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Aphasia: Some neurological, anthropological and postmodern implications of disturbed speech

Doody, Rachelle Smith, Ph.D.
Rice University, 1992

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APHASIA: SOME NEUROLOGICAL, ANTHROPOLOGICAL AND POSTMODERN IMPLICATIONS OF DISTURBED SPEECH

by

Rachelle Smith Doody

A THESIS SUBMITTED IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE DOCTOR OF PHILOSOPHY

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1992
ABSTRACT

Aphasia: Some Neurological, Anthropological and Postmodern Implications of Disturbed Speech

by

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This work begins by examining the history of aphasia studies, placing them in the context of historically concurrent theories about speech and language. The historical analysis can be read as a deconstructive incision into contemporary discourses which use information about language to make inferences about brain functioning or thought processes. A deconstructive critique of aphasiology and those sciences upon which it is built, including linguistics and localization theory, suggests that aphasia is constructed artificially so that it cannot be localized or explained by brain mechanisms.

Anthropological influences in this work inform the style of analysis as well as the range of inquiry. Situated in postmodern anthropology, the thesis includes an investigation of positioning: positioning of the author within medicine (neurology) and anthropology; and positioning as a phenomenon brought about by certain sets of practices. Among these practices are those related to the scientific method and those related to more interpretive or hermeneutic strategies. Several controversies within anthropology are related to the clash between science and not-science, including feminist and postmodern debates. Practices, which are situation-dependent,
are not as conflicted as theories are and provide reasonable ways to separate sense (or meaningfulness) from non-sense (or artifacts) in daily life and work.

Related to questions of method and interpretation are questions about "data." What count(s) as data? Should units of significance be predetermined, or discovered in the process of investigation? How do standardized methodologies or interpretive expectations shape the outcome of clinical, scientific, and anthropological studies? A narrative style is employed to discuss these questions by telling particular stories involving research and publication: case reports in neurology; semantics of sentence accent in Alzheimer's disease; and fieldwork in northern Thailand concerning non-literacy and its effects on cognitive processes among Karen hilltribes. These disciplinary projects are contrasted and data creation discussed.

What began as an examination of the history of aphasia studies concludes in discussion of aphasic speech as an example/critique of postmodern and anthropological discourse. Practices that cluster around the study of aphasia, particularly those involving living patients, provide useful critiques to scientific, anthropological and postmodern theorizations.
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This work began as a rather straightforward examination of the history of aphasia studies, with the intent of understanding some current limitations in the field. Despite a recognition since ancient times that brain injuries and disturbances resulted in disturbances of speech, we really do not understand the relationship between speech and the brain, either anatomically or functionally.

Those who study human speech also have a tendency to talk about "language" instead of "speech", despite the fact that speaking is what we actually do. Particularly in medical fields, speech is considered a mechanical process, the motoric output of thought accomplished through the coordination of various anatomical structures that produce and modify sound. Language refers to a vast array of concepts including national tongues, written texts, literature, and, vaguely, that "faculty" which must somehow be localized in the brain or its processes.

Although my training was in neurology and neurolinguistics to begin with, this investigation took place in the context of anthropology rather than medicine. The anthropological context accounts for some differences in topic and approach which may be particularly noticeable to practitioners of medical science. The Historical Approach to the Definition of Problems in Aphasiology considers primary neurological texts, but places them in the context of
concurrent writings about language from outside of neurology. It is not an exhaustive analysis of language theories, but attempts to determine outside influences which affected medical notions about what language is. This analysis also suggests some assumptions that affected the field of aphasiology, and some ideas about language which did not have strong effects within aphasiology despite widespread influence on other ideas of language. This historical analysis could also be read as the incision point for a deconstructive critique of aphasiology, as it begins to suggest that the whole field is built upon assumptions and distinctions that would ultimately foil any attempt to explain how language is localized or produced in the brain.

Other anthropological influences on this project have to do with current controversies within anthropology, and how the practice of medicine or the study of disturbed speech and language might inform such controversies. One conflict concerns whether anthropology is primarily scientific or humanistic, art or science, and how it should therefore be practiced. Sense and Nonsense: Dissemination and Empiricism in Practice discusses these issues and suggests that various critiques within anthropology are related to this science versus non-science question, including the postmodern and feminist critiques. In this piece I also suggest that practices are not as divided by dichotomies as theories are, and that, for example, practicing medicine is not as theory-
driven as one might think. Practices are dependent on some way of sorting out sense from nonsense, which is a general way of saying that practices, as I use the term, have to do with deciding what is meaningful under the circumstances. If we want to avoid reifying practices into a set of rules or principles, and I do, we also have to take into account who is involved and what his or her biases are. This is where anthropological concerns about reflexivity and rhetoric are useful. Neurology and Anthropology Enabling Practices was initially presented at a panel of physician anthropologists discussing how both disciplines and training processes affected their work. It is self-reflexive and in Modernist terms, "revealing" about the author. At the same time, it may accomplish that distinctly post-Modern aim of positioning her so that her knowledge claims are situated rather than general. This paper also explores the notion of multiplicity in individual identities, a fact of life but not the usual trope of literature.

Closely related to questions about knowledge claims is the question "What counts as data?" Obviously, this varies between disciplines and between individuals within disciplines. And it is mainly an academic question, since "data" are not important to most everyday practices. The purpose of Data Stories is to explore the intuitions, rules and regulations, and outcomes of generating data in different kinds of projects. It includes a distinctly anthropological
project, the study of nonliterate cognitive approaches among the Karen hilltribes in northern Thailand; and a more neurological project, the semantics of sentence accent in patients with Alzheimer's disease.

What began as a traditional examination of the history of aphasia will then conclude in an essay called Aphasia as Postmodern (Anthropological) Discourse. Postmodernism is, admittedly, a controversial set of concepts and descriptions that has affected many disciplines, particularly the non-science ones. Although powerfully descriptive and, for me, indispensable to the process of making sense in most circumstances, postmodernisms have not grappled as well with practices as they have with descriptions. Perhaps we should conclude that postmodernisms are therefore best kept withing hermeneutic disciplines, like philosophy and literary criticism. But it is foolish to try and "keep" them anywhere. We are all the postmodern condition, and all of our practices are affected by this acknowledgment, including scientific ones. My hope is that various situated practices will provide interesting and useful critiques of postmodern descriptions, beginning with those practices that cluster around the study of aphasia, speech and language.
INCISION

"...the incision is neither the incised integrity of a beginning, or of a simple cutting into, nor simple secondarity."

"The incision of deconstruction, which is not a voluntary decision or an absolute beginning, does not take place just anywhere, or in an absolute elsewhere. An incision, precisely, it can be made only according to lines of force and forces of rupture that are localizable in the discourse to be deconstructed. The topical and technical determination of the most necessary sites and operators—beginnings, holds, levers, etc.—in a given situation depends upon an historical analysis. This analysis is made in the general movement of the field, and is never exhausted by the conscious calculation of a 'subject'."

-Derrida, Positions
AN HISTORICAL APPROACH TO THE DEFINITION OF
PROBLEMS IN APHASIOLOGY

Aphasiology, the study of disordered language due to brain disease or dysfunction, might constitute a "universe of discourse" as Edward Sapir defined the term (Sapir, 1957:182). It is an area with an "excellent terminology," that has "more or less self-contained and self-consistent principles, and some insight, however tangential, into a highly selective phase of human behavior." The precepts that guide aphasia studies are fragmentary, especially viewed in the context of broader language theories, and "philosophically arbitrary;" two qualities that Sapir accepted as inevitable. Sapir lamented that we cannot rely on these self-perpetrating domains to describe real human experience, since the operational terms contained within them provide an inevitable and arbitrary cast to all perceptions.

It is my contention that aphasiology has ever-increasing difficulties as a universe of discourse for several reasons. First, like all subspecialites of neurology, it relies heavily on observations of human behavior and the classificatory systems and explanatory paradigms that have been developed for aphasia often do not agree with clinical characteristics of
patients. Scattered observations about speechlessness that began in antiquity accumulated until, in the late eighteenth and early nineteenth centuries, they were forcibly fitted into an anatomical model for speech. Although the best research efforts of the time supported localization of some speech deficits, the model was used to systematize all reported deficits and to construct a paradigm for normal speech. This same anatomical model is used today, with few changes since 1900. A second reason for the inadequacy of current aphasia discourse is that basic neuroscience information and shifting scientific epistemes have created more obvious gaps between information about brain processes and observations of behavior. The nineteenth century model for speech cannot encompass theories or even isolated information about neural nets, long-term potentiation, and brain metabolism, or pathological data about the organization of the cortex. Since current aphasia terminology and assumptions do not describe aphasic behavior in a clinically compelling way, current neuroscience information that deals with how the brain organizes behavior cannot begin to be related to aphasia studies. Additionally, aphasia studies have borrowed heavily from linguistics and have therefore incorporated many arbitrary and ultimately self-destructive principles from that field. Such studies have taken place alongside the dissemination of literacy and literate approaches to cognition and have included many literate biases in their theories,
models, and conclusions.

We will begin by reviewing the body of information that has built up about aphasia, from some brief observations dating several hundred years B.C. through 1950 or so. Although a number of surveys have dealt with different periods in this time interval (Benton, 1964; Benton and Joynt, 1960; Head, 1926; Marx, 1966; O’Neill, 1980), my account will be somewhat comprehensive in order to show the forces that went into development of the field. It will also be selective by focusing on specific behaviors or signs or symptoms, and specific features from the works of major authors notable for their personalized, historically conscious approaches to the field and for the great length of their productions. I do not follow the writing conventions of either history of science or sociology of science accounts. These approaches would be more systemic in describing all of the individuals, institutions and ideas that were involved in any period of the history of aphasia. Although my decision to discuss only those individuals, works, or institutions which seemed crucial to a given change in thought or perception involves selection, I feel it is justified for two reasons. First, I cover many primary aphasia texts in the course of this review and maintain my focus by deleting everything not essential to describing the changes in subject matter and method represented in these texts. Secondly, the writers I discuss from outside aphasiology are generally acknowledged as having
been influential in their historical times, and these writers were also cited by or associated with the aphasiologists in their own writings. My approach, thus, leaves much room for detailed investigations of specific individuals and the historical influences on their thought and careers.

After I describe my view of the history of aphasia, I hope to show, by looking at language theories concomitant with the aphasia writings and at certain elements of Standard Average European languages, some of the assumptions that have gone into the creation of this discourse universe. Nineteenth century linguistics and psychology together with Darwin's evolutionary theories helped to root speech and language in brain structure. Distinctions raised by Saussure, Sapir and Whorf did not influence their contemporary aphasiologists.

