> is further support for the hypothesis that the phonological representation is neurologically distinct from the others. A corollary of this hypothesis is that the semantic and syntactic representations subserve an "integrative" function of the central language system whereas the phonological representation subserves the input and output functions and are thus more related to the peripheral language systems. The *possible* neuroanatomical substrates of these, suggested by the discussion in Chapter II of the effects of lesions in cortical structures, is that the phonological representations are properties of Broca's Area and Heschl's gyrus, their derivative graphemic representations are properties of Exner's center and some part of the visual cortex; the syntactic and semantic representations would be properties of the other language areas in the cortex. The notion "properties of" is to be taken only in the most general sense, of course.

## B. The Lexicon

The lexicon or dictionary is traditionally considered in linguistics to be the repository for idiosyncratic facts about a particular language; Chomsky [1965](p. 87) notes that "in general all properties of a formative that are essentially idiosyncratic will be specified in the lexicon", and he relates this view (p. 214, note 16) to Bloomfield's [1933] very similar notion, etc. In the standard theory a lexical entry specifies three classes of information -- semantic, phonological and syntactic properties -- which are represented by a set of features for each property. The lexicon and its structural organization are clearly of central importance in linguistic theory and as might be expected there are many controversies about it (cf. Lees [1960], Katz and Postal [1964], Chomsky [1965], Lakoff [1965], Gruber [1967], Chomsky [1968], McCawley [1968], Chomsky [1969].) An area of linguistic investigation as broad and problematic as this cannot be completely evaluated here and I shall limit my discussion to two problems. The first bears on the controversy known as the "lexicalist" vs. "Transformationalist" positions (Chomsky [1968]). One of the distinctions between these positions which will be considered is the treatment of nominalization. In the transformationalist approach, all nominalizations are accomplished by a general

rule applying to verbs or verb phrases. Thus, related to the VP in (i)

(i) John presented the data.

is the gerundive nominal (ii)

(ii) John's presenting the data...

and the derived nominal (iii)

(iii) John's presentation of the data...

The transformationalist position would mark VP that do not form derived nominals with an exception feature; thus, there is no derived nominal associated with (example is from Chomsky [1968], p. 5):

(iv) John is easy to please.

(v) \*John's easiness to please...

In addition, the transformationalist position posits certain underlying verbs which never occur in surface structure and which are marked with a feature that requires them to become derived nominals by rule. For example, to establish the generality of the relation between agentive nominals and their associated verbs (the examples are from Lakoff [1965] Chapter V, particularly pp. V20-V22):

(vi) John is a painter.

(vii) John paints.

the transformationalist posits hypothetical verbs for "agentives" which are (to use Lees' [1960] term) unanalyzable, as in:

(viii) John is a plumber.

(ix) \*John plumbs.

The lexicalist position distinguishes gerundive nominalizations, which are quite general and thus adequately characterized by a transformational rule, from derived nominals, which are less productive, have idiosyncratic semantic relations between the nominal and associated VP and which have the internal structure of noun phrases, unlike the gerundives (for discussion see Chomsky [1968]). The lexicalist position represents the facts of derived nominals as separate lists of features under the same lexical item heading in the dictionary; thus /receive/ has both [N] and [V] features and a later morphological rule spells out the [N] form as /reception/. Where a corresponding verb does not exist, it simply is not listed; e.g. under the [N] /carpenter/ there is no [V] \*/carpent/.

In many respects this issue is theoretical. Obviously, if a certain class of facts is accounted for in either the lexicalist or transformationalist model, such facts cannot alone be evidence for one or the other model. The data which will be presented is more psychological in nature, having to do with putative strategies and the ease of handling some linguistic tasks on the part of aphasic patients, and *inferentially* supports the lexicalist position. What will be claimed is that the lexicalist position provides a model that is *simpler* to relate to this data; that is, the data requires one to postulate fewer neurological/psychological mechanisms in accounting for it, if the lexicalist (list) model is adopted. Consistent with any empirical investigation, the simpler model is always preferred as long as the facts are accounted for.

The second question to be raised concerning the lexicon is the general organization of lexical entries, specifically the three sets of features which represent semantic, syntactic and phonological information about the entry. One way to represent the lexical item is to relegate polysemy to sets of semantic features, i.e. to group them as a single phonological set of features, (see Katz and Postal [1964]). In this view *table* is entered once in the lexicon and the meanings (a) the top part of a gem stone, (b) a piece of furniture, (c) a systematic arrangement of data, (d) a geological stratum (and other meanings) are differentiated in the semantic features associated with the entry /table/. An alternative way to represent lexical items is to provide a separate dictionary entry for each meaning; thus there would be at least four entries for *table*, all with the same phonological features (see McCawley [1968]). The differences in these two views (and, for that matter, a third view which has never been suggested -- organizing the lexicon in terms of syntactic features) reflect among other things a most important aspect of lexical organization: either the phonological information (i.e. something closer to the surface forms) or the semantic information (i.e. something closer to the deeper or more abstract aspects of language). The data does strongly suggest that the semantic relationships between words is neurologically more important or prominent than phonetic similarity and thus indirectly suggests that the semantically organized lexicon is a better (simpler) model to relate to the central language system. This is in agreement with other data presented by Weigl and Bierwisch [1968] (above on pp. 83-84). Some of the data discussed above in section A on levels in the grammar will of course bear on this question, too, as will be indicated.

It is generally agreed in linguistic theory that the process of lexical insertion or lexical attachment (the formal operation of placing lexical items into appropriate syntactic frames) is not simply a matter of hanging formatives onto a deep structure configuration. As a matter of fact, the level at which they are attached has not been easy to determine, theoretically. Earlier theory assumed that lexical items were attached to deep structures so that the semantic rules could interpret the meaning at a point before any transformational rules had operated on the structures

(transformations were assumed to be meaning-preserving, i.e. not to affect meaning; cf. Katz and Postal [1964] for discussion). Since some transformational rules like negation do seem to affect meaning, since most transformations do not seem to depend upon the phonological features of a lexical item although some, like the coordination of compared adjectives do, and finally since some transformations introduce lexical items like pronominalization, or the Do-So rule, it is not clear whether there is any one point or level at which lexical entries should be added to syntactic structure. For further theoretical discussion of this point see Bach [1968], Gruber [1967], etc. One aspect of the problem of lexical attachment -- the fact that most transformational rules do not require mentioning phonological characteristics of lexical items and thus the attachment of phonological features can be done at a later stage in the derivation -- seems to explain certain aphasic deficits in a straightforward way; this will be briefly discussed in the next section.

#### 1. Organization of the Lexicon

The discussion in the preceding section leads naturally to questions about the general organization of the lexicon in the central language system. We know that words are coded entities in the nervous system for we produce and recognize them, properly and erroneously; notice that this in no way implies that they are stored in one particular location. We have seen good evidence that a deeper level of coding in the nervous system is the functional distinction between phonological, semantic and syntactic features of words. We may then speculate as to how the brain organizes its store of words (as before the term 'word' only refers to the major lexical categories and not to the grammatical formatives). It is conceivable that the lexicon is no more than a set, with no further connections or associations. However, our ability to rhyme, to find synonyms, to list nouns, name colors and the like argues against such a notion, and suggests strongly that the central language system is fully capable of associating lexical items along any of the three parameters -- phonetic, semantic and syntactic. We may ask though whether any of these associable parameters is functionally stronger, whether there are any hierarchies or alternatively, whether the lexicon is fundamentally represented along any particular parameter, the other parameters then being realized by special calculations. This question may be re-phrased to bring the evidence from aphasia to bear. Clearly, if lexical items were neurological units coded only in terms of phonological identities, we would expect that any impairment in ability to match semantic-syntactic features with appropriate phonological features would result in totally random words in actual speech output. Such a lexical structure (which is the one suggested by Katz and Postal [1964] pp. 12 f) asserts that the relationships between words is based upon phonetic similarity; if the sound-meaning relation is impaired, there would only be sound-sound connections to work with, thus any phonetic shape would be equally likely to result. If, on the other hand, lexical

structure is semantically based (cf. McCawley [1968] for theoretical arguments in support of this idea), a dissociation of sound-meaning relations would often lead to speech production and recognition in terms of semantic classes, for the meaning-meaning connections would be all that remained. This would likely result in circumlocutions or other meaning-related error possibilities. This is the conclusion drawn by Weigl and Bierwisch [1968] already noted before and is corroborated by some of my own data. In attempting to read the sentence *David wakes up early*, R.H. said:

(44)

VF And what does he do early?

R.H. To go to bed, early in the morning, very early in the morning to go to sleep.

The confusion here is *go to sleep*, *go to bed* with actual verb *wakes up*, clearly a similar semantic field showing a confusion of some of the semantic features but not others. In attempting to read the sentence *He eats his breakfast*, R.H. said:

(45)

R.H. To have, something to eat, he's having food which is /čer/, to eat in the morning and it's the first thing you eat for, for morning. He's eating, right here, your /ef/. Well let's see, first thing in the morning you'd eat, /brek/, to /brek/.

VF /fəst/?

R.H. Right! your dinner.

The error at first is more of a circumlocution than a confusion, for in fact R.H. has defined breakfast as the food which one eats the first thing in the morning. It is remarkable to observe that, having retrieved the first syllable of *breakfast*, upon being given the final syllable by the interviewer he repeated *dinner*. Again the conclusion must be that the semantic field dominates. In attempting to read the sentence *He goes for a ride*, R.H. said:

(46)

R.H. Take a, he's getting a, getting a start to go /rəč/ to walk, to walk. his ride

HW Can you say that again?

R.H. He's taking a ride.

In this case R.H. ultimately produced the noun *ride* but in the first attempts used the circumlocution involving *walk*. The substitution of *taking* for *goes for* is of the same order. The strength of the semantic association often holds up even in cases of severe non-fluent aphasia, as in the next example. In an interview testing for object recognition, LBVAH-G.R. responded as follows to being shown a bottle of hand lotion:



(47)

D What's this item right here, G.?  
 LBVAH-G.R. lady, man, that's a polish here  
 D What do they do with it?  
 LBVAH-G.R. Polish their face  
 D Well you showed that to me correctly [Evidently, G.R. had rubbed his hands as though applying hand lotion].

Another patient, K.H., made errors that also follow this hypothesis. He referred to his wife as *mother*; shown a picture of a boy putting on his pants with the caption "he dresses himself" he said *shoes*. Asked to identify the tape recorder, he wrote the word *stereo* (misspelled as *stero*). In another part of the interview with LBVAH-G.R. he was asked to name objects whose picture he was shown; two of the errors made were *office* for a picture of a chair and *cookies* for a picture of saltine crackers. In a repetition task G.R. said *fifteen* for 'five' and *ninety* for 'nine'.

R.H., K.H. and G.R. all had marked symptoms of apraxia of speech; that is the phonetic output of many words were highly distorted. R.H. and K.H. (whom I personally worked with) both suffered from severe alexia and had great difficulty in repeating words; on the other hand, they both were less severely impaired in writing and speaking although still clearly aphasic in these latter modalities. Therefore the strength of the semantic associations in their most disrupted modalities, the visual and the auditory, seems good evidence that the basic organizational parameter of the lexicon is semantic and thus supports the model proposed by McCawley [1968].

As would be expected however, lexical structure is not at all so simple. There are very interesting sorts of data from aphasics which indicate that some type of *syntactic* classification of the lexicon is used by the central language system, too. Marshall [1968] reported on some interesting data from an alexic patient; the test used was to ask the patient to read aloud individual words on cards. The types of errors were classified by Marshall as follows:

- (1) SEMANTIC: read *freedom* for *liberty*
- (2) VISUAL: read *exit* for *next*
- (3) VISUAL COMPLETION: read *gentleman* for *gentle*
- (4) VISUAL & SEMANTIC: read *orchestra* for *sympathy*
- (5) Indeterminate: (comprised only 6% of the data)

The results he obtained, indicated as percentage of errors, as shown in Table 3.

	Nouns	Adjectives	Verbs
semantic errors	69	49	37
visual errors	10	36	36
visual completion errors	9	6	18
visual & semantic errors	(appreciably smaller)		

Table 3. Alexic Errors and Grammatical Category

The most interesting data obtained, however, was the fact that the different grammatical categories ranked in a hierarchy of difficulty, which might suggest that such categories subdivide the lexicon. Out of context, i.e. on the cards as a single word, both verbs and nouns were read as *nouns* approximately 90% of the time as shown in Table 4:

Classification of part of speech of the Stimulus		Classification of part of speech of the Stimulus		
		Noun	Adjective	Verb
Classification of the part of speech of the response	Noun	90	24	90
	Adjective	8	72	10
	Verb	2	4	--

Table 4. Hierarchy of Grammatical Category Error

The facility for reading nouns properly and the corresponding difficulty in reading verbs and adjectives suggests that there may be a syntactic division of the lexicon (like the feature division) between N and other major grammatical classes. Since Marshall's patient only suffered from alexia and thus the deficit is properly identified as a performance impairment, it would be instructive to see if other data supported this notion of the syntactic division of the lexicon, on a deeper level. Although by no means absolutely confirming, I think there is such evidence. Luria [1968] discusses an aphasic syndrome which he calls "dynamic aphasia"; his theoretical framework is more psychological than linguistic but still illustrates an aspect of this question. He notes that dynamic aphasics can name objects, repeat words and sentences,

but cannot use speech for free spontaneous communication if the syndrome is severe; i.e. such a patient cannot initiate the topic of a sentence (usually the subject NP) and thus either speaks haltingly in fragments or not at all. These patients can, however, complete a sentence perfectly if it is initiated by the examiner; i.e. if the examiner gives a subject NP the patient can add an appropriate predicate to it. The implication which Luria made is that the noun, which is the head of the NP, and the verb, which is the head of the predicate in simple sentence structures of course, are differentially affected in this type of aphasia. To test this, Luria asked his patients to name as many nouns in one minute as possible and on another test to name as many verbs as possible in the same time span. The results showed that dynamic aphasics could name on the average four times as many nouns as verbs (Luria [1968] pp. 298 f). By considering some other tests, Luria concluded that the "linear scheme" of the sentence<sup>6</sup> is defective in dynamic aphasia, exemplified by the ability to name nouns but not verbs. From my point of view, however, the syntactic division may not be strictly a matter of the grammatical category of the word but a matter of the syntactic phrase into which the word would be placed in a normal sentence -- something on the order of the subject or topic NP and the predicate or comment VP, in simple declaratives. Let us reconsider the strategies and abilities for producing a sentence that were suggested above in section A.2. The ability to make use of the syntactic phrases which constitute a sentence (and which therefore determine appropriate contexts for lexical items to attach per their grammatical categorization), is further divisible into two parts: (1) might be applicable to NP in general but particularly the subject or topic and (2) might be applicable to VP or generally phrases other than NP's. In other words, the knowledge of sentence structure is first the information and ability to make use of NP's and second the information and ability to make use of, e.g. VP's (the hierarchy which is implied by numbering them is in fact intended). With this hypothesis some predictions are possible. We would expect, for example, that if a patient has a mild dynamic aphasia (note that Luria describes such patients in terms of the degree of inability to name verbs) would be able to do the word-to-sentence task #8 but would find it easier to do it if given *nouns* as stimuli than he would if given *verbs*, because the NP structure to which N are attached are more *available to him* (notice that the proposal as to the underlying structure of NP to be discussed in Section C is not related to this hypothesis).

W.L. and K.T. were checked for the degree of facility in listing nouns and verbs, following Luria's procedure. Both were able to produce a list of nouns in a minute's time -- K.T. giving 12 different N and W.L., 8. On the verbs, K.T. did noticeably better than W.L. although he found it both more difficult to understand the instructions and harder to do the task itself (impressionistically measured by the frequency and duration of pauses); K.T. gave 4 different verbs in the same time span. I was not able to get W.L. to produce any verbs in this fashion!

It is clear that both W.L. and K.T. have some degree of Luria's dynamic aphasia and it likely that W.L.'s is the more severe. Test #8, the word-to-sentence test, revealed some interesting phenomena relevant to the question of the availability of NP structures as opposed to other syntactic phrases, especially the VP. In the first place, it was quite clear that N stimuli were handled more easily by both patients (the response was faster and the errors less frequent) than V stimuli words, as would be expected. A surprising thing occurred when the stimulus word could have been taken as either a noun or a verb, e.g. /sE/ = *see* or *sea*, even when the noun homonym was a word with a lower frequency of occurrence than the verb, e.g. /rEd/ = *read* or *reed*, the noun form was chosen, even if it necessitated a great deal of circumlocution on the part of the patient to construct a sentence. Some of the responses which illustrate this phenomenon are as follows; it should be remembered that the stimulus word was given verbally and was also printed on a piece of paper for visual input unless it had two spellings, in which case it was only given verbally.

(48)

HW ring  
 W.L. Ring goes with other methods in ringing  
 HW Why don't you tell me what it means?  
 W.L. This is what I'm doing, I've got about nineteen different ways to do it, I can't even say that, for instance in an automobile you talk about a ring, that goes into it, I've been working with rings in an automobile

(49)

HW /sE/  
 K.T. sea is in the ocean, it's usually a boat  
 HW /sE/. What's the first thing you think of?  
 K.T. seaweed

It is possible that my question "What's the first thing you think of?" prompted a noun response, of course. Generally I was careful to ask for additional information or an additional response without prejudicing the part-of-speech to be selected.

(50)

HW /rEd/  
 K.T. well they have in the, what we would call the 'boonies', it's a weeds, how is it? a week is a, you just, something like grass or reed

It is apparent that even word frequencies are less important than syntactic categorization, since surely *reed* is significantly less frequent than the V *to read*. W.L. made an analogous but perhaps even more esoteric choice when given the word /rEd/ -- he attempted to describe the double reed of an oboe. On another homonym, *paint*, both W.L. and K.T. used the N form in their response.

By far the most interesting responses, however, were those in which the verb stimulus word was converted to its derived nominal or the infinitive form of the verb was used as an NP or the response clearly indicated that a nominalization was intended even if the surface form was deviant with respect to its correct phonetic specification. The following responses show this phenomenon:

(51)

HW           decide  
 K.T.        I have to have it, I either have to, well, let's say that it's going to be a real decision (snaps his fingers) just like this it'll be real fast.  
 HW           strike  
 K.T.        striking a match #

(52)

HW           conceal  
 W.L.        concealment  
 HW           Use it in a sentence  
 W.L.        I gave a concealed weapon

The last one is atypical in that what appears to be a repetition of the original stimulus word is the derived nominal and then upon being asked to use it in a sentence, W.L. employed the adjectival form *concealed*. The failure to match the selectional constraints between *give* and *concealed weapon* is typical in W.L.'s language and has been commented on in the preceding section.

(53)

HW           smile  
 W.L.        I hate smiling (W.L. then laughed)

Given the word *obstruct*, K.T. went into a long description of the problems of combat patrolling in Vietnam, emphasizing the difficulties of walking past such barriers as swamps, ditches and the like. When I asked him to clarify what he was talking about, he replied "well, the *obstruction* here...", which is the derived nominal form of the original stimulus word.