Finally, I hope to lay the groundwork for a later discussion of the fact that aphasia studies in the second half of this century have dealt largely with brain imaging techniques or comparisons between groups of patients, rather than with the broader issues defined by our predecessors. Aphasiology has become a smaller universe of discourse intersected by cognitive psychology, neuropsychology and linguistics at precise points. Yet, aphasiology and aphasic behavior can be successfully studied by the methods of interpretive anthropology, which may, perhaps ironically, connect theoretical constructs with biology and individual experience.
PART 1: HISTORY OF APHASIA STUDIES

Early Observations About Speech

By the year 1800, most of the clinical forms of aphasia currently recognized were already described (Benton and Joynt, 1960), but there was no order to these descriptions. In fact, they began as descriptions of human affliction that did not distinguish between brain and vocal cords, speech and language, physical and psychological. Early documents, like the Edwin Smith Surgical Papyrus (18th century B.C.) and the Hippocratic Corpus (384-322 B.C.) described speech problems in terms of agency - the ability or loss of ability to speak; and associated speechlessness with other conditions, such as apoplexy, epilepsy, and protracted illnesses (Benton, 1964; O’Neill, 1980). Analysis of types of speechlessness or descriptions of disordered speech other than dysarthria did not occur, suggesting that speech was considered either normal or absent, with no separate category for noncommunicative speech attempts.

The distinction between speechlessness due to motor paralysis involving the tongue and speechlessness due to some high level behavioral disturbance did not clearly exist until the sixteenth century when Johann Schenk von Grafenberg mentioned it in his Observations (Benton, 1964). Von Grafenberg speculated that the central, brain-related type of
speechlessness was due to a problem with memory. Throughout the Renaissance, writers added more and more precise observations about specific speech difficulties; for example, paraphasias, alexia without agraphia, and preservation of serial speech. Notably absent were any descriptions of patients who could speak fluently, but had impaired comprehension, which suggests to me that comprehension was not viewed as an aspect of speech. Since evidence exists that senility or dementia was a recognized entity and no doubt associated with comprehension problems then as it is now (Fogel, 1989), comprehension must not have been distinguished from intellect; and patients with comprehension problems were probably perceived as senile or insane. We will return to this question later when discussing Wernicke's contributions to aphasiology.

By the mid-eighteenth century, Morgagni's autopsy studies added to data like von Grafenberg's supporting central causes of speech and language problems (Benton and Joynt, 1960). Even so, no one correlated disease of the left hemisphere with the occurrence of these problems. Although most of the information accumulating was descriptive, a few theories arose. Johann A. P. Gesner, in 1770, described jargon aphasia, characterized by the production of neologisms and incomprehensible speech. He is credited for advancing the first linguistic concept of abnormal speech: that some disorder of verbal memory leaves the patient unable to
associate images with their linguistic signs. A strong desire to express something then leads to inaccurate pairing of image and sign, and a paraphasia or neologism results (Benton, 1964). Over 100 years later, Saussure almost seemed to begin where Gesner left off, although there is no evidence that Gesner’s influence was widespread, even in his own time. Toward the end of the eighteenth century, Franz Gall began to advance his faculty theory of brain organization. He said that brain matter was divided into three groups of organs: those concerned with the exercise of vital force; the inclinations and affections of the soul; and the intellectual qualities of the mind (Head, 1926). He further noted that people with prominent eyes had good verbal memories, and that soldiers with retroorbital injuries had diminished verbal memories; thus, he concluded that the language faculty was in the frontal lobes.

Summarizing so far: ancient observers noted that some people lost their powers of speech. Over time, others commented on abilities that were lost and abilities that were preserved, sometimes noting associations between the two and thereby creating entities; e.g., alexia without agraphia. The particular abilities or qualities of speech described were subjective and nonsystematic. The theories that arose to explain different patterns of difficulty were likewise dependent on what particular patients and manifestations happened to be observed. It is unlikely, for instance, that
Gesner would have come up with the same theory of mismatch between ideas and linguistic signs if he had observed slow, effortful speech instead of fluent jargon aphasia. Overall, these observations were guided by an Aristotelian assumption that reason was made manifest in language, and that language-related phenomena should be logically related as well. The relationships were sought and the deficiencies noted: that a patient could read, but not write; that another could say nothing spontaneously, but could recite a series such as the months of the year; that he/she meant to say one word but chose another, related to the right one by sound or meaning. Gall proposed that particular intellectual and moral faculties occupied organs in the brain. The list of observations continued to build up, haphazardly, until the nineteenth century when an academic debate resulted in Broca’s nearly inconceivable contention that the brain was functionally asymmetrical, and the era of localization began.

Broca, Wernicke, Jackson and the Nineteenth Century

Broca, in 1861, published a paper where he concluded that articulate speech was localized to the frontal lobes. Evidently, this is not what he had expected. Bouillaud, who rejected Gall’s conception of faculties, but did try to localize various functions based on autopsy data, also believed that language was in the frontal lobes, but could not
prove it with his data (Head, 1926). Broca, a general surgeon, was also an anthropologist noted for his comparative studies between species and races, and for his libraries of preserved brains. At a meeting of the Anthropological Society in Paris, Broca presented his data that related intelligence to brain size. He was challenged by Aubertin, Bouillaud's son-in-law, who maintained the localization view, and insisted that brain mass was less important than relative size and configuration of specific areas. Aubertin again asserted that language, for example, resided in the frontal lobes. Over the next few months, Broca hoped to end the discussion by autopsy of his patient, Tan, an aphasic who had been examined by both Broca and Aubertin in the months before his death (Berker, 1986; Critchley, 1961).

In his 1861 paper, Broca published the case of Tan, a gentleman who first developed difficulty speaking at age 30 (Broca, 1861). At 40, he had right arm weakness which progressed over several years to include the right leg. Caretakers remembered hearing that his intelligence was intact in the beginning, although this seems vague. Tan lived on in an institution until age 51, when Broca saw him for the first time for gangrene of the leg. He had "intellectual impairment," as judged by Broca, but could answer some questions with gestures. Speech was limited to "Tan," and one expletive. At autopsy, Tan had extensive brain injury due to chronic infection that involved the skull and meninges and
much of the left hemisphere; an abscess "the size of a hen's egg" was found involving the second and third frontal gyri, the insula, corpus striatum, and superior temporal gyrus. Based on this case, Broca conceded to Aubertin that the frontal lobes seemed to be involved in Tan's problem. He was not influenced against the decision for localization by the extensive brain damage that was also present.

In this paper, Broca distinguished between the "general faculty of language," the ability to establish a constant relationship between an idea and a symbol, and "aphemia," the condition in which a cerebral lesion abolishes articulate speech. He avoided the term "aphasia," championed by Trousseau, although posterity has adopted Trousseau's term, even in referring to Broca's work (Broca's aphasia). Because he inferred that Tan's intellectual functioning was intact, by history and by the fact that the patient could gesture, Broca presumed that his general faculty of language was intact as well. Broca felt that the "emission" apparatus (brain, nerves and muscles necessary for articulation), the "receptive" apparatus (sensory organs, nerves, and part of brain necessary to receive speech), and the "general faculty" of language all had to be intact for normal speech. It is important to note that in this paper, only the "emission" apparatus was presumed defective, and Broca thought it was defective due to frontal lobe disease. On this point he also disagreed with Trousseau, who thought that memory and attention were almost always
affected in aphasics (Henderson, 1990).

Broca set out to study the problem further. In 1863, he reported 25 cases of aphemia with lesions in the left hemisphere, and all but one lesion included (but was not limited to) the third frontal convolution. The patients' language was not tested systematically, but all had a speech problem that involved articulation. Broca then reported, in 1864, Duval's descriptions of two patients with traumatic language problems secondary to left-sided head injuries. Finally, in 1865, he published his best-known paper, which established the third left frontal convolution as Broca's area, the seat of articulate speech (Broca, 1865; Berker, 1986). First, he cited anatomical evidence that speech was localized in the left hemisphere. He mentioned several cases; described the fact that these patients usually had right hemiparesis, indicative of left-sided lesions; and mentioned the fact that several patients with similar lesions in the right hemisphere were not aphasic. He also cited earlier unpublished observations by Marc Dax that associated speech problems and left hemisphere disease (1) and work by Vulpian and Charcot showing that lesions of the right hemisphere occurred with equal frequency, to rule out a chance association.

Broca had to overcome the prevailing belief, considered a fact of physiology, that anatomically symmetrical structures were functionally identical (Berker, 1986). He acknowledged
that this was true for most organs. Yet, most people are right-handed, and the right hand is usually stronger than the left, regardless of occupation; both facts seemed to suggest dominance of the left side of the brain. If people acquired right-handedness by imitation alone, there should have been an equal number of remote societies in which everyone was left-handed and, despite his studies of other cultures, he knew of none. Finally, he mentioned that in Gratiolet's studies the left side of the brain seemed to develop before the right which might be the reason for its motor and language dominance.

Broca knew that articulation itself involved both sides of the nervous system, from brain to vocal cords; however, aphemia, which involved linking of intellectual phenomena to articulation, must be left-dominant. He supposed that a small number of people would have right language dominance, since there was an acknowledged group of left-handed people who were presumably right brain dominant. Because speech comprehension seemed to be intact in his patients with aphemia, he inferred that the general capacity for language was either bilateral or resided in the right hemisphere. The intellect was somehow damaged in aphemia because adults could not reacquire normal speech after severe left brain injury, despite preservation of the right side and of the general faculty of language. Thus, Broca acknowledged at least three mental phenomena related to language, and ascribed them different pathophysiologies:
articulate speech, in the third left frontal convolution; the
general faculty of language, bilateral or right hemisphere
dominant; and general intellect, which he does not localize.

Broca focused evidence from many areas to reach his
conclusion that the cerebral hemispheres functioned
asymmetrically with respect to language. He never suggested
that the left hemisphere subserved all language functions;
only the linking of intellectual phenomena to articulation
which, when injured, resulted in aphemia. The clinical
features of his cases are hazy, and the pathology incompletely
documented because he sought the least common denominators and
not complete descriptions. Broca’s findings linked him
inadvertently to Gall, who had argued for the localization of
mental faculties in an attempt to make them biologically
based, rather than philosophical entities. It is not
surprising that Broca’s work was taken up by those
neurologists interested in mapping out brain functions, or
that subsequent language studies sought anatomic localiza-
tions. However, this anatomical approach to language has been
retained despite much evidence to the contrary. We will now
turn to Wernicke and then to Jackson to see how the
localization and nonlocalization approaches were reformulated
in light of Broca.