(54)

HW           arrange  
 K.T.        arrangement? I'll have an arrangement with my mistress.  
 HW           engage  
 K.T.        My engagement is just about finished. my engagement is almost over with.

(55)

HW           smoke  
 K.T.        smoking is not good for me

This last is interesting in that *smoke* can be either an N or V but K.T. chose to nominalize the V form.

An illustration of the infinitive nominal form is:

(56)

HW           tear /ter/

W.L.        To tear a piece of paper, is correct.

As noted above, on occasion there was a clear attempt to use the derived nominal but the phonetic specification, particularly of the derivational affix, was sometimes in error. Consider the following:

(57)

HW           reside

K.T.        My residing is 1957 West Avenue, 41.

HW           fill

K.T.        I've got a fill, plate for my teeth

HW           contain

K.T.        Can I have my containment, please?

HW           contain. Do you know what 'contain' means?

K.T.        yes, contain

HW           What does it mean?

K.T.        contained is a # it's usually about so, like about this from here about over here, for example, worms, when I have a containment, or for# (during this time K.T. was using his hands to picture a can or container)

It is perhaps not too unreasonable to assume K.T. was thinking of *hunting* in the following:

(58)

HW           hunt

K.T.        hunt and deer, this is what I really like to do

And it is also reasonable to assume he had *reception* in mind in this:

HW           receive

K.T.        well I say I had another baby and we're going to give this present type thing, we'd say, it's a, no it's not a thank you it's a #

HW           receive

K.T.        receive engagement, receive birthdays, we always say have a little note and say thank you very much

The notion of the semantic association or field has been developed above and good evidence has been shown that verb stimulus words are analyzed as nouns, often by employing the derived nominal. If the derived nominal were not available for the response, the patient could either try to formulate a sentence with the verb or attempt to locate another noun

within the semantic field. The latter strategy seems to be the case in the following examples:

(60)

HW bathe  
 K.T. it's like a bath, usually, wash myself, my face and hands  
 HW speak  
 K.T. debate # I speak politics, tomorrow's discussion  
 HW jump  
 K.T. well, usually for, the Olympics they have a # not the races  
 but # What do they call those things? # see they have things  
 like shot put and pole vaulting, pole vaulting!!

Presumably *bath*, *debate* and *pole vaulting* were substituted for *bathing*, *speaking* and *jumping* respectively, which is quite similar to the semantic substitutions noted above. The whole hypothesis of using a noun or nominal when possible is given further support by the following examples in which it is quite clear that the patient had no idea of the meaning of the words in question and yet still considered the verb stimulus word as a noun.

(61)

HW proliferate  
 W.L. proliferate is a complete time about a word that is correct  
 HW went  
 W.L. went came in better than it did before

Note that *went* is a very high frequency verb; it is of particular interest that it should be taken as a [N], too. There are three other responses which relate to the general phenomenon being discussed in that they indicate once again the relative inaccessibility of the verb as a response compared to even the agentive nominals (which theoretically are not semantically idiosyncratic like the derived nominals but have rather consistent relationships with the associated verb) and adjectivals.

(62)

HW admire  
 K.T. your admirer # your tact  
 HW catch  
 K.T. is that *Catcher in the Rye*? wasn't there a book?  
 HW persuade  
 K.T. say I wanted my wife, he doesn't really want to go to the  
 show and I want her to so we'd have to, uh, persuasive so we  
 can go up to the show

The deviant use of personal pronouns in K.T.'s language has already been noted.

To my knowledge the nominalization phenomenon in aphasic speech like the above cited from the responses of W.L. and K.T. has never been

described in the literature before. None of the control subjects did this; their responses invariably used the same syntactic category as in the original stimulus word, although as one would expect [tense] was altered on occasion. W.L. and K.T. responded in the above manner spontaneously. Nominalization was not taught them prior to these tests nor was any mention made of it until after the above data had been obtained. At a later date some work directly involving nominalization (agentives) was done which will be discussed shortly. The question then is how to plausibly explain the facts.

## 2. The Lexicalist/Transformationalist Debate

If the lexicalist/transformationalist debate is carefully considered, it is obvious that either theoretical proposal is capable of providing an account of this data. Nevertheless, it does not seem to me that the choice of a model is simply arbitrary. The possible strategies involved in successfully completing the word-to-sentence task were suggested in section A.2; there, it was argued that a brain-damaged patient is likely to pursue the easiest (for him) set of strategies or calculations which will lead to a response. We already know from data presented in section A that W.L. and K.T. have central language system deficits involving major semantic features in the grammar: animacy, humanness, number, gender, concreteness, case, person, etc.; these features are more general (in the sense of the variety of structures which they affect) and therefore I *assume* are more critical in the language system than the kind of exception features involved in derived nominals. If this assumption is plausible, it is surely equally plausible that an aphasic deficit affecting important features would also affect minor features; in other words, it is reasonable to assume that the exception features for derived nominals are lower in the hierarchy of features than those used for selectional restrictions or syntactic operations such as pronominalization and agreement. If we try to explain this data with the transformationalist hypothesis we not only have to argue against these very plausible assumptions but more significantly have to argue that the additional computation or strategy of applying the nominalization rule is employed by these aphasics in doing this task, when clearly they could (theoretically) have avoided the extra operation by supplying the response word in the verb form.

On the other hand the lexicalist hypothesis -- that the derived nominals are *listed* as part of the lexical entry for the word in question -- requires us to make only one assumption, namely, that there is a syntactic hierarchy of accessibility in the several forms a lexical item can take, the N form being the highest one and least susceptible to disruption. And as already noted, Marshall [1968] has independent evidence that this is exactly the case. There is other evidence in support of this line of reasoning, namely the way in which W.L. and K.T. responded to a task designed expressly to make use of agentive nominalizations.

The agentive nominal test consisted of my presenting either a verb or an agentive nominal and asking for the alternate form from the patient. Thus, if I said *sailor* the correct response would be *to sail* and if I said *to survey* the correct response would be *surveyor*. It is worth

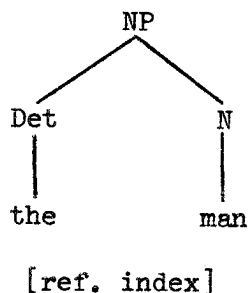


noting that neither W.L. nor K.T. had the slightest difficulty in understanding or doing this task for the agentive nominals like *teach/teacher*, *swim/swimmer*, etc. Consequently, the agentives were used rather than the more problematic factive and gerundive nominals. My intention was to trick the patient into applying the rule (in either direction: making the agentive or analyzing its source verb) when in fact it did not apply. That is, after presenting a few verbs like *to fly*, *to fight*, *to paint*, etc. I would present the irregular verbs *to nurse*, *to kiss*, *to type*, etc.; or, after presenting a few standard agentives such as *administrator*, *hunter*, *baker*, etc. I would present unanalyzable nouns (cf. Lees [1963] pp. 70 f) such as *carpenter*, *grocer*, *soldier*, etc. When the rule was not applicable, I either got no response at all, or laughter. Incidentally, I tried the reverse procedure, too: presenting the non-occurring forms with as natural an intonation as I could muster; e.g. to try to elicit *grocer* I presented /tuw grOš/, and got responses like "what the hell does that mean?" Since we may assume that aphasics find it extremely difficult to analyze language in linguistic terms -- as one would expect, since so much effort and concentration is required merely to use language in the first place -- such a response set indicating that the (putative) rule does not apply would be remarkable indeed if nominalization were a transformational rule, for that would entail the patient's being immediately aware of a rather sophisticated "exception" feature (i.e. a marked lexical item; for the theoretical arguments supporting such a proposal see Lakoff [1965] and for counter-arguments see Chomsky [1968]). In trying the same test on the control subjects I found it quite easy to trick them into the non-occurring response, although of course the deviancy was often recognized shortly afterwards. As I mentioned, this agentive nominal test was not conducted in a thorough systematic way and hence can only be regarded as a pilot study indicative of a possible generalization. It does suggest to me that normal subjects are capable of playing a game of adding the /-ər/ affix or removing it as the case may be, but that aphasics find this more difficult than referring to the lexical entry to ascertain whether or not the alternative form is present or absent. In this case W.L.'s and K.T.'s responses can easily be attributed to the fact that the non-occurring forms were simply not in their lexicons; the fact that they noticed the gap is psychologically interesting but no longer so linguistically surprising.