Carl Wernicke had a diverse background, having been a
surgeon’s assistant in the Franco-Prussian war, an
ophthalmology fellow, and a physician in residence at a
psychiatric unit (Geschwind, 1967). Most importantly for his theories about language he spent six months with Meynert, the pathologist who "brought order to the fiber systems in the brain (Geschwind, 1967)." Meynert’s descriptions of the projection pathways from cortex to lower brainstem centers, the association pathways connecting areas of cortex in the same hemisphere, and the commissural systems that connect corresponding areas in the opposite hemispheres, form the basis for Wernicke’s connectionist theory of the language system.

As mentioned above, comprehension difficulties were not related to speech disturbance prior to 1800. Bastian and Schmidt did describe patients with disturbed comprehension in the late nineteenth century. Although both were later cited by Wernicke, it is not clear to me if he knew of these reports prior to his publication, in 1874, of "The Symptom Complex of Aphasia. A Psychological Study on an Anatomical Basis," (Wernicke, 1874; In: Proceedings from the Boston Colloquium for the Philosophy of Science, 1966). At any rate, his work with psychiatric patients and his interest in language led Wernicke to distinguish people who lacked language comprehension because of some focal brain pathology from people with dementias and other psychiatric diseases, who had different comprehension problems. Although there were several English "diagram markers," including Bastian, Ogle, and Broadbent, who sought to explain aphasias by drawings linking different
speech functions, we will focus on Wernicke and the German school.

With Meynert's model in mind, Wernicke acknowledged certain elementary psychic functions, such as vision, smell, and tactile sensation, that he assigned to various areas of the cortex. All other psychic functions he preferred to think of as products of association tracts connecting different areas of cortex. Memory images consisted of "movement images" left behind in the cortex itself, and were divided into primary and secondary types: primary occurred after a reflex-like event that originated in subcortical areas and traveled to the frontal cortex; secondary memories were for movements generated in the cortex independently of external stimuli, and were more goal-directed. Wernicke thought that memory images were linked to each other in facilitated pathways. He viewed speech as one instance of voluntary movement.

Primary, reflex speech movements were the mimetic attempts of infants, and originated in brainstem structures. Sound images conveyed from the brainstem to the cortex as a result of this reflex activity remained as sensory and motor images, in the temporal and frontal regions, respectively. In Wernicke's view, secondary, voluntary speech movements or consciously uttered words would later be "innervated" by the sensory and motor images thus created. These image-containing areas of cortex are the basis for his now famous drawings which show an anterior speech area, responsible for the motor
aspects of speech, connected by a subcortical bundle of association fibers (the arcuate fasciculus) to a posterior region, which is responsible for speech comprehension. Before discussing this model further, we should look at Wernicke's own review of the localization theories for speech.

He mentioned that Broca's area, the posterior third left frontal convolution, is clearly involved in many cases of speech disorder, but not in all of them. Further, many cases fit with Broca's descriptions of the clinical findings, but some did not. Meynert had shown that auditory projections ended centrally in the claustrum or insula and that disease there resulted in aphasia, although Meynert did not describe a clinical syndrome. After reviewing these data, Wernicke generalized the region responsible for producing aphasia to the entire peri-sylvian region and underlying grey matter of the insula and claustrum. Although Wernicke initially thought that the fibers connecting movement images and sound images ran in the insula, he later revised this, after Monakow, to the arcuate fasciculus (Geschwind, 1967).

Let us now summarize Wernicke's speech circuit: sounds travel from the medulla to their central termination in the temporal region where they are stored as sound images. This region connects to Broca's area, where motor images are stored, and Broca's area projects to the medulla again, to the motor nuclei controlling articulation. If the sound images cannot get to their central destination in a developing child,
he will be mute, and never acquire the images necessary for speech. A lesion at the same place in an adult will only cause deafness (2). Wernicke used this information to prove that speech arises from perceived sounds, not from concepts that are present in the brain due to other sense impressions, as Steinthal had maintained. According to Wernicke, if Steinthal were correct, blind people ought to be mute, as they would have a paucity of concepts to give rise to speech. In Wernicke's view, reflex imitative speech allows the acquisition of words, and only later are concepts attached to the words.

After this background, Wernicke described his three now classical types of aphasia. The first, which bears his name, is due to a temporal lobe lesion: sound images are destroyed, so the patient does not comprehend or repeat spoken words; concepts and other sensory images are intact and lead to the production of fluent speech; but the corrective function of listening to one's own words is gone, so the speech sounds abnormal and writing, also under the direction of sound images, is impaired. Reading comprehension varies, depending on how dependent the individual is on sound images - less educated people are generally more dependent. The second type of aphasia, due to an interruption between the temporal and frontal regions, today called conduction aphasia, spares comprehension and fluent speech. The patients lose some ability to self-monitor, so they may choose wrong words. They
cannot read letters or write, since both require utilizing the pathway from visual to sound to motor images, but can read words because the pathway from concepts to the motor area is intact. Finally, patients with damage to Broca’s area can say and write little, since the writing area is close to the motor speech area. Oral and written comprehension are intact.

Wernicke then analyzed 10 cases, utilizing his classification system. He described a more or less consistent syndrome in which comprehension is impaired and speech is fluent, although confused: the so-called Wernicke’s aphasia, which had never been characterized before. In one such case, there was softening of the first temporal convolution at autopsy. Otherwise, his descriptions did not clearly fall into one category or another, and many would create diagnostic controversy if presented today. In fact, the difficulty he had fitting cases into the three syndromes has only grown over the years. Further, the clinical elements he used to define the three groups have had to be modified; for example, Broca’s aphasics do not enjoy normal comprehension and Wernick’s aphasics, even highly-educated, have trouble with reading comprehension. Anatomical exceptions have accumulated, showing diverse locations for similar syndromes, such as several lesions in the right and left hemispheres that can be associated with conduction aphasia; which, incidentally, is now defined on the basis of an inability to repeat, and would rule out one of Wernicke’s own cases.
Wernicke clearly attempted to carry language localization further than Broca did, and to be guided by careful, pathologically based principles, although Head pointed out that his use of clinical cases was not always careful (Head, 1926). His major contribution, in my view, was in characterizing language comprehension problems pertinent to aphasiology. However, his conclusions are difficult to accept in the light of actual patients suffering from aphasia who seem to defy the categories. Even so, his general approach was expanded and continued as the best alternative available to those investigating disordered language and the brain. Electrical stimulation studies done in the 1870's by Fritsch and Hitzig and Ferrier seemed to confirm the idea that sensory and motor functions were localized in animals (Head, 1926), and helped Wernicke's model to gain acceptance. Although his contemporary, Hughlings Jackson, proposed alternative approaches, they were even less clinically useful and ultimately sidelined in the chronicle of aphasia studies.

Jackson, like Wernicke and Broca, was steeped in an atmosphere of brain localization theories. His teacher, Brown-Sequard, was a student of Bouillaud's in Paris and Bouillaud, as we discussed above, was a major proponent of the localization of language to the frontal lobes (Greenblatt, 1970). In fact, Jackson began his career in aphasiology by describing a left middle cerebral artery syndrome with right hemiparesis and aphasia, due to cardiac embolization in
patients with valvular heart disease. Although he cited Broca in this 1864 paper, Jackson already held certain philosophical differences that influenced his observations and set him toward a different course.

To begin with, Jackson talked about "language," as well as "speech" and viewed both as part of psychology rather than as physiological acts. Unlike Wernicke, he resisted giving particular localization to the "memory of words" because he wanted to keep psychic entities separate from physical descriptions. He defined articulation as the activity of the lips, tongue and palate; voice, as the product of vocal cord function; and speech as the role of the brain in speaking (Jackson, 1864; In: Head, 1915). Broca's patients, Jackson pointed out, had aphemia, a problem with speech; but their ability to use signs was intact since they were able to write. Broca did not systematically test writing or anything else in his patients, so this involved some inference on Jackson's part. Nowhere did he mention Broca's distinction between articulate language and the general faculty of language. Jackson wrote that his own patients had more than just aphemia. They could not express themselves properly with any system of signs, including words, and this was a function of a more pervasive loss of language, as Jackson used the term. Jackson's patients were probably no different from Broca's, but he examined them more thoroughly. Despite the fact that he made this distinction between speech and language, Jackson
usually talked about inner and outer speech, not about language, and did not treat them any differently.

Like Broca, Jackson initially struggled with the idea that the brain should be a "double organ," with functionally identical sides. Because he continued to look at aspects of language function other than articulate speech, Jackson never gave up including both hemispheres in the physiology of language, although he did eventually assign them different roles. Henry Head pointed out that Jackson's method was entirely different from Broca's: instead of testing hypotheses with pathological verification, he tried to classify mental phenomena, which led to a classification of defects of mind, which led to classification of defects of the nervous system (Head, 1915). Jackson emphasized that there were both negative symptoms, due directly to brain damage and positive symptoms which arose from non-damaged areas of brain, in all patients. With this background in mind, we will now go on to discuss Jackson's view of normal brain functioning, then his classification of the affections of speech.

Jackson believed that healthy language was divided into two kinds, emotional and intellectual. This distinction is similar to Gall's notion of natural language and the language of arbitrary signs, which he of course assigned different localizations in the brain. Greenblatt credits Spencer as Jackson's source for this differentiation (Greenblatt, 1970); but, as we will see later, Jackson's debt to Spencer is
probably overstated, as is Head's contention that Jackson was "neglected" because of his Spencerian phraseology. Jackson also distinguished "superior" and "inferior" speech. He thought that the basic unit of speech is the proposition, and that the loss of speech was therefore the loss of ability to propositionize. We do not speak or think in words or signs alone; the words refer to each other in particular ways and lose their individual meanings within sentences. Internal speech and external speech, which is just internal speech passed through the articulatory apparatus, were both propositional. Although Jackson thought the left hemisphere was essential to produce articulate, propositional speech, he thought that the right hemisphere was associated with the automatic service of words. This duality was necessary to account for the fact that some speech is preserved in aphasics, as we will see shortly.

The affections of speech were a little more complicated. All of them left the patient with "lame thinking" because he could not propositionize material, especially new material. Since we think in images as well as in words, his/her image-thinking would usually be intact. If images were also affected, the patient had "imperception" and an impairment of general intelligence in addition to his lame thinking. Although he originally used the term "aphasia," Jackson had abandoned it by 1868, when he wrote about his two classes of speech defects (Jackson, 1868; In: Head, 1915). Class 1 is
characterized by severe speechlessness, inability to repeat, and some degree of intact comprehension. Patients may retain a few words without propositional use. Class 2 patients say many words with many mistakes, including their attempts at repetition, yet comprehension is extremely poor. Patients in both classes have difficulty with reading and writing, and more difficulty with voluntary behaviors than automatic ones (Jackson, 1868; In: Head, 1915). These two classes seem to include "Broca's aphasia" on the one end and "Wernicke's aphasia" on the other end of the spectrum which they constitute. They are vague enough to include most patients, without the definitional problems posed by Wernicke's classification system. Jackson never mentioned Wernicke by name, but did discuss "pure word deafness," as formulated by Wernicke, in one of his later papers, suggesting familiarity with his work (Jackson, 1893; In: Head, 1915). Finally, utilizing the distinction between emotional and intellectual speech and his own adaptation of Spencer's theory of dissolution (to be discussed in detail later), Jackson looked closely at the speech that was retained in aphasics and developed a classification system for these non-propositional "speech residues," which includes recurrent utterances and occasional utterances. First, he noted that certain patients repeat "stock phrases," and he provided anecdotal evidence that these phrases were the leading "sensorimotor processes" the patients had held in mind at the time of their strokes.
Next, he argued that in all cases of brain damage, behavior is returned to a more automatic, less voluntary level; for example, patients following a convulsion initially show reflex movements only. In voluntary speech, a patient must call forth words and make propositions, an active process; whereas, to receive speech that is already cast in propositional form is a passive process. "Speechless" patients then retain various automatic expressions (Jackson, 1874; In: Head, 1915). He calls some of these recurring utterances, which consist of: jargon speech, words with emotional intonation but no propositional meaning; phrases, also without propositional meaning; and "yes," "no" or both. Occasionally patients retain the propositional, meaningful use of yes and no, which Jackson says are at the "bottom of intellectual language and at the top of emotional language." In addition to recurring utterances, patients may retain automatic use of another category of language that appears propositional, but is involuntary: these so-called occasional utterances often occur in emotional situations and may take the form of oaths or interjections; inferior speech like "merci;" or even a brief burst of appropriate speech like "How is Alice?" Both categories of automatic speech were considered "positive symptoms" of brain damage, and Jackson thought they represented release of more automatic behavior from inhibition by higher centers.

Regarding the anatomy of speech, Jackson made an analogy
to chorea, in which the cortex adjacent to the corpus striatum has to be involved in controlling motor activity, since cortical destruction leads to choreic movements. He, therefore, assumed that the cortical regions near the corpus striatum also direct the coordinated, voluntary, and educated movements of articulation and, thus, must be damaged in his Class 1 group of aphasics. The lesion in Class 2 patients was "not necessarily in the region of the corpus striatum (Jackson, 1866; In: Head, 1915)." He suggested that the right and left hemispheres cooperate together in the process of propositional speech: first, an involuntary revival of words occurs in the right hemisphere, followed by an involuntary perception of the words; then, voluntary revival and perception of the words occurs in the left hemisphere. This argument is vague and incomplete (Jackson, 1868; In: Head, 1915), but he later went on to relate the involuntary/voluntary distinction to automatic/voluntary processes in language production (Jackson, 1874; In: Head, 1915). He also proposed that the left brain serves automatic retrieval purposes for visual images, which are then voluntarily recognized by the right brain. Automatic perceptions, like automatic language are more primitive and well-organized than voluntary perceptions. He used an example of a delirious patient who mistakes his nurse for his wife. Although he theorized this alternation between hemispheres, Jackson also stated that "the most fundamental law of the developmental
education of the mind is the continuous reduction of successions to coexistences (Jackson, 1874:73; In: Head, 1915)." His implication was that the speed of processes in the nervous system may, in fact, diminish the importance of right/left distinctions.

Although championed as a rejector of the "doctrine of faculties" (Riese, 1955), Jackson had roots in Gall and Broca that account for some of his fundamental distinctions. Riese emphasizes his connection to prior and contemporary philosophical writings, which are certainly important (Riese, 1965); yet, Jackson remained first a neurologist and observer of patients who had to adjust his speculations to what was known about brain function, and obviously could not abandon the doctrines of his predecessors. Jackson is important because he reestablished a psychological dimension within aphasiology. First, he decided that normal language had emotional and intellectual components, and he looked at how these were differentially affected by brain disease. This gave rise to his descriptions of speech retained by aphasics, and his theories of voluntary and automatic speech (3). Second, he widened his subject from articulate language to the use of signs in general, which gave rise to his two classes of disordered speech, and to his speculations about the meaning of speech and language. His questions about meaning included the difference between internal and external speech, and the basic units of language and perception, which he ultimately
organized into a theory of mentation (Jackson, 1893; In: Head, 1915). Jackson’s semiotic approach is what links him to more contemporary language theorists and assures him his place in the history of aphasia studies.

**Turn of the Century Summations: Lichtheim and Freud**

Near the end of the nineteenth century, L. Lichtheim spelled out the localization theory of aphasia to its near-final degree (Lichtheim, 1885). Anything that has been added since is a minor modification. His work is more a blueprint of presumed connections between points of brain matter than it is a theory: I will talk about it generally in order to avoid reproduction of his several detailed vector diagrams that would encumber this discussion.

Lichtheim assumed that the language system consisted of functional centers linked together by fasciculi, and that disruptions of this organization resulted in particular types of aphasia. The frontal motor area, which contained motor images, was connected to the temporal area containing auditory images. Each was connected to an area "where concepts are elaborated," represented by a point in his diagrams, but he does not give it an anatomical localization. Visual images in the occipital lobe were connected to the auditory area, so that naming could occur; and to the frontal center for innervation of writing organs. Lichtheim supposed that
volitional speech came about when impulses passed from the center for concepts to the frontal motor center; he did not accept Wernicke's contention that motor speech required a passage through the auditory images in the temporal lobe, although he mentioned this possibility. Reading aloud was represented by a set of lines connecting the visual center to the auditory center and the auditory center to the motor center. He constructed analogous diagrams for copying written material, volitional writing and writing to dictation. He then went on to describe seven syndromes resulting from interruption of the stated inter-center connections: Broca's aphasia (or motor aphasia), Wernicke's aphasia (or sensorial aphasia), conduction aphasia (4), a variant of motor aphasia, ataxic aphasia, a sensorial aphasia with echolalia, and isolated speech deafness. A brief description of each will show their similarity to contemporary classifications.

Lichtheim stated that Broca's aphasia is associated with a loss of volitional speech, reading aloud, writing spontaneously and to dictation, and repetition. Comprehension and copying are spared. The anatomical defect is in the frontal motor region known as Broca's area (posterior third frontal convolution). Wernicke's aphasics have a loss of spoken and written comprehension, repetition, writing to dictation and reading aloud. Voluntary writing, speaking and copying were said to be spared. The defect in these patients is in the temporal lobe in Wernicke's area (posterior superior
temporal convolution). Conduction aphasia includes loss of repetition, reading aloud, writing to dictation; volitional speech and writing are characterized by paraphasic and paragrammatic errors; comprehension and copying are spared. The lesion is in the commissure that connects Broca’s and Wernicke’s areas. The variant of motor aphasia he described is now called transcortical motor aphasia and is characterized by speech and writing problems similar to Broca’s aphasia, with preserved repetition, writing to dictation, and reading aloud. It results from lesions that disconnect the motor area from the concept areas of the cortex. Kussmaul’s atactic aphasia is manifested by loss of volitional speech, repetition, and reading aloud. Comprehension is spared, as is writing; thus, it is like a Broca’ aphasia with spared writing, similar to what is today called aphemia. The lesion is in subcortical fibers going from Broca’s area to the control centers for the speech organs. Lichtheim’s sensorial variant, now called transcortical sensory aphasia, is like Wernicke’s aphasia but with spared repetition (usually in the form of echolalia), writing to dictation, and reading aloud (although without comprehension). The lesion here is in the fibers connecting Wernicke’s area to the concept areas. Finally, he states that isolated speech deafness is not really aphasia, but shows loss of comprehension for spoken language, loss of repetition and writing to dictation with preserved volitional speech (no paraphasia), comprehension of written
material, reading, and copying. The defect, analogous to ataxic aphasia, is subcortical and adjacent to Wernicke’s area, which is where Wernicke also posited it in the entity he called "pure word deafness." Lichtheim also describes a "total aphasia" that would now be called "global aphasia," due to extensive left hemisphere damage.

Lichtheim’s paper crystallized the nineteenth century discourse on localization of language. It is clear that he had read widely in the German, English, and French literature of his time. He was probably the first to describe transcortical sensory and transcortical motor aphasia, and his system does incorporate more clinical data than his predecessors’, for which he should be given credit; for example, he systematized the use of repetition in language testing and related its absence to conduction aphasia, which is usually attributed incorrectly to Wernicke. Yet Lichtheim’s discussion is most interesting when he attempts to explain the clinical findings that do not fit with his diagrams. For example, Wernicke’s patients do not speak normally: they make paraphasic errors, despite the fact that their motor centers are presumably intact, and some Broca’s aphasics have auditory comprehension problems, despite an intact Wernicke’s area. Similarly, Broca’s aphasics may have problems with silent reading comprehension that the speech center model cannot really explain. Although Lichtheim chooses a vague answer that stays within his model to explain
these findings – that the circuit from concepts to motor area to auditory area to concepts must be intact for normal function – Lichtheim raises the clinical points that weaken his conceptualization. Although patients continue to be fitted into categories similar to his, nearly every patient has some exceptional features that call the strictly anatomical model into question.

Sigmund Freud, Lichtheim’s countryman and contemporary, read the same literature and came to different conclusions (Freud, 1891; In: Stengel, 1953). He did not accept the idea of individual speech centers, connected by fiber pathways. He noted the lack of anatomical evidence for reproducible speech centers, clinical disparities that weakened the diagnostic groups, and the arbitrariness of a classification system in which "combined lesions" were used to explain all of the exceptions. In rejecting the view that the speech centers functioned independently, Freud criticized Meynert, whose work formed the basis for Wernicke’s theories; then Wernicke, Jackson and Lichtheim were passed under his scrutiny. His final formulation is a functional view of the language system, and it raises questions about methodology for studying psychophysical processes that merge into twentieth century language studies.

Freud thought that Meynert overemphasized the role of the cortex in his theory of brain organization. Freud pointed out that the peripheral body is not represented by a one to one
correspondence with cortical regions. Probably he was referring to the fact that cortical representation, since Meynert's time, was shown to be disproportionate to body size; for example, the tongue has more cortical representation than the entire trunk of the body. Freud also criticized Meynert's assumption that a white matter tract remains a tract even after passing through various subcortical nuclei, which he instead assumed must alter the composition of the tracts. Freud even criticized the idea that specific regions of the visual fields had precise areas of representation in the occipital cortex (5). Further, Freud challenged Wernicke's belief, derived from Meynert, that sensory impressions were somehow stored in individual neurons; a question still subject to debate. With these anatomical and physiological criticisms in mind, he went on to challenge Wernicke on clinical grounds. Freud said that conduction aphasia, as described by Wernicke, did not exist. We recall, Wernicke had predicted that patients with conduction aphasia could express themselves fairly well, although they might choose wrong words (paraphasias) because they could not monitor their own speech. Such patients could not read letters aloud as this would require a connection between sound images and motor images, and they could not write for the same reason. If educated, they could read words as concepts without reference to their sounds. Wernicke's own cases of conduction aphasia did not quite fit the prediction. Freud pointed out that paraphasias
occurred both in Wernicke's aphasia and conduction aphasia, which meant they occurred with destruction of a so-called center as well as with destruction of a so-called pathway. If centers and pathways were distinct, he felt they should be affected differently by pathology. He concluded that center aphasia is really no different from conduction aphasia, and that both represent interruption of multiple crossing tracts.