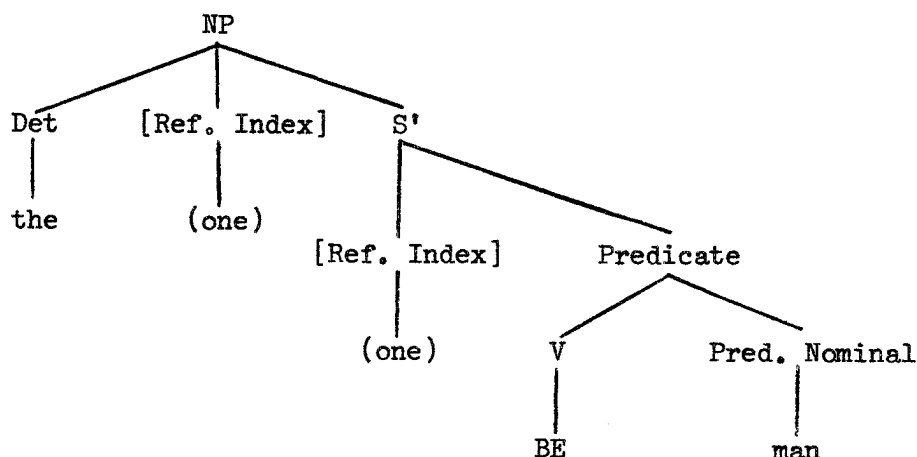
### C. A Problem in the Underlying Structure of Noun Phrases

One of the problems in the analysis of the structure of noun phrases is how to handle reference, the property by which an NP refers to things in the real (or cognitive) world, to other NP's in the same derived sentence or (anaphorically) to NP's in other sentences. This problem is crucial in the description of processes like relativization, pronominalization, complementation, etc. There are a number of proposals for handling reference in the grammar. I will confine my remarks to a particular aspect of two such proposals, one which develops the referential

index as features of the lexical item [N] (Chomsky [1965] pp. 145-147) and one which develops the referential index as a separate category in the deep structure of the NP (Bach [1968] pp. 104 ff). The former position introduces nouns with their referential index via a base rule of the form:  $NP \rightarrow (Det) N (S')$ ; thus for the NP, *the man*, there would be the following deep structure:



Bach's is to establish the referential index as the "head" of the NP and to provide the referential index with the lexical realization *one*, *thing*, etc. under certain conditions. Instead of being directly generated by the base rules as in Chomsky's model, [N] is introduced separately as a Predicate Nominal. The rule is not formalized in Bach, but for NP, *the man*, the following structure exists:



The relative transformation applies to this structure generating the phrase *the one who is a man* and Rel-reduction rules and a rule for deleting *one* ultimately generate the NP, *the man*. The full theoretical discussion is in Bach [1968].

The difference between these proposals which I wish to consider is the way in which referential indices are characterized. Notice that both models distinguish the index (and in this sense are equivalent) but in Chomsky's it is one part of the collocation of features which comprise a lexical item and in Bach's it is one part of the collocation of features which comprise an NP, the lexical item being a second part. Further, Bach claims that the referential index has its own lexical

realization, presumably specified by a late transformational rule. The aphasic data discussed in section D below supports Bach's proposal in that I believe it can be shown that the lexical form of the referential index can be realized as speech output even though the realization of the lexical item, the N, is blocked due to neurological deficit. Presumably Chomsky's proposal (as well as others) could be patched up in such a way that similar predictions could be made, but unless it were to make virtually the same distinctions between the noun and the referential index of the NP (i.e. become a notational variant), it is difficult to see how such a patching up would avoid either complicating the model itself or the model's relationship to the neurological representation of the central language system. The hypothesis which matches the data in the simpler way is again clearly preferable.

It will have been noticed that I made no claims about the other aspects of Bach's proposal, particularly the suggestion that the [N] is introduced as a Predicate Nominal in a BE-clause attached to the referential index head of the NP. From the standpoint of the aphasic data I have no direct comments on this part of his proposal. However, if the hierarchization of syntactic categories ([N], [V] and [Adj]) proposed by Marshall [1968] (discussed in the next section) is valid, then it is possible to argue that Bach's *reason* for introducing the [N] this way is not a very good one. Bach establishes this structure in order to obliterate the distinctions between the grammatical categories noun, verb and adjective at the deep level of the grammar. (Bach [1968], p. 91); evidence from aphasia indicates that these categories are not on a par in the central language system and it may be that further research will produce good evidence that they must be distinct. (Also cf. Luria [1968] and the notion of "dynamic aphasia" which bears on this point, too.)

Without serious distortion of the theoretical notions, we may consider the referential index (the variable) to be that aspect of the NP by which the linguistic system refers to or interacts with the cognitive system and ultimately the outside world. That aspect of the NP by which the grammar *internally* selects the lexical items (the nouns) which have appropriate semantic, syntactic and phonological features, might be represented by the lexical node in the base rules; in Chomsky's [1965] version this is the [N] and in Bach's [1968] version this is the [Pred Nominal]. As already noted, the question of interest is whether the referential index is an aspect of the lexical item (i.e. "brought up and attached" with the noun) or an aspect of the NP itself. If these theoretically distinct parts of the NP could be separately disordered we may find evidence for their neurological distinctiveness; specifically, it might be that (1) a person's use of the internal lexical selection system would be dissociated from its references (real or intended) or that (2) a person's referential system would be unable to make use of lexical selection, to venture just two possibilities. What speech output might be in the case of (1) is not at all clear to me and consequently I am not sure whether I have any data that bears upon

it or not, although a potential candidate is discussed below. But in the case of (2), in which the lexical selection system was disrupted, we might expect that the speech output of an aphasic so afflicted would still be syntactically acceptable. It would appear as though the patient knew what he was talking about, but one could not know for certain because many of the NP's would be realized as *one* or *thing* or some other semantically empty form. In other words, such an aphasic would be unable, or at least would find it very difficult, to spontaneously produce nouns, particularly if they were part of [+def] NP's which related to the topic of a sentence (and thus were the noun to which subsequent pronouns referred, for example), the subject of discourse (and thus were the noun to which anaphoric reference would be made), etc. Some aphasic language behavior appears to reflect just such a loss. However, I must point out that the evidence cited below is highly tentative, partly because the hypothesis is speculative enough to make it difficult to think of valid experimental measures and partly because none of this data was obtained with the hypothesis in mind. On looking over the aphasic language which I had gathered, I found a noticeable use of indefinite nouns like *one*, *thing*, *stuff*, etc. apparently correlated with the classical syndrome anomia, a naming or word-finding impairment. This syndrome is well-documented in the literature (Penfield and Roberts [1959], Geschwind [1967], Head [1926] to name but a few) but to my knowledge no one has observed the correlated use of indefinites. Consider the following interview with LBVAH-C.S.; he does not use nouns at all which is in striking contrast to some of the other aphasics studied and underscores the fact that there is a very wide range of deficits in aphasia. C.S. can, as is quite obvious, pronominalize freely and relativize as well:

(63)

MM Can you tell me how long you've been sick?

LBVAH C.S. Well, I think it started, it begin first, that was the first one, the first one that had before any of it, I don't know if it was but somebody did once, I don't know when it started any though.

MM (Is this) the American flag?

LBVAH C.S. Well it hasn't as far as each one, no. You see it don't give it quite, somehow it don't give it quite, I don't know why, see?

MM What's this about? What is this? [apparently picture of a man]

LBVAH C.S. I don't know that anything as far as it is now.

MM What does he do?

LBVAH C.S. I haven't noticed who it is, who that is.

In this connection we might consider some further data from R.H. who, quoted before in connection with his apraxia of speech, obviously has other deficits. [Asked to read the sentence: "He takes his cod-liver oil."]

(64)

R.H. To put in, which I'm sure she takes the first thing if she's supposed to take, this is your morning, this is your, thing that you would hate to take (laughs) that no part of, I know exactly what it is I can't put the name up and she takes it every morning, its.

VF It's a special kind of oil?

R.H. That's exactly right dear that she should take the first thing is a boy she eats every morning, right, to start it's good for you. I never like it myself.

Notice that R.H. reproduced the verb *take*, the pronoun subject although he did confuse the gender of the pronoun on several occasions and evidently recognized that cod-liver oil is customarily taken in the morning. He does recognize and understand the noun *cod-liver oil* but cannot actualize it phonetically. To test this out with a different example, he was given a written set of nouns, one of which was *squirrel*, which he identified as matching a picture of a squirrel even though he was unable to either utter or repeat *squirrel*. Unable to say *cod-liver oil*, R.H. said "this is your *thing* that you would hate to take..." and later used the pronoun *it* to refer back to *thing*; in both cases he surely used the indefinite in lieu of the noun itself. In another test, R.H. was asked to identify a picture of a child's tricycle:

(65)

R.H. He's taking a little, little kid's, a little thing that you'd ride.

HW Is it a scooter?

R.H. Oh yes.

HW Is it a tricycle?

R.H. It's a little thing and three ways, one two three there's three ways to it, one in the front and hanging on the side.

Although we find the indefinite, *thing*, used in lieu of the noun, *tricycle*, as in the preceding example, this time there is a problematic item: *ways*. *Ways* could be explained by a phonetic analogy to *wheels*; on the other hand, considering it to be simply another of the indefinite nouns is equally possible, given the paucity of data.

Unlike either LBVAH C.S. or R.H., patient W.L. is a fluent speaker.

However, in conversation he does not communicate well because his speech is filled with anaphoric NP's that have no referents, and, like the above two patients, an abundance of indefinite nouns like *one, thing*, etc. The fact that his spontaneous speech is largely a succession of incomplete sentences is, I think, irrelevant; the syntactic structure of phrases is fairly intact (although as has already been noted, his knowledge of semantic features for selectional restrictions is virtually lost). This loss is not seen to a great extent in his conversation because there are so few complete sentences and it is the larger unit, the sentence, across which selectional constraints play such a vital role. W.L.'s lack of nouns is not as marked as C.S. or R.H., either. Anomia is often characterized as a disorder of speech *production*, sometimes referred to as a word-finding problem, because patients with this deficit are often able to recognize the word they were searching for, if given a choice of several words one of which is the correct one. This is true, for example, of R.H.; it is therefore interesting that W.L. will on occasion, when trying to find a noun, use pencil and paper to write it out, after which he can then pronounce it! Lacking pencil and paper he will use his finger to imaginarily "write in the air", a technique which also "de-blocks" his lexical selection problem. If one were to consider this aspect of W.L.'s disorder a performance problem (bound to the verbal modality) which is a reasonable assumption, it still poses problems. Among other things, such a deficit indicates that it is not a matter of a disrupted lexical representation but a disrupted verbal lexical selection strategy. LBVAH C.S.'s deficit is more a central language system or competence loss, since he neither produces nor recognizes the nouns he needs.