Freud's basic contention, then, was that speech centers are not real entities, and that the language-related areas of the cortex do not function independently. The appearance of such centers results from the fact that many fiber crossings converge in certain regions, and destructive lesions at those locations had been erroneously equated with clinical deficits. In other words, analysis of deficits had been used incorrectly to localize normal functions. Freud thought that his functional view explained the exceptions Wernicke and Lichtheim could not account for with their models: paraphasic speech in Wernicke's and conduction aphasias; the problem with "inner language" or comprehension in Broca's aphasics. Posterior lesions naturally influence anterior function and vice versa since the areas are interrelated. If speech centers were discrete areas surrounded by association cortex, polyglot patients should have their different languages represented in adjacent regions of cortex, especially if languages were acquired after their native language was well established. Yet, brain damage usually affects both languages
in bilinguals, which implies that the two are somehow superimposed in the same area. He also rejected the idea that transcortical aphasias are due to subcortical interruptions of white matter tracts, and thought instead that they are due to partial damage of the speech cortex. Like Jackson, Freud adopted Charlton Bastian's idea that "centers" could suffer three states of reduced excitability as a result of brain damage: failure to react to volitional stimuli (but reactive to other centers and sensory stimuli); reaction only to sensory stimuli; and no reaction to any stimuli. Transcortical motor aphasia, then, is Bastian's first state applied to Broca's area.

Freud also apparently embraced Jackson's notion that brain damage caused disinvolution. He thought that "the aphasias simply reproduce a state which existed in the course of the process of learning to speak (Freud, 1891; In: Stengel, 1953: 42);" and that the different centers were co-stimulated in the process of speech acquisition. He thought that speech remnants in aphasics were evidence of this return to an earlier developmental level, as was preservation of series speech and the fact that words with few associations were more likely to be lost in aphasics. Like Jackson, Freud proposed his own classification system for the affectations of speech, and like Jackson's, it was not clinically cogent enough to replace the existing system, or even to influence it much. Basically, he defined three types of aphasia: verbal,
asymbolic and agnostic. In verbal aphasia, the associations between single elements of a word (for example, kinesthetic word images and visual images) are disturbed at the "neural substrate" level. Usually "speech centers" are involved at areas where bilateral impulses come together: speech, writing, and the recognition of letters are impaired. Asymbolic aphasia occurs when the association between a word and the concept underlying it are disturbed. In its pure form it is due to circumscribed lesions lying across an association tract, but most commonly there is mixed verbal-asymbolic aphasia, due to impairment of the auditory aspect of speech, and giving the picture of Wernicke's aphasia. Agnostic aphasia occurs when agnosia is present; a disturbance in object recognition due to disturbed association between an object and its idea. It is usually due to extensive brain damage.

Throughout his discussion Freud continued to refer to centers, although he argued against their existence. Probably he was unable to talk about psychological processes without conceiving of a spatial arrangement in which they took place. This spatialization bias is inherent to all Standard Average European languages, the languages in which aphasiology flourished in the nineteenth century, and will be discussed again later when we talk about Benjamin Whorf. Freud also accepted, at least to some degree, the clinical types of aphasia organized by Lichtheim, and refined again later by Wernicke. These clinical types form the basis for some of his
arguments; for example, the fact that paraphasias occur in both conduction and Wernicke's aphasia, and they find their way into his own definitions. This apparently inconsistent acceptance of clinical subtypes created by anatomical theories while rejecting the theories is never explained. Freud did not mention any cases of aphasia that he himself saw, which is certainly unlike his later psychiatric writings. It is, therefore, likely that most of his ideas in the aphasia monograph arose from reading aphasia literature, rather than from experience with patients. Freud focused on the psychological nature of language, as Jackson had before him and like Jackson, considered brain damage an instance of disinvolution. The fundamental assumption of his proposed aphasia classification is that the word is the most important psychological unit of speech. This assumption does not take him very far: his descriptions are more related to naming of objects than they are to actual meaningful speech. Jackson had insisted that the proposition was the unit of speech that should be studied. This lead him to his insights about the nature of speech residues, but left out many clinical phenomena, like paraphasic speech. Neither Freud nor Jackson could adequately grapple with the anatomical information that was accumulating about aphasia. Broca, Wernicke and Lichtheim, on the other hand, were more concerned with the mechanics of word formation and manipulation than with the psychological aspects of speech. Their theories did provide
some organization to the mass of neuropathologic information, and did provide labels that could separate patients to some degree; but they were based on clinical features that had arisen haphazardly since ancient times and were uninfluenced by contemporary notions of psychology. The more language was studied as a window on thought, the more dissatisfaction was bound to arise with theories based on conventional observations. As we move into the twentieth century, it is not surprising that at least two of the major figures in aphasiology began by revising the testing methods used to gain information about aphasic patients, and then went on to develop psychological theories based on behavior elicited by their tests. Pierre Marie actually started this reanalysis of the old theories based on behavioral testing and reexamination of pathological material, (including Broca's cases) late in the nineteenth century. However, his methods and analysis were taken up by Henry Head, and more fully developed there.

Head and Goldstein

Twentieth century aphasia literature is characterized by precise descriptions of how patients are tested. Unless certain standard test batteries are included, a study on aphasia will likely go unpublished. This is a marked change from the writings of Broca, who did not systematically test his patients and relied on the memory of caretakers for some
of his clinical descriptions. Wernicke and Jackson seem to have chosen tests based on the circumstances and what they wanted to elicit. Yet, in order for Lichtheim to elaborate the anatomical theory of speech, he had to systematically test those functions that conventional wisdom had handed down to see if his model worked. He made testing of spontaneous speech, repetition, oral and written comprehension, writing voluntarily and to dictation, reading and copying all part of his evaluation. This systematization of what, to him, constituted the important aspects of speech was only part of his evaluation. He also created the "syllable test" to see if patients understood words that they were unable to say: he instructed them to squeeze his hand once for every syllable in a word they were thinking of. Lichtheim viewed this as a test of inner speech. Tests such as these were not new to neurology, which has always relied on specific spontaneous and elicited behaviors, so-called signs, to localize and diagnose disease. Yet even when compared to Lichtheim and Marie, the rigor of language testing introduced by Henry Head in the early nineteen hundreds seems like a new phenomenon.

Head's major paper is a study of young men with language disturbances resulting from gunshot wounds to the head (Head, 1920). He began with a lengthy discussion of his battery of tests that included the following: naming and recognition of common objects; naming and recognition of colors; the man, the cat, and the dog test; the clock tests; coin-bowl test; hand,
eye and ear tests; and miscellaneous tests including reciting the alphabet, reading the newspaper, describing a picture, doing calculations, and drawing. Before looking at these further, we should note that speech testing now seems to include many behaviors that are not, at first glance, a part of speech. At least we can say that Head’s observations included much more testing than the nineteenth century studies had. We will now look more closely at a few of his carefully described and controlled methods for probing language capabilities, and then at the classification system for "aphasia and kindred disorders of speech" which he developed as a result of these methods.

We recall that Freud’s emphasis on the importance of the word, especially words describing objects, was central to his classification of the aphasias. Patients would either have problems with recognition at a preverbal level, or with associating a word with a concept or with eliciting a word in response to various internal stimuli, depending on the type of aphasia. Nowhere did he mention how to go about checking for these things. In contrast, Head tested the following with respect to naming and recognition: he showed the patient six objects; screened them from view and gave the patient a duplicate of one to feel and identify; removed the screen so the patient could match the felt object to one seen; had the patient point to each object as it was named aloud by the examiner; had the patient point to written names of objects
read aloud by the examiner; asked the patient to write object names; asked him to repeat object names; and asked him to copy printed names into cursive writing. For the clock test, Head told patients to set the time on one clock according to another clock, then according to a verbal command; then to tell the time aloud, and then to print it. Finally, we will discuss one more test, the so-called hand, eye and ear test. The patient first imitated the examiner as he touched his own right or left eye or right or left ear with one hand or the other. This was done with the examiner facing the patient, and then with both facing a mirror. Then the patient imitated the actions from cards, then from oral and printed commands. He ended by reading the commands aloud and writing them down.

Head concluded that inconsistency characterizes all cortical lesions, so much so that one cannot divide disorders of language into disorders of speaking, reading, writing, etc. For example, a patient might successfully read object names and identify the objects in front of her, but be unable to set a clock or perform the appropriate hand, eye, or ear gesture in response to written commands. Was her disorder, then, one of reading or not? Further, Head’s research indicated that images were not the same as their linguistic representations. A patient might be unable to name what he wanted, but indicate it perfectly with gestures, implying that his image for that thing was intact although the word for it was gone. The same patient might successfully match a sensation to an object; for
example, he might associate a soft tactile sensation with a soft object in front of him, lending further support to the idea of intact images. Tasks that involved matching sensations or images seemed easier for most of the patients than carrying out formulated propositions. Head concluded from all of this that his patients had problems with symbolic formulations, in general, which he preferred to talk about as problems of symbolic thinking and expression.

Head's classification of aphasia included four groups: verbal, syntactical, nominal, and semantic. Verbal aphasia is a defect in getting concepts of images into the form of words. When severe, the patient only says "yes" or "no," and may or may not use these words appropriately. Enunciation is slow and halting, and the structure of words is abnormal. Writing is difficult in the same way as speech. Automatic speech is sometimes better, and copying drawing, playing games (like cards) and enjoyment of jokes are normal. Verbal aphasia, then, is defective word formation in patients who can symbolize in other ways. They are somewhat like those patients called Broca's aphasics by Lichtheim, and Class 1 by Jackson. Patients with syntactical aphasia talk rapidly and use jargon words. Their speech is without grammatical coherence. Although they can often name objects, especially in writing, writing spontaneously and to command are otherwise impaired, which implied to Head a defect in internal speech. They can copy print into cursive writing, which necessitates
recognizing the letter symbols of each mode. In general, operations dealing with words are better than attempts to convey formulated statements. These patients sound similar to Lichtheim's Wernicke's aphasics, and Jackson's Class 2. Like the other classification schemes we have seen, Head puts verbal and syntactical aphasia at either end of a spectrum from non-fluent and comprehending to fluent but uncomprehending. The next group, nominal aphasia, is characterized by lack of comprehension for the nominal value of words and other symbols. These patients use names defectively, including letters of the alphabet. They write poorly and cannot convert print into cursive writing. Since they have trouble with the value of numbers, they cannot play games. They cannot draw well to command and have great difficulty with ground plans, presumably because they cannot symbolize spatial relations or objects on paper. In the last group, patients with semantic aphasia fail to recognize the full significance or intention of words or phrases. Their problems include dysfunction not related to verbalization. Although they can enumerate the details of a picture before them, such patients cannot formulate the meaning of the whole picture at a narrative level. They form words well and repeat well, but do not understand jokes. Head said that their memory and intelligence seem fine and that their problem is related to complex symbolization abilities.