On the other hand, consider what the significance of W.L.'s impairment really means. Assume that his deficit in this connection is only in the verbal modality and can be overcome by employing the tactile modality when necessary. The deficit then must be the strategy of actualizing the *phonological* representation, and not the major lexical selection strategy. Therefore, he probably has the whole NP *structurally* (his lack of semantic features has been noted) i.e. both the referential index and the lexical selection of the noun; failing to pick up the third element, the phonological specification, the only specification left is the realization of the referential index and the final output looks to be like that of a person with the more central deficit. A short sample of W.L.'s conversational speech will clarify these remarks; the possible realizations of the referential index are underlined:

(66)

HW           How're you getting on with the doctor?

W.L.         I don't know which one you're talking to.

(67)[different part of conversation]

W.L.         ...The amazing thing is when you have doctors today, let's say

you have doctors all that you have right now, now let's say some *thing* like this happens, to how many other, pa--, pa--, patients are there, who are knocked out with this *stuff*, and how many go at that time, as far as being medicines are concerned, the one, two, fifteen, a hundred and twenty five? at this point?

(68)[different conversation]

W.L. At twenty three hundred he was starting to get this *thing* straightened out and he was. I tried to help him with some of this *stuff*, where Duffy wasn't there. All of a sudden all these *things* I knew about Duffy were interested to him, tremendously interested. I could turn around and I couldn't see it, it would be coming from some. bag, right straight in and all this new *stuff* and I couldn't then turn around to the man I was talking to, at all. This is the idiocy of the thing. Because the more you've got of this *stuff*, the less you could push out of it. Do you follow what I'm saying

HW In other words, you were unable to do anything else?

[Note: Although I did not have the slightest idea what W.L. was talking about, I did want him to continue talking.]

In the following conversation W.L. was referring to the different pills he was required to take, to his epileptic seizures, to his visual field defect (hemianopia) and possibly to his general state of health and mind. It is clear even from this short sample that the listener could not sort out which references were intended and that the conversation of W.L. thus deviates substantially from the normal.

(69)

W.L. And all of a sudden I couldn't do this *thing* right and as I say, I finally got home and I started to put this *thing* down and I knew exactly what I was looking for under and it came true.

HW Did they give you the wrong medicine?

W.L. I believe that they're giving people a great deal more than they have ever should have before. That's why I was opposed to get three of *one* and two of the *other*. pills on these *things* and I, boy I, first time I ever talked to *these*. Way up here in the air just stopped the whole *thing* right there.

HW Did they put you to sleep or what?

W.L. Well, I've been put to sleep before and when I started to get me these *things* it knocked me out completely. not completely

but you way, walking like you were seventeen feet tall or eight feet tall; with this sort of *stuff* and I just quit it.

The data above is different from some manifestations of anomia; for example, consider some isolated samples from the conversation of L.P. who was discussed above in connection with ictal speech automatisms. L.P. had extreme difficulty in speaking in his ordinary utterances and used very few nouns:

- (70)
- HW Did you ever take the family on camping trips?
- L.P. we had # we've been kind of, uh, horsing around with out, uh, cabin here in the last, uh, *thing*
- HW You have a cabin?
- L.P. we have a cabin. But that was just a # that's # it had a #
- HW How big a cabin is it?
- L.P. well, uh, oh, it's, it's about uh # This mind over matter you can't seem to connect it # uh, it's a nice, uh, it's a nice
- HW Is there any water?
- L.P. Yea, there's some water on the, going around this way you can, this, I

On only one occasion was an indefinite used where a noun should have been (it is in italics above) but in the other instances there is simply a block following the article and any modifiers. Since the articles *the* and *a* are often used, it is patently obvious that L.P. has some structure of the NP available; for some reason though, he does not employ the indefinites with any regularity to cover for the inaccessible nouns.

Needless to say I do not have a thoroughly satisfactory explanation of the above data, but I think a partial account can be ventured. First, consider what aspects of the grammar (in linguistic terms) could lead to a failure to produce nouns. Some possible candidates are:

- (1) a disruption of whatever function is represented by lexical attachment (the lexical insertion rule)
- (2) a lack of access to the neuromuscular specification of the lexical item or a lack of that specification altogether (tracking)
- (3) a missing or severely disrupted set of phonetic features --



loss of the lexical representations.

In the case of W.L. (2) and (3) may be immediately ruled out for the simple reason that when he used the tactile modality to de-block the verbal, he could then produce the appropriate noun. If (1) is a partial explanation of W.L.'s deficit, then we are forced to consider the referential index as part of the NP and not part of the N, as proposed by Bach [1968], on the reasonable assumption that the referential index can be lexically (phonetically) realized by one of the indefinites.<sup>7</sup> This analysis is supported to some extent by the fact that he can pronominalize and relativize, both of which basically depend upon the referential index (although not exclusively, of course). L.P. does not pronominalize frequently (in fact he does not string together complex phrases at all except in the ictal speech automatisms) and he rarely uses the indefinites. Yet as noted, he must have something of the NP structure available in order to produce articles. As was noted above in section 4.0, L.P. is very aware of his inability to speak fluently and to use nouns and he quite often remarks that he knows what he wishes to say but cannot produce it. I hesitate to take his judgment of his own deficit at face value, but it is a fact that the typical Broca's aphasic can identify the nouns which he cannot utter, if presented in verbal choice tests. W.L. is also aware of his language disorder, but in an entirely different manner; he rarely comments upon it in a conversation (although he frequently notes his physical handicaps such as the visual field defect) and when he does it is only in the most abstract terms and never as a specific remark like L.P.'s "I know but I can't seem to communicate."

I would propose, very tentatively, that L.P.'s deficit is not in the lexical attachment function (1), but in either the lexical representations (phonetic features) (3) or in the neuromuscular specifications (2). Since a deficit in (2) is likely to be a concomitant of dysarthric symptoms (but not necessarily) described above in A, by elimination one is led to consider L.P.'s deficit primarily as a *loss* of lexical representations. It will be recalled that it was strongly argued earlier in section A that disruptions of the lexical representations produced apraxia of speech. Obviously I am now suggesting that there is a difference between disruption of the phonetic specification of a lexical item and the loss or loss of access to the entire phonetic specification; in any event, this still does not account for the lack of indefinites in L.P.'s speech and obviously leaves many questions unanswered. The investigation of this aspect of Bach's proposal and its interrelationship to the lexicon and proposed deep structure of NP's, is clearly an important and problematic area of linguistic research that merits further attention.

#### D. Lacunae and Further Research Problems

The data obtained from the patients at Long Beach VA Hospital raised

a number of questions about linguistic hypotheses for which there are only partial or tentative answers; these questions are of sufficient interest however to merit even a cursory discussion. For example, considering the tests used in this study (see Appendix B) it is somewhat noteworthy that I could not find a patient who was completely unable to produce relative clauses, coordinate sentences and complements, providing that his speech production and recognition was of a sufficient level to understand the tests. It is obvious that a severe aphasia -- such as an inability to utter more than one or two words at a time coupled with a severe loss in speech comprehension -- will also result in the inability to do these tests successfully, but that is hardly good evidence for the existence of a particular rule. What I did find was that errors in these tasks related to components of rules (as shown below). In turn the errors correlated with deficits identified on other tests, suggesting that the neurological substrates of the transformational component may in fact be a set of strategies. When an appropriate subset is selected and applied to an appropriate structure, this results in an operation which in total may be linguistically analyzed as a transformational rule. Some of these strategies may in fact be the neural analogs of "elementary transformations" (see Chomsky [1956] and Chomsky [1961]) and others may be those units and constructs which have already been noted in this chapter.

Consider for example K.T.'s errors on the complement test (#4) in which the task is to make a single sentence from the two putative underlying source sentences; he often constructed a complement regardless of whether he used the original verb in the complement sentence or confused the nouns and pronouns:

Stimulus Sentences  
(71)

K.T.'s Response Sentence

- |  |   |
|--|---|
| a. i. Michael imagined _____             | Michael imagined that I was writing a beautiful song. |
| a. ii. Michael sang the song.            |   |
| b. i. The colonel wanted _____           | The colonel wanted you to have the foxhole.           |
| b. ii. The colonel got into the foxhole. |   |
| c. i. Michael persuaded _____            | Michael persuaded them to go to the movie.            |
| c. ii. I went to the movie.              |   |

Sometimes the complement verb was erroneously deleted without substantially altering the sense of the two sentences:

(72)

a. i. They expect \_\_\_\_\_

I expect him tomorrow.

a. ii. He will come tomorrow.

And on one occasion K.T. juggled the possibilities several ways, partly confusing the meaning of the main verb and partly failing to keep track of NP's involved:

(73)

a. i. She approves of \_\_\_\_\_

She approved of cigarettes, or you could say, she approved of smoking

a. ii. She smokes cigarettes.

[3 weeks later, same test]:

She approved me to stop smoking.

Considering K.T.'s errors on the pronoun test noted earlier, this data is hardly surprising; he seems to be capable of using complementizers and making the appropriate adjustments to the VP of the complement sentence, but is unable to keep track of the semantic features of the NP's, particularly when it is necessary to do so for deletion, for the accusative rule and for the correct pronominal replacement. On the same complement test, W.L. did very poorly. Although he kept track of the NP's much better than K.T. and did not introduce spurious verbs as did K.T., W.L. never changed the verb of the second sentence to the infinitive or -ing complement form (that-complements, of course, don't require altering the verb).