All four groups of aphasics share difficulty with symbolic
representation. Although Head suggested avoiding the term "proposition" because it was difficult to define, he stated that they have increasing difficulty with increasing degrees of propositional value. For example, a two-step command (like close your eyes and then touch your ear) has higher propositional value than a one-step command. Or, imitation of an action is more difficult when the patient faces the examiner than when the two of them face a mirror, since the first task involves the patient stating to himself what to do. Because of this problem with propositionizing, patients often use metaphors instead of exact words. After all of his careful descriptions of the behaviors disturbed in association with language, Head concluded that the defects of symbolic thinking and expression "form a group of mental processes, which can be defined at present by enumeration only."

By comparison to Head’s modest conclusion, Broca’s placement of language in the left hemisphere and Wernicke’s description of a speech circuit embedded in the peri-sylvian region seem momentous. Even Jackson’s speculations about automatic behavior, links between motor function and speech or brain duality and simultaneity, are more dramatic than the simple statement that we can only enumerate deficits in patients who have problems with symbolization. It seems that Head’s obsessive attention to detail in one particular group of language disordered patients had rendered inadequate the prior categories of speech disturbance which had focused on a
narrower range of behaviors. Head emphasized that a brain damaged organism maximized all of its remaining functions, and that its behavior was the sum of changes wrought by injury and positive compensations, much like Jackson's notion of negative and positive symptoms. This view, together with the large number of tasks he gave to patients, showed that brain injuries resulted in diverse effects.

Although much of Head's behavioral examination has filtered down within neurology to the present time, few or no neurologists utilize the whole battery. Further, use of his diagnostic categories is not widespread. Before deciding why this might be, it will be useful to look at the work of Kurt Goldstein, another prominent aphasiologist from the first half of this century.

Goldstein stated in his Preface that he would deal with the biologic, psychologic, pathologic, anatomical, and philosophical issues related to language (Goldstein, 1948). Probably because of his wide reading outside of medicine, he used the term "language" in a way that was new to the field of aphasia studies and seemed to encompass the word-related activities of individuals as well as collective groups. Like Head, Goldstein focused initially on the testing situation to decide what behaviors he would study. Like Jackson and Freud, he was a phenomenologist, with a basically psychological approach to speech. Finally, as promised in his Preface, he attempted to synthesize his observations with anatomical
theories of speech, which linked him back to Lichtheim, and ultimately to Wernicke and Broca. We will look at Goldstein's process for studying aphasic behavior, highlighting a few clinical points that are unique to him. We will then look briefly at his classification scheme for language disturbances. Discussion of his work will, I think, give a good summation of aphasia studies prior to the advent of noninvasive brain imaging techniques in the nineteen seventies, and will raise questions about the relationship of aphasia theories to non-medical theories about language, which we will take up in the next part of this paper.

Goldstein began by redefining neurologic symptoms into four types: direct symptoms, due to dedifferentiation of function in a damaged area (Jackson’s negative symptoms) or to separation of an undamaged area from the damaged region (Jackson’s positive symptoms); indirect symptoms, which produce effects of dysfunction in regions removed from the injury (diaschisis); symptoms due to catastrophic reactions which occur when stress and anxiety from a deficit overwhelm the patient, leading to withdrawal, denial, etc.; and symptoms due to fatigue and perseveration. His reasoning for these categories includes a discussion of electrophysiologic principles, like firing threshold and spread of electrical activity, which he applies equally to brain function and to the behavior of the whole organism, without acknowledging that the latter use must be metaphorical. This discussion, then,
seems "unscientific" and abstract, and weakens his argument for these particular categories of symptoms. One of his conclusions, which sounds very much like Head could have written it, is reiterated throughout his work: "every individual speech-performance is understandable only from the aspect of its relation to the function of the total organism in its endeavor to realize itself as much as possible in the given situation" (Goldstein, 1948:21). This is not necessarily true of motor performances because testing situations can be devised in which only one muscle, or one myotome, or one motor activity is tested at a time. Similarly, the modalities of sensation can be separated at a receptor level (pain versus vibration), or a dermatome, or a complex, cortical-level sensory task can be identified. This difference between language behavior and motor and sensory function is important because the localization of deficits, so important in neurology, can only be applied if the deficit can be isolated behaviorally. Goldstein's conclusion would argue against the possibility of localizing language functions, but, as we will see, he did not abandon the localization approach.

Goldstein offered an original division of normal behavior, concrete versus abstract, and maintained that each had its own associated type of language. Concrete language is made up of speech automatisms and the "instrumentalities" that produce words. Persons speaking concretely use sounds, words, and sentences to refer to familiar situations and to express
emotional language. Naming of objects occurs in a limited way; for example, when asked to list the animals in a zoo, someone might list them as he has seen them when walking into a familiar zoo rather than by abstract categories. So, instead of bears, giraffes, gorillas, etc. he might say brown bears, polar bears, South American monkeys, chimpanzees, small mammals, etc.. Although concrete language is a part of normal speech, some abnormal patients utilize this exclusively. Abstract language is more inclined to deal with meaning; it is volitional, propositional, and rational. Naming in the abstract sense consists of accessing categories, rather than referring to actual entities. When asked to list the names of fruits, one might say oranges, pears, apples, etc. without having particular examples of each in mind. Abstract language allows a person to assume an "abstract attitude," which is made up of six abilities: 1) assuming a voluntary mental set, taking initiative and beginning a performance; 2) shifting from one set to another; 3) keeping two aspects of a situation in mind at once or reacting to two different stimuli at once; 4) grasping a whole and being able to break it into parts and voluntarily recombine parts; 5) abstracting common properties, planning ahead, recognizing possibilities, and thinking symbolically; 6) detaching the ego from the outer world (6).

Like Jackson and Freud, Goldstein thought that aphasia represented a dedifferentiation of language. He cited several phonetic observations, such as the order of acquisition of
phonemes, and then tried to show that the language defects in aphasics were related to the developmental observations that he had cited. For example, the sounds acquired latest in normal development are the earliest lost in aphasics. We will not spend much time on this discussion since most of the linguistic observations he mentioned, both in normals and in aphasics, have not been substantiated by later research. However, it is noteworthy that Goldstein read linguists, such as Jakobson, and attempted to relate their findings to aphasia.

Having introduced his approach to symptoms, localization, and the concrete and abstract composition of normal language, Goldstein went on to list and describe his categories of language pathology. Patients might have disturbed language secondary to impairment of abstract attitude. Such patients could not use words as symbols, had trouble initiating and comprehending speech, and used concrete language in a compulsive way. Or, patients might have problems with rational or emotional language. Or, they might show disturbances in word-finding, which could take several different forms: problems with confrontation naming secondary to impaired abstract attitude; literal or verbal paraphasias with preserved meaning due to trouble with the instrumentalities of speech; or disturbed naming of low frequency words only, which improved with prompting, that he thought was characteristic of normal old people. His next
category was disturbances of repetition of heard language which, unlike the mere imitation of sounds, involved symbolization. He then described disturbances of the expressive side of language, the receptive side of language, inner speech, intelligence, reading and writing, calculating, gesture and pantomimic performance. Finally, he discussed disturbances of language in polyglot aphasics and disturbances of musical performance.

We will look at a few of Goldstein's categories more closely in order to get a feeling for his style and to have a basis for comparison with earlier work. Although Goldstein separated his clinical and anatomical descriptions into Part II of his book, we will include them with the theoretical descriptions from Part I (7). The first disturbance of the expressive side of language is dysarthria, caused simply by paresis of the muscles used in speaking. Next in this category is peripheral motor aphasia, or loss of the capacity to use these same muscles for the production of language. These aphasics have telegraphic speech and stuttering, with only mild impairment of abstract attitude: their condition is due to a lesion in Broca's area. Central motor aphasics can speak better than peripheral motor aphasics, but have a lack of spontaneous speech and marked impairment of abstract attitude. Their lesions are presumably more posterior, toward the center of the language area. Next he described "isolation of the motor instrumentatlities," defined as the disturbances
of motor speech seen in sensory aphasia. Finally, he mentioned transcortical motor aphasia which is produced by damage of non-speech processes or by isolation of the speech instrumentalities from the rest of the brain. It is either due to partial damage to Broca’s area or some combination lesion involving Broca’s and other frontal areas.

He separately delineated disturbances of the receptive side of language. Pure word deafness or peripheral sensory aphasia is a condition in which sounds are not recognized as language, just as Wernicke had defined it. Speaking, reading, and writing are intact. The lesion involves bilateral disconnection of Heschel’s gyrus (the central projection area for audition) from Wernicke’s area. Heschel’s gyrus itself has to be intact in at least one hemisphere to ensure hearing. In central sensory aphasia, patients cannot perceive heard words, although they can produce words. They have damage to inner speech as well as perception, and all speech performances are affected, although Goldstein thought that their spontaneous speech is more impaired than their comprehension. The lesion is near Wernicke’s area. Transcortical sensory aphasics perceive words perfectly, but the words evoke related words instead of the correct meanings. Speech instrumentalities and repetition are usually intact. He thought such patients were either recovering Wernicke’s aphasics or had slight damage of speech perceptions and partial disconnection of these perceptions from non-speech
areas. Goldstein pointed out that in all aphasics the understanding of words is conditional; for example, a patient might understand a word better after reading or repeating it, or he might understand a word but be unable to repeat it. Like Head, Goldstein emphasized that patients with brain damage will utilize whatever systems and functions are available to them in order to complete a task, which might well lead to variable performance.

It is interesting to note that Goldstein, like Freud, denied the existence of conduction aphasia, described by Wernicke and Lichtheim. He felt that such cases in which repetition is markedly impaired with some mildly paraphasic speech and good comprehension represent milder forms of central sensory aphasia. He also predicted that multiple lesions, both within and outside the left temporal lobe might account for these findings, which we now know to be the case. Goldstein also described amnesic aphasia, marked by loss of categorical naming abilities, especially with confrontation naming. All such patients have impaired abstract attitude and, therefore, fail in sorting tasks. The anatomy is unclear, but he felt that localized damage to the temporo-parietal region, if it spared the speech instrumentalities, was a possibility.