(74)

a. i. The sergeant ordered \_\_\_\_\_

The sergeant ordered the private who got into the foxhole.

a. ii. The private got into the foxhole.

b. i. Michael wanted \_\_\_\_\_

Michael wanted John who bought this car.

b. ii. John bought the car.

c. i. Sam imagined \_\_\_\_\_

Sam imagined a man named John who wrote the book.

c. ii. John wrote the book.

Incidentally, W.L.'s written response to (c) was different and at least partly indicates that he could construct that-complements:

(75)

[written response to c.3:

Sam imagined that a fellow John  
who wrote the book.]

(76)

a. i. John thinks \_\_\_\_\_

John thinks John is strong.

a. ii. John is strong.

On the other hand, he sometimes used that-complements incorrectly, which leaves entirely open the question of whether he knew he was making a complement and did the wrong one or whether he just erred,

(77)

a. i. Michael persuaded \_\_\_\_\_

Michael persuaded that I went  
to the movie.

a. ii. I went to the movie.

which makes it difficult to decide then whether W.L. had (1) lost all but that-complements or (2) had lost all compliments but still knew how to change a sentence into a that-clause or (3) just did not understand the test in the first place. Note that in text #8, the word-to-sentence test, he did use infinitive complements, although very infrequently. These examples are:

(78)

a. expect

I expect to go to school

b. decide

I'll decide to go

c. imagine

We imagine to return to heaven

Of course, the last should have been the -ing-complement, *returning to heaven* and is thus syntactically deviant. A careful search of all the responses and conversations of W.L., an estimated twenty hours, turned up one example of an -ing-complement:

(79)

I think apparently I'm going to have to be doing more of this just to keep pushing and pushing and pushing to get these things right back into shape again...

This is hardly very good evidence that W.L.'s ability to use complement structures was intact but is still equivocal enough to leave the question open.

In another test W.L. was given a set of 8 cards, each of which had a single word printed on it as indicated:

(80)

AT	LITTLE	RED	LOOK
APPLE	THE	WILL	YOU

He was instructed to arrange these in proper form. W.L. first arranged a phrase on the table to read THE LITTLE RED APPLE: next he placed WILL LOOK, and finally, in a row underneath these he arranged AT YOU and read to me: "The little red apple will look at you!" After I had placed the cards correctly on the table, I gave W.L. another card with the word NOT on it; he was able to place it in its correct position. After replacing the word WILL with DID, so that the sentence now read YOU DID NOT LOOK AT THE LITTLE RED APPLE, W.L. was not able to rearrange the cards to form a question. Several other attempts with different sentences on cards produced the same result. Incidentally, W.L. was also unable to change verbally presented simply statements into questions as this shows:

(81)

HW           Let's try another one: "This room is hot."

W.L.        This room, home? room. This room (spelled it) R, O, O, M, was hot.

HW           Now, can you make that into a question?

W.L.        When the room, no, later, later the room was hot?

HW           O.K. what you did was make it into a question without doing anything else to it, you see what I mean?

W.L.        This room was hot. Later, the room was hot. [No Q intonation this time]

HW           But I want a question. I want you to make it into a question.

W.L.        I see. The heat or the hot, the question concerned the room, oh boy!

HW           Now remember the ones we just did [the model]: John hit the ball and Did John hit the ball? So now we want to do the same thing with this sentence.

W.L.        [reading his written example] This room is hot. This room is not hot.

HW           Is that a question?

W.L.        Yes, that would be a question. This room is not hot.

HW           Wouldn't that be a negative?

W.L.        Probably

Whether this indicates a difference in the nature of a permutation rule like the question transformation contrasted with an insertion-type rule like the negative transformation, or whether this has some bearing on the scope of negation, remains to be investigated. Some further evidence in support of the notion that the ability to *permute* is involved, is the fact that W.L. could not recognize the semantic identity between active and passive sentences -- *John hit the ball; The ball was hit by John* -- as seen in another part of the same interview:

(82)

- HW           Can you read the two sentences back to me now?
- W.L.         John was hit by the. The ball was hit by John and John hit the ball.
- HW           Do these mean the same thing to you?
- W.L.         At this point, yes. I can't say that they could or could not.
- HW           You're not sure?
- W.L.         No.
- HW           Usually those two sentences ought to mean the same thing, it's just a different way of putting the verb.
- W.L.         Probably they are and yet,
- HW           It's a little difficult to see the connection between them?
- W.L.         Probably, as a guess. I don't know.

However, *all* the data from W.L. is not that obvious. For example, he was given the two sentences: *John hit the ball; The ball is big* -- and asked if he could make one sentence out of them. W.L.'s response was: *John had the big. John, hit the big ball.* According to the standard theory the rule for generating prenominal adjectives requires a permutation of the adjective as the final step in the derivation. Obviously a lot more work needs to be done before a reasonable argument can be presented concerning linguistic units, transformational rules and the elementary operations of which they are composed.

There is an additional aspect of the word-cards test given to W.L. as described above, which possibly bears on semantic rules. The original purpose of having a sentence on word cards was to experiment with syntactic rules such as the question and negative. But it may be that in the first part of the test -- when the cards were given to W.L. in scrambled order and he was asked to make them into a sentence -- another linguistic problem was overlooked. It seems to me that the task of placing word cards to form a sentence involves, among other things, semantic amalgamation rules. Clearly to form a sentence in this fashion both the semantic and syntactic features of each lexical item would have to be determined, these would then have to be placed in a temporary memory

storage and finally these would have to be compared to the corresponding features of the other words on cards. The manner in which W.L. "built up" the sentence indicates that precisely such a strategy is being used. He located the words which formed the phrases, the noun phrase, the verb phrase and the prepositional phrase, as distinctly separate tasks. It is therefore quite significant that the final sentence in the case noted above violated selectional constraints on the verb *look*, which requires an [animate] subject, in forming

(83) \*the little red apple will look at you

but did not violate any syntactic constraints. This is consistent with the other data obtained from W.L. as has been noted in sections 4.0 and 4.1. The word-card test may prove to be a means for investigating semantic rules in aphasic patients.

In the preceding three chapters I have been concerned with a rather broad problem in applied linguistics which I believe not only proposes new empirical evidence for linguistics but also indicates how this evidence may bear upon theoretical issues. It was suggested that the basic distinction between linguistic competence and linguistic performance needed modification in order to make use of neurological data and a modification was offered which attempts to correlate in a very direct way a linguistic and a neurological model of language in the central nervous system. This latter is not a well-accepted assumption and the one-sided arguments in the preceding pages may not have given due credit to the opposition. The following quote from Chomsky [1965] pp. 139-140, I think epitomizes the prevailing view:

Such a description of the form of the syntactic component may seem strange if one considers the generative rules as a model for the actual construction of a sentence by a speaker. Thus it seems absurd to suppose that the speaker first forms a generalized Phrase-marker by base rules and then tests it for well-formedness by applying transformational rules to see if it gives, finally, a well-formed sentence. But this absurdity is simply a corollary to the deeper absurdity of regarding the system of generative rules as a point-by-point model for the actual construction of a sentence by a speaker. Consider the simpler case of a phrase structure grammar with no transformations (for example, the grammar of a programming language, or elementary arithmetic, or some small part of English that might be described in these terms). It would clearly be absurd to suppose that the "speaker" of such a language, in formulating an "utterance", first selects the major categories, then the categories into which these are analyzed, and so forth, finally, at the end of the process, selecting the words or symbols that he is going to use (deciding what he is going to talk about). To think of a generative grammar in these terms is to take it to be a model of performance rather than a model of competence, thus totally misconceiving its nature. One can study models of performance that incorporate generative grammars, and some results have been achieved in such studies. But a generative

grammar as it stands is no more a model of the speaker than it is a model of the hearer. Rather, as has been repeatedly emphasized, it can be regarded only as a characterization of the intrinsic tacit knowledge or competence that underlies actual performance.

If Linguistics is seriously and honestly seeking the characterization of the *actual* "knowledge" a man has of his language, then it is in fact seeking the representation of language in the human brain. To create an artificial dichotomy between an abstract linguistic model of that knowledge and the neurological structures and functions and events which *are* that knowledge, can gain nothing and may even mislead us into erroneous hypotheses. But there is potentially an even more invidious consequence of such an attitude. Someday man's understanding of the brain and its behavioral mechanisms will progress far beyond the contemporary awareness of a few biochemical properties of neurons, a rough approximation of electrical events and partially specified functions for some of the neuro-anatomic structures. And when that day arrives, the biochemist, physiologist, anatomist, neurologist and all others concerned with brain function will suddenly be in need of a specification of behavioral units that can be correlated with their information. It is difficult to see how such an intersection of sciences can be of value unless all concerned are aiming at the same goal. However, regardless of whether such an interdisciplinary correlation motivates present linguistic research or not, there is still an inescapable fact which cannot be ignored. What we say and what we hear comes from and goes to the brain; it is a product of brain structure and mechanisms -- the closer we get to the brain, the more likely we are to be discussing the realities of the structure of language.



## Notes to Chapter IV

1. The interesting question of whether features, segments, syllables or words or all four, are the neurological "primes" in the central language system is not taken up here.
2. As in all transcriptions, the periods indicate a full stop with terminal juncture as at the end of a sentence and commas indicate pauses that seem to be more of a phrasal nature. The series of dots indicates some omitted material, as usual.
3. These are probably not an exhaustive list, but will serve well enough for the explication of some linguistic hypotheses.
4. The usual theoretical assumption is that adjectives are derived from relative clauses, as in:
  - (1) the box -- the box is black -- contains a lot
  - (2) the box -- which is black -- contains a lot
  - (3) the box -- black -- contains a lot
  - (4) the black box contains a lot [by permutation]

presents some difficulty in assuming that relativization may not be generally employed in the word-to-sentence task #8, since adjectives are commonly used. It is possible to think of two or three "classes" of relative clauses, perhaps related to separate neurological computations, and that they differ in what they required of short-term memory for semantic feedback monitoring, such that the ones which result in fewer words in the surface structure are easier to do. It is possible that adjectives are not computed in this way by the central language system, that is not by the relativization process at all. Not having data that bears directly on this question, I will not speculate on which possibilities are the most plausible; it would be an interesting area for further research.