Although Head argued against specific disorders of reading and writing, Goldstein provided these categories and subtypes within them. There are primary alexias, actually expressions
of visual agnosia in reading. These patients might read letters but not words or words but not letters. They might accomplish reading by using systems other than the visual; for example, by tracing letters kinesthetically with movements of the head. Patients who have disturbances of the speech instrumentalities as well as impaired reading have secondary alexia. Primary agraphia is a disturbance of the complex motor act of writing and could be due to lack of initiative, apraxia, or impaired abstract attitude. Secondary agraphia, analogous to secondary alexia, is due to disturbed speech instrumentalities.

Goldstein’s work included a lengthy section on techniques of examination. In it he criticized Head for not separating out the effect of impaired abstract attitude on test performance. He also criticized Head’s view that poor performance on tests like "the hand, eye, and ear test" was because of defective internal verbalization; Goldstein insisted that visualization, kinesthetic cues and other modalities were involved as well. Goldstein created and co-created a number of tests that have been standardized and are still marketed: the stick test, cube test, color sorting test, and color form sorting test are among them. His examination section included tips on how to interview a patient, encourage transference, and avoid catastrophic reactions. He utilized his own tests for abstraction. With respect to language, he suggested that spontaneous speech, series recitation,
repetition, naming, auditory comprehension, and counting should be tested in all patients.

Both Head and Goldstein were concerned with the relationship of speech and/or language to thought, perception, production of words, and understanding of spoken and written language. Both rethought and reorganized behaviors into testing situations, and attempted to deduce language capabilities from patterns of performance on these tests. The emphasis in their work, compared to nineteenth century endeavors, is clearly on testing methods as a means of organizing phenomena.

Although usually cited as a non-localizer, Head, in a later work, attempted to integrate his results with data concerning localization: he claimed that verbal aphasia was due to injury to the lower pre- and post-central gyrus; syntactical aphasia, to lesions of the upper temporal gyri; semantic aphasia was associated with supramarginal gyrus lesions; and nominal aphasia, with angular gyrus injuries (Head, 1926). Despite these claims, which were even more anatomically specific than many of his predecessors', Head's work was important for pointing out problems with the traditional aphasia nosology. His major original contribution is the notion that aphasias all involve difficulties with symbolization. Head was also responsible for the reprinting of Jackson's work in *Brain*, in 1915, and was obviously influenced by Jackson's non-localizationist theories. He
showed that some behaviors were disturbed that had not previously been associated with aphasia, thus widening the field of study. He also demonstrated that reading, writing, repeating, and other speech-related behaviors might vary with the situation, and could not therefore form diagnostic categories. He concluded that the classification of language disorders, which represented an attempt to organize behavioral observations on the basis of anatomy alone, was not adequate to organize the behaviors that he had enumerated. The categories of affectation that Head proposed can be partially related to prior definitions: Broca's Class 1 and Verbal aphasia; Wernicke's Class 2 and Syntactical aphasia. His nominal aphasia is like the "amnestic aphasia" attributed to "loss of memory for words" by James Russel, in 1864, and called "anomia" by Lichtheim, who thought it was due to a nonfocal process and should not be included with the aphasias. Semantic aphasia involves such pervasive cognitive problems that it could be thought of as reflecting a loss of functional intelligence. So, although his approach to testing and his focus on symbolization were new, Head's categories were not. His later discussion of aphasia localization did little to maintain a differentiation between his classes and those he objected to. When Goldstein and others further expanded the subject matter of aphasia and the testing of patients and related these findings to anatomy, Head's observations could easily be subsumed into the classic model of language.
Goldstein contributed further testing methods to the body of aphasiology, and several new categories of disease, from defects of inner speech to disturbances of musical performance. His categories read more like a list of maladies he has known than like a theoretically based group of observations. Further, they are not mutually exclusive, which is a departure from the categorization method used by all of his predecessors. He attempted to relate some of his categories to Lichtheim’s anatomical model as previously described. Goldstein’s system is the first in which Broca’s aphasia is not anatomically parallel to Wernicke’s types: he said that Broca’s aphasia is due to a peripheral lesion (on the periphery of the speech area), whereas Wernicke’s is due to a centrally placed lesion (8). Interestingly, there is nothing in the data that came before him or in his own observations to prove or disprove such a shift from conventional theory. Although willing to localize Broca’s, Wernicke’s and transcortical aphasias, Goldstein did not address localization for all of his described language and language-related problems.

Like every observer before him, Goldstein had his own conscious and unconscious impressions of what constitutes speech and language. From Broca through Freud, actual speech seems to have been the main subject of aphasia analysis, although they all acknowledged nonverbal and nonlinguistic mental processes. With Head, the emphasis on symbolization
shifted the focus from inner and outer speech to a more intangible mental activity: Head called it "symbolic thinking and expression," which acknowledges the speech situation, but places it secondary to that which is expressed. Goldstein, however, seemed to abandon speech. People manifest abstract and concrete attitudes of thought, and all symbolic activity related to these attitudes, whether silent or spoken, he now referred to as language. Also, he felt that language, so defined, could be related to information about language from other fields, such as phonetics. This shift in terminology, from speech to language, and the underlying attitudinal change that it represents had occurred much earlier in fields outside of neurology; in Schleicher's linguistics, for example. The confusion of language with speech was unfortunate for aphasia studies. Although it was already difficult if not impossible to relate speech to the known structure and physiology of the brain, it is more difficult to relate the entity of language, which can be talked about as a self-sufficient entity outside of speakers. Goldstein and Head expanded the range of behavior looked at by aphasiologists and the testing methods at their disposal. However, these contributions were soon overshadowed by technological advances that include radiologic techniques to examine the living brain and the ability to stimulate specific areas of the cortex in awake patients during surgery. Once all aspects of language were viewed as physiologic processes localized to the left hemisphere, the
stage was set for trying to dissect the hemisphere with new technology and to localize the units of language by the same means.

With this survey of the history of aphasia complete, we will now turn to an historical discussion of a few theories about language. My hope is that we can reconstruct some of the influences that such theories had on the selection of behaviors that were considered part of speech and later language, and which we have seen form the basis for differentiating aphasic patients into groups. I also hope to show some of the linguistic assumptions that have gone into theories of how language is organized in the brain, particularly since most of the literature is written by speakers of Standard Average European languages, which have their own biases for the treatment of psychological phenomena. Finally, in the course of this discussion, I will suggest certain approaches from outside Neurology which might have been usefully applied to the investigation of speech and language.

PART 2: LANGUAGE THEORIES AND APHASIA

Linguistic Influences: Broca, Wernicke and Jackson

As noted before, observations relevant to aphasia began in antiquity and consisted at first of descriptions of
speechlessness and later of lists of preserved and missing speech behaviors. By the end of the eighteenth century, all of the major aphasic symptoms, with the notable exception of sensory aphasia, had been described. The eighteenth century was also characterized by the enlightenment debate about the philosophical aspects of language: whether language was God-given or something that man created himself as an expression of his own intellect and emotional structure. Several German philologists, Johann Gottfried von Herder (1744-1803) among them, argued that language originated in human nature and expressed man’s individuality and his culture. Franz Gall (1758-1828), a physician writing at the end of the eighteenth and beginning of the nineteenth century, hoped to end the debate with his theory that the various human faculties were actually represented in the brain in circumscribed regions, identifiable by inspection of the gross brain and of the protruberances in the overlying skull. Bilateral function in symmetrical organs such as the brain was considered a fact of physiology in all discussions at the time (Berker, 1986).

We also mentioned in Part I that Gesner, in 1770, proposed a linguistic explanation for the abnormal speech of jargon aphasics: a disturbed ability to associate images with their linguistic signs. This type of explanation for aphasic speech was unusual, as most other observers in neurology were still writing purely descriptive work. The fact that Gesner proposed it is, however, indicative of the interest in
interpretation of texts and the beginnings of hermeneutic theory in the eighteenth century, in the works of Johann Martin Chladenius (1710-1549) for example.

However, Broca, Wernicke and Jackson were nineteenth century men and all of this was just background to them. Their contemporaries were men like Charles Darwin, Herbert Spencer, and August Schleicher who certainly do not encompass all of nineteenth century thought, but were at least influential and were read widely in their naturalistic, philosophic, and linguistic fields. This is part of my justification for choosing to look at the work of these three men more closely. The fact that they were mentioned by or connected with the major figures in aphasiology will help us to address the extent of cross influence among them.

Charles Darwin (1809-1882) speculated about the origin of language and its relationship to human emotions throughout his long career. His views about language are most fully expressed in The Descent of Man, but can be found in his so-called N notebook and in The Expression of the Emotions in Man and Animals. His basic argument is that the rudimentary forms of communication exist in animals and that higher mental powers, like language, arose from simpler instinctive processes such as emotional expression and imitation (Barrett and Gruber, 1980). Darwin felt that articulate language was peculiar to man, although inarticulate cries, gestures, and facial movements were shared with lower species (Darwin, 1871;
In: Princeton U. Press edition, 1981). He believed that thought was possible without language, and he therefore contended that lower animals could think in some fashion despite their lack of speech. Men, parrots, and apes were all capable of articulation per se, but only men had the power to link definite sounds with definite ideas. This articulate language, he believed, was not invented by man but "slowly and unconsciously developed" over time.

Acknowledging a debt to two contemporary linguists, August Schleicher and Max Muller, Darwin theorized that language originated in "the imitation and modification, aided by signs and gestures, of various natural sounds, the voices of other animals, and man's own instinctive cries" (Darwin, 1871:56; In: Princeton U. Press edition, 1981). Darwin, who wrote before Mendel's laws of inheritance were widely accepted, believed in the "doctrine of the inherited effects of use," and he used this doctrine to explain how language evolved in the human species. As the voice was used, the vocal cords were strengthened and perfected. Similarly, the use of sounds to express various emotions reacted on the mind in such a way that it developed the capacity to carry on long trains of thought. Language then evolved along with the species and was dependent on the evolution of intelligence. The fact that brain damage can lead to particular language behaviors caused him to conclude that the brain was intimately connected with the faculty of speech. Darwin also believed that individual
words evolved and that their origins could be traced backwards, often to the imitation of sounds, which he felt originated in our prehuman ancestors. Language contained rudiments analogous to the physical rudiments found in many species; for example, in the construction "I am," the I is really no longer necessary as the first person singular is contained in the conjugated form "am." Darwin agreed with Muller that individual words go through a kind of natural selection, favoring simplicity and slight change. Higher functions, like self-consciousness, abstraction, and the moral sense could only evolve after language.