5. This must be different from the "tip-of-the-tongue" phenomena, for in this the case it is not at all clear how much of the semantic and syntactic information is present and generally some part of the phonological representation, usually the first segment or syllable, is present. Penfield and Roberts [1959] have an excellent discussion of the phenomenon of blocking the phonological representation entirely by means of a small current passed through an electrode in the brain. In their cases, the evidence is pretty good that the electrical blocking is artificially separating the phonological representation from the semantic and syntactic representations of the word.
6. Luria correlates his notion with the "inner speech hypothesis" of Vygotsky [1934]. According to Vygotsky, inner speech serves a "predicative function" in the use of language.

7. Alternatively, one might consider the indefinites *one, thing, etc.* to be analogous to cover symbols, i.e. as general lexical items representing classes of other lexical items. In this view the indefinites would be the least specified lexical items of the particular class in question. Presumably the aphasic who had lost many of the semantic features differentiating items could still make use of the very general features and thus select an indefinite from the lexicon. The primary distinction between this view and the one supported here is whether the indefinites are 'normal' lexical items or more on the order of grammatical formatives (in which case they would be phonetically specified by a late syntactic rule). The data suggests that there is a significant difference between ordinary nouns and the indefinites. In Bach's model which posits the referential index as a separate part of the NP, potentially realizable as one of the indefinites, this is naturally accounted for. In Chomsky's model which posits the referential index as a part of the lexical item, one would either have to make the distinction in an ad hoc way or else fashion a more complicated notational variant of the other model.

## APPENDIX A - Part 1 [LBVAH APHASIA CLASSIFICATION]

LANGUAGE FUNCTIONS	BROCA	WERNICKE	AMNESIC	GLOBAL
Spontaneous speech rate	decreased	increased	hesitant	Speaks like a Broca's Aphasic
Interruptions in speech	frequent	infrequent	present	
Prosody	poor	normal	normal	
Articulation	distorted	normal	normal	
Effort in speaking	present	absent	intermittent	
Amount of speech	reduced	increased	normal	
Sentences	simplified	not simplified	normal	
Word distortions	present	present	none	
Verbal paraphasias; wrong words in sentences	rare	common	none	
paragrammatism; wrong word order; omissions; misuse of grammar	not seen	present	employs circumlocution	
agrammatism; telegraphic stule	present	not present	not present	
Word finding difficulty	proportionate	excessive	present	
Dysarthria	present	not present	not present	
spontaneous writing	impaired	impaired	impaired	
Comprehension of speech	normal	impaired	normal	
Reading Comprehension	normal	impaired	normal	
Naming difficulty	proportionate	excessive	normal	
Repetition	impaired	cannot repeat	can repeat	
Oral reading	cannot	impaired	can	
Take dictation	cannot	cannot	can	
Copying ability	can	cannot	can	
Oral spelling	cannot	cannot	can	
Recognize spelled words	can	cannot	can	
Recognize by touch	can	cannot	can	

## APPENDIX A - Part 2 [GESCHWIND'S CORRELATED TERMS]

Geschwind [1967] proposed the following classification scheme in an attempt to correlate terminology used by researchers in the field.

## I. Non-fluent Aphasia:

Motor	Classic Broca's aphasia, Head's Verbal
Expressive	aphasia, Wepman's Syntactic aphasia
Howes' Type A	(hemiplegias are characteristic)

## II. Fluent Aphasia [2 groups]

Sensory	(1) Classic Wernicke's aphasia, Wepman's
Posterior	Pragmatic aphasia, Head's Syntactic aphasia
Howes' Type B	(2) Classic Amnesic or Anomic aphasia,
	Head's Anomic aphasia, Wepman's Semantic
	aphasia
	(no Broca's lesion; no hemiplegia)

[Note: Head's Semantic aphasia is a 3rd type]

More detailed treatments of the classification of aphasia can be found in: Bay [1967], Critchley [1967], Goldstein [1948], Head [1963], Howes [1967], Jones and Wepman [1967], Nielsen [1946] and Spreen [1968].

## APPENDIX A - Part 3 [PATIENTS CITED IN TEXT]

Patient	Diagnosis	Etiology
F.Z.	dysarthria	skull fracture (hematoma) in mid-brain & cerebellar region
J.W.	dysarthria	skull fracture (hematoma) in mid-brain & cerebellar region
R.H.	Wernicke	anarterectomy (right carotid artery)
L.P.	Broca	CVA in frontal region
L.S.	Global	skull fracture left hemisphere
W.L.	Wernicke	contusion (hematoma) in fronto-parietal regions
K.T.	Amnesic	trauma from bullet in parietal region
K.H.	Wernicke	CVA in parietal region
LBVAH-G.C.	Wernicke	} etiology not checked for those not personally interviewed
LBVAH-W.	Broca	
LBVAH-E.J.	Broca	
LBVAH-W.H.	Wernicke/Amnesic (jargon)	
LBVAH-G.R.	Wernicke	
LBVAH-C.S.	Amnesic	

## APPENDIX B [TESTS]

## TEST #1

- 
1. Mary is reading a book.  
I like Mary.
- 
2. Bob has black hair.  
Bob lives in Los Angeles.
- 
3. This room is very small.  
This room is my office.
- 
4. I have to go to lunch.  
It is 12:00 noon.
- 
5. The paper is on the table.  
I have the paper.
-

## APPENDIX B (cont.)

## TEST #2

---

EXAMPLE: Shirley is a little girl. Shirley's mother has to help Shirley  
(her) (her)

get dressed in the morning before Shirley goes to school. After Shirley gets  
(she) (she)

to school, the teacher takes care of Shirley.  
(her)

---

TEST: John and Susan are married. When John came home, John asked

Susan if Susan wanted to take a trip. Susan said 'yes'. So, John and Susan

went to San Francisco. It was raining when John and Susan arrived. John did

not bring John's raincoat; John had left the raincoat at home. However, Susan

remembered to bring Susan's. While John and Susan were in San Francisco,

John and Susan met Paul. Paul was an old friend who knew John and Susan

back in high school. John, Susan and Paul all enjoyed the vacation.

## APPENDIX B (cont.)

## TEST #3

1. The colonel wanted \_\_\_\_\_  
(The colonel got into the fox-hole.)
2. They expect \_\_\_\_\_  
(He will come tomorrow.)
3. Michael imagined \_\_\_\_\_  
(Michael sang the song.)
4. She approves of \_\_\_\_\_  
(She smokes cigarettes.)
5. The lieutenant expected \_\_\_\_\_  
(The lieutenant commanded the platoon.)
6. Michael persuaded \_\_\_\_\_  
(I went to the movie.)
7. The sergeant ordered \_\_\_\_\_  
(The private got into the fox-hole.)
8. John thinks \_\_\_\_\_  
(John is strong.)
9. Michael wanted \_\_\_\_\_  
(John bought the car.)
10. Sam imagined \_\_\_\_\_  
(John wrote the book.)



## APPENDIX B (cont.)

## TEST #4

John is a doctor.

Mike is a doctor.

1. \_\_\_\_\_

Sam asked the doctor for some medicine.

Sam asked the nurse for some medicine.

2. \_\_\_\_\_

This book is very long.

This book is easy to read.

3. \_\_\_\_\_

There are many cars in Los Angeles.

There are many people in Los Angeles.

4. \_\_\_\_\_

John gave his wife a present.

John gave his son a present.

5. \_\_\_\_\_

Peter bought some paper at the store.

Peter bought a pencil at the store.

6. \_\_\_\_\_

## APPENDIX B (cont.)

## TEST #4 (cont.)

Mike is a happy man.

Peter is a happy man.

7. \_\_\_\_\_

Can you read this?

Can you understand this?

8. \_\_\_\_\_

Peter likes movies.

John hates movies.

9. \_\_\_\_\_

Please give me the book.

Please give me the cards.

10. \_\_\_\_\_

The tiger caught the rabbit.

The tiger ate the rabbit.

11. \_\_\_\_\_

The lamp is very bright.

The lamp is very hot.

12. \_\_\_\_\_

## APPENDIX B (cont.)

## TEST #4 (cont.)

Peter does not like Barbara.

Barbara does not like Peter.

13. \_\_\_\_\_

Mike will not drink coffee.

Mike will not drink coca-cola.

14. \_\_\_\_\_

Multiplication is difficult.

Division is difficult.

15. \_\_\_\_\_

## APPENDIX B (cont.)

## TEST #5

1. I persuaded John to \_\_\_\_\_.
  2. John dropped the soap \_\_\_\_\_ the sink.
  3. The ball was \_\_\_\_\_ over the fence.
  4. There are \_\_\_\_\_ chairs in the room.
  5. I have a \_\_\_\_\_ car.
  6. The \_\_\_\_\_ is on the table.
  7. John and Mary \_\_\_\_\_ to the beach.
  8. I \_\_\_\_\_ he will come today.
  9. I may \_\_\_\_\_ home this weekend.
  10. My arm does not \_\_\_\_\_ anymore.
- 
1. John is very happy. \_\_\_\_\_
  2. The man loved her wife. \_\_\_\_\_
  3. The table is in the corner. \_\_\_\_\_
  4. Bob is three stories tall. \_\_\_\_\_
  5. The book is on the sad and lonely table. \_\_\_\_\_
  6. I ate some buildings for breakfast. \_\_\_\_\_
  7. John cannot spell very well. \_\_\_\_\_
  8. My car cannot see very well. \_\_\_\_\_
  9. My book belongs to John and John's book belongs to me. \_\_\_\_\_
  10. Do you want a pencil? \_\_\_\_\_

## APPENDIX B (cont.)

## TEST #6

1. argue \_\_\_\_\_
2. require \_\_\_\_\_
3. perform \_\_\_\_\_
4. conceal \_\_\_\_\_
5. speak \_\_\_\_\_
6. approve \_\_\_\_\_
7. decide \_\_\_\_\_
8. open \_\_\_\_\_
9. arrive \_\_\_\_\_
10. remember \_\_\_\_\_
11. expect \_\_\_\_\_
12. imagine \_\_\_\_\_
13. brought \_\_\_\_\_
14. arrange \_\_\_\_\_
15. contain \_\_\_\_\_
16. amaze \_\_\_\_\_
17. carry \_\_\_\_\_
18. receive \_\_\_\_\_
19. pursue \_\_\_\_\_
20. persuade \_\_\_\_\_
21. admire \_\_\_\_\_
22. wander \_\_\_\_\_
23. take \_\_\_\_\_

## APPENDIX B (cont.)

## TEST #6 (cont.)

24. write \_\_\_\_\_
25. wear \_\_\_\_\_
26. challenge \_\_\_\_\_
27. shoot \_\_\_\_\_
28. welcome \_\_\_\_\_
29. insert \_\_\_\_\_
30. found \_\_\_\_\_
31. hunt \_\_\_\_\_
32. search \_\_\_\_\_
33. refuse \_\_\_\_\_
34. sing \_\_\_\_\_
35. overlook \_\_\_\_\_
36. surprise \_\_\_\_\_
37. study \_\_\_\_\_
38. eat \_\_\_\_\_
39. lick \_\_\_\_\_
40. report \_\_\_\_\_
41. catch \_\_\_\_\_
42. smile \_\_\_\_\_
43. force \_\_\_\_\_
44. throw \_\_\_\_\_
45. walk \_\_\_\_\_

## APPENDIX C [FULL INTERVIEW WITH LBVAH-W.H.]

- ST Can you tell us your name?
- W.H. That's my only phone, and my mother's /AdEw!/, and then the bath, that's where the three vegetables are wreathed, no calendar there /kaetaek/ you know what I mean? they (mumbled) /jAnEən/, and it's double /kənfAmd/. Knife, isn't it. This other man should come back in while you will have a /kO/ on that I'm sure, I don't have it. I'll hand it to man it you (mumbled) general wretched, will that do it? Good.
- ST Now, Mr. Harris, we're going to show you some pictures of famous people and I want you to tell us who they are and whatever you know about them and whatever you can remember about them. Will you do that, please?
- W.H. All right.
- ST [picture of Jimmy Durante] Mr. Harris, who is this?
- W.H. And this is a predator or present tent receipt, very /təʃes/, /kəntəʃəd/ boy and an electric soul, and /æktrE/ for most every /kənsiʃən/ for a long period.
- ST Thank you. Now what else do you know about him? What is his name?
- W.H. Well they /trɔiv/ and very /nativ/ and makes good loyal buildings stink or /rErərdz/ any way you want to look at it but he is a good /krugər/ and as a good loose for all his position and, as plate the float of these individuals going in to the off stand (mumbled)
- ST Thank you. [picture of Fidel Castro] Now who is this Mr. Harris, and what can you tell us about this picture?
- W.H. Pang, eight, eighty, jay, see bee oh eight seven ninety seven, eight nine eight nine eight /senʃO/, he is a /kulpentər/ unpaid, advisor against our /ænθrɪpedEəm/, or /gərdmEəlɪnment/.
- ST And what is his name? Can you tell us his name?
- W.H. Jay all eighty one /AdA/ to see joy one oh seven, and that was a fond of day of theirs, it was opposed to the /UbErEən/ dintment of /taent/ and not anybody's /əjæmpərdid/ except people who, became (mumbled) in stealing.

## APPENDIX C [FULL INTERVIEW WITH LBVAH-W.H.] (cont.)

- ST And where is he from?
- W.H. Oh, it, he's got some in /kə/ Cuba and he's so badly in bomb just what he stands that there are long in Paris.
- ST Thank you. [picture of John F. Kennedy] Now Mr. Harris, can you tell me who this man is? Can you tell me his name?
- W.H. It was one eighty, one eight jay, known as nine /A/ nine /dəθE/, and it was, uh, he is a misser the /defərE/, is invaded, and after this was over, it was /ə!Antəd/ after the can these into the present mister, oh what's his name now I've forgotten, come back as his father who was, uh, after the first /wiðərtAn/ from the rural terry /prezifin/.
- ST Can you tell us what this man's name was?
- W.H. Oh federal, /foEdəl/, no, something like that, to fill his a fishing.
- ST Is that John F. Kennedy?
- W.H. Yea, yea
- ST John F. Kennedy. What happended to him?
- W.H. Well he was killed by a /ə!/ accident in a almost about lung, down into, uh, /!oEzEəsfnəs/ down there who were the americans phoning /šorfis/, and he died a matter of hours of airs that afternoon.
- ST And what was his position? What was his job at the time he was killed? What position did he hold?
- W.H. Well he was, uh, permanently a raised, at the new semmonstrations that he's holding the /kler/ and he's backing it up, worth faze
- ST Thank you. [picture of Harry S. Truman] Now Mr. Harris, can you tell us who this man is?
- W.H. In ah, he was the president, as the /rEpOt/ completing a sub/ər/, and till in the as a poison p<sup>l</sup>resident, now he is a /fərsən/ should be, I don't remember the /nAt/ of the day of the /dae:/ but all of it wasn't lived in live at the time of a glisten, so, that's it.



## APPENDIX C [FULL INTERVIEW WITH LBVAH-W.H.] (cont.)

- ST Right, he was a president. Did you think he was a good president?
- W.H. Not necessarily
- ST Not necessarily?
- W.H. he's well /afiʃent/, out and out /wətaʃin/ and, he was a /əkentEəs/, commitment, work, on second gram leading, and he was an /ədə/lection or /daemərč/ or anything else but, they did they could with what they had but, it wasn't original /bAzəl/ /mæθə/.
- ST O.K. Anything else you'd like to say about this man?
- W.H. No, I uh, we won't talk about it.
- ST Fine, thank you. [picture of General Douglas MacArthur] Now, can you tell us who this man is, I bet you know him.
- W.H. Yea, I worked for him as a, while he was a president, of this /lo/city, to over quarrel a recently /nAtəd/ ones, that's when he was ordered out of the Philippines, under /grawθorθObAsən/, and went back to North America, not America, South /pəzæʃən mæəsəjəs/ brought this representative and his appeared up with all medals for returning every receded, and his name is General /mæ/, his next one what's his called there is, uh, all /timOdin/ size or /testEz/ for medal or /frAtəlE/.
- ST And what do you think of his ability? Was he a good general?
- W.H. very very good but there's some they had often bad /emərəns/ for and against it, and, their own good a pair, but, there's other people just as /riqE/ about it as somebody else too.
- ST Do you think he would have made a good president?
- W.H. Oh yes, yea I think so, his uh, well you can put your /arθ/ into the thing and say he was the only man that could do it that isn't true, uh the ignorant take use and it is your husband goes to the side and he can put it in it just it has to be the other president man in (mumbled) beat up, but he has a lot of /peləjE/, he has a lot of /peləjE/, and they over checked him right in here no one if he didn't have anything else why his, his /sIe/ wouldn't, overalls wouldn't look like it overly paid a little bit too much, you know? Let's go down and, uh, they'll say they never find a /ʃɪd/ and /barč/ here like the rest of it, when I hunt these and let 'em come along and pass it down 'cause it's /presolAʃəsə/.

## APPENDIX C [FULL INTERVIEW WITH LBVAH-W.H.] (cont.)

- ST They say he designed his own uniforms; did you read that?
- W.H. Yes, I've /hərnd/ of those I've, this is one of them here, but on the other hand, but any nobody at all didn't put more /kolabər/ in here, from here to here, and it not that /didriʃ/ to play with, 'course that's /θətapE/. He did very good.
- ST O.K. Thank you. That was MacArthur again?
- W.H. The man show the man and if we /ədabʃən/, half of the olive and the navy and all other people, you either do it the way he says or they don't pay, that's not true, we take in the /nigrisəsmonA/ that there is, he's gotta produce the things and he can come from all old /kərpədE/ and keep them all irregardless of where they came from and leave it that way, driving may /biʃ/, but he here he *impawned* this all his property, but there's no need of me putting it in there but that is a very poor one there as spread for could stick out to here. doesn't that look funny? that's all I've got to say about it.
- ST Mr. Harris, your wife is a former teacher; is that right?
- W.H. ummhmm [agreeing] -- my wife is uh, she was bored in uh, we were born in uh, she in /int/ it in first nineteen, let's see we quit in, uh, I'd have to look that peg to get it, but as of right now she's about twenty-eight months, fifty-eight.
- ST Is she still teaching or does she do some other kind of work now?
- W.H. oh yes, and she did, at first she taught English entirely then she /spO/ /peld/ in /bartmənz/, after she left the /dAvE/

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