Darwin mentioned Broca several times in The Descent of Man, usually to refute his contention that skull size is correlated with intelligence or his work dealing with the anatomical differences between races. Darwin also mentioned Broca’s work as having lead him to believe that he put too much emphasis on natural selection, for there are structures that are neither beneficial nor injurious to the species, that probably evolve by some other mechanism. Darwin was referring to a critique of his work written by Broca. As founder of the Parisian Anthropological Society in the early nineteenth century, Broca was very involved in the debates surrounding Darwin’s theories and wrote several articles and commentaries (Schiller, 1979:142-3). Broca was also familiar with the work of the linguist, August Schleicher, and Schleicher and Darwin referenced each other repeatedly. It is probably safe to
conclude that Darwin's work was profoundly influential for all of the nineteenth century aphasiologists.

As early as 1861, before Darwin published *Origin of Species*, Broca differentiated the general faculty of language from articulate speech. His differentiation was an historical one based on similar definitions in the past, such as amnestic aphasia versus ataxic aphasia. Several years later Darwin seems to have provided evidence that the entity of articulate language was separate from general intelligence, by showing that animals were capable of articulation and some "higher functions;" however, these did not constitute language because animals could not link definite sounds to definite ideas. This perspective, in the context of Darwin's whole evolutionary theory, no doubt did a lot to further the idea of speech as a definable entity rooted in the human brain. By the time Wernicke wrote his aphasia monograph, both Darwin and Broca were well established and influential. Darwin also strongly influenced the thinking of Herbert Spencer, often mentioned in connection with Hughlings Jackson. Both Darwin and Spencer contributed observations about the emotions in man and animals that were important to Jackson's formulations. Darwin's work, then, seems to have given credence to the idea that a faculty of articulate language existed in the human brain. This faculty must be structurally based because he also provided evidence that it had originated from instincts and emotional behavior, and then evolved within the human
species. Although Darwin did not directly enter the debate about language localization, his observations would have strongly supported the idea because he accepted the existence of separate human faculties with structural bases that were subject, at least in part, to natural selection.

August Schleicher (1821–1868) was mentioned above as a link between Broca and Darwin. He influenced Abel Noldeke, a French linguist who collaborated with Broca and agreed with Schleicher that language was located in the somatic character of man and must be studied along with the evolution of the brain, an idea that obviously derives from Darwin (Koerner, 1983). More importantly, Schleicher was a linguist; the first to study a language directly from speech, rather than from texts (Lithuanian); and the first comparative linguist to codify the piecemeal investigations about languages into a theory of Proto-Indo-European and its relationship to the existing languages.

Comparative linguistics probably began with Sir William Jones in the eighteenth century and was systematized by Franz Bopp who linked Sanskrit rigorously to other European and Asian languages (1816). Several years of such investigations passed before Schleicher, in 1861 and 1862, organized them into his theory of how the various languages are related to each other; the basis for the language tree we are now familiar with. Shortly after this work he wrote an essay (in letter form) called "The Darwinian Theory and the Science of
Language," in which he commented on Darwin's *Origin of Species* (Schleicher, 1863; In: Koerner, 1983). In this essay, Schleicher talked about languages as "organisms of nature" which "rose, developed, grew old, and died out (Schleicher, 1863; In: Koerner, 1983:20)." He felt that all of Darwin's principles concerning natural selection were, therefore, applicable to the organism called language. A few years later he wrote another piece in which he answered the criticism he had received for treating languages like "material things." He argued that speech, perceivable by the ear, was just the manifestation of language; speech was to language what light was to the sun. Like light, then, speech itself was a structure and a physical symptom of the whole organism, the same as gait. Since language had to evolve before our ancestors could be called "man," the assumptions of linguists lead to the assumptions of the gradual evolution of man from lower forms.

Schleicher believed that there were anatomical differences in the brains of people who spoke different languages. Whatever structure underlay speech was therefore subject to evolutionary forces. Broca, too, believed that different races were anatomically different; almost to the point of being different species, although Darwin argued strongly against this idea. The interesting point here is that both Broca, the anatomist, and Schleicher, the linguist, talked about language as a brain structure, consistent with Darwinian
ideas of structure. Broca used the term "articulate speech," and it is this faculty that he localized to the third left frontal convolution. Schleicher talked about speech as a physical manifestation of language; by language, he apparently meant the various national tongues he studied and systematized. Darwin's influence has, over time, allowed both ideas of speech to be treated like biologically based structures. This perception may have cooled the discussion of whether language was God-given or evolved in man, but it also helped to create a problem with the definition of speech and language, and our ability to study and to localize them. Before we enter the discussion, sparked by Saussure, about the differences between speech and language, a brief look at Spencer will finish our nineteenth century survey.

Henry Head was not fond of Spencer. In his introduction to the reprinting of Jackson's work, Head blamed Spencer's style for contributing to Jackson's obscurity (Head, 1915). This no longer seems justified to me, as Spencer is easier to follow than Jackson, and probably contributed less to Jackson's theories than Head has suggested. Head also credited Spencer with making the distinction between emotional and intellectual language that was taken up by Jackson and which contributed to some of Jackson's theories, such as automatic speech and recurrent and occasional utterances. As mentioned in Part 1, the emotional/intellectual distinction was at least as old as Gall, who assigned each a separate
faculty, and it was already part of aphasia discourse. Darwin respected Spencer, and cited his writings about "the moral sense" in *The Descent of Man*, and Spencer read Darwin's *Origin of Species* which he referenced in his *Principles of Psychology*. This listing of connections is leading up to two points: that Spencer directly influenced Jackson, although to a mild degree; and that Spencer's main importance to aphasiology is that he provided a link between Darwinian theory and psychology, which naturally appealed to Jackson, and to those aphasiologists who also had a psychological approach and followed later.

Spencer believed that all products of human thought, including language, underwent a process of evolution, defined as a movement from incoherent to coherent, and from homogeneous to heterogeneous (Spencer, 1867). The lowest form of language, the exclamation, was found in lower animals as well as man. Exclamations were clearly a low form of language because an entire idea could be conveyed by a single sound. From exclamations, simple parts of speech (nouns and verbs) arose, which allowed more coherent expression of ideas, but were more heterogeneous at the same time. Later, the parts of speech multiplied to include active and passive verbs, abstract and concrete nouns, mood, tense and person indicators, etc. Similarly, diverse words arose from common origins and diverse languages from primitive ones. He dealt with all of these examples in greater detail, and then revised his definition of
evolution to: "a change from an incoherent homogeneity to a coherent heterogeneity, accompanying the dissipation of motion and integration of matter" (Spencer, 1867:359). Having established Spencer's definition of evolution as it related to language, we can now look at the concept of dissolution, which was utilized by Jackson. Spencer's definition was still metaphysical and even more vague: dissolution is "the absorption of motion and concomitant disintegration of matter (Spencer, 1867:288)." Summarizing his lengthy discussion, he thought that evolution always reached a point of harmony between the system under scrutiny and the environment such that evolutionary changes ceased. However, since environmental actions continued, "motion" was induced and eventually restarted the evolutionary process. Although he discussed dissolution as it applied to various things, such as society, organic and inorganic compounds, Spencer did not apply it within individuals, as Jackson clearly did.

Let us now turn to Spencer's views about emotional language, espoused in his *Principles of Psychology* (1896). His use of naturalistic observations, and even his prose, are reminiscent of Darwin. In Chapter IV, "Language of the Emotions," he said that humans influence each other's intellects through signs and words, and each other's feelings through the physical manifestations that accompany their feelings. All feelings give rise to the same general muscular excitement in the body. Additionally, each feeling has a
special muscular effect which gives a distinct feeling to each emotion. He related the general and specific muscular effects to generalized and restricted discharges of neurons in the nervous system. Spencer assumed that small muscles were affected to a greater degree than large muscles by these emotional discharges; and that the muscles that controlled the voice and facial expression were, therefore, greatly affected.

Spencer then related his observations to evolutionary theory. The restricted discharges that gave rise to special muscular effects were produced in specialized neuromuscular connections which united the brain with the periphery in a precise manner. These specialized connections were established in the course of evolution, were subject to natural selection, and were passed on from generation to generation. For example, a specialized effect of anger, the frown, could confer an advantage upon men who fought in bright sunlight by reducing glare. It would lead to better survival and would therefore be selected and retained. Eventually, frowns might be associated secondarily with pain and other feelings. Similarly, the special effects of emotions on the vocal apparatus would have resulted in various sounds, specific for various emotions. Animals and humans alike would learn to recognize these sounds in others from their own prior experiences, and this would form the basis for a language. He concluded the chapter with a reassertion that the expression of feelings was explained by the evolution of neuromuscular
Although Spencer's precise theories and formulations about brain function have not been supported by later information, other theories about the biological basis for psychology continue to arise: from biochemical models of depression to the localization of one's sense of humor in the frontal lobes. Jackson was more naturally inclined to think in psychological terms than, say, Lichtein. Spencer provided him with a compilation of information from natural science that seemed to link the phenomenology of psychic function with the organic function of the nervous system. Like Darwin and probably because of Darwin, Spencer viewed this organic basis for psychological functions as a structure or structures that were subject to natural selection and inheritable. He did not talk about precise anatomy for these structures, and he seems to have preferred metaphysical terms. Jackson also had a nonlocalizationist, systems approach to language function. He may have become interested in studying emotional language because of Spencer's descriptions that tied the motor system to the emotions, an idea that Jackson pursued. He may or may not be indebted to Spencer for the idea of dissolution which, regardless, he used in a very different way. Finally, Spencer helped to introduce into psychology the linguistic notion of evolution whereby cries became words, words gave rise to more complex words, and whole languages gave rise to other languages. His view of this process is value-laden, with a
bias for what is historically later as being more complex and superior to what came before.

Although many important nineteenth century thinkers have been left out of our discussion, the ones we have discussed were at least representative of contemporary trends, and have clear relationships to some of the major figures in aphasiology. I have tried to show that Darwin's theories were pervasive, and helped to further the idea of localization of "higher functions" in the brain. I have also suggested that Schleicher's assimilation of Darwin's theories into linguistics and the more indirect introduction of these concepts into aphasiology helped to merge investigations of languages with the investigation of human speech. This merger is really a matter of confusion and spells the beginning of the end for localization: the new object of study created cannot be related to brain structure. Spencer, as a representative of psychology outside of medicine, was both a conduit of information from natural science and an inspiration for Jackson, who probably is less indebted to him for ideas than he is for inspiration. The crossing of these various intellectual forces made possible Freud's psychological schemes as well as Goldstein's attempt to fuse all that was known about language into a theory of disordered speech. At the same time, the definitive shift in thinking since the eighteenth century that now made speech and language topics more biological than philosophical allowed and encouraged
localization of specific functions.

**Saussure and the Beginning of Modern Linguistics**

Ferdinand de Saussure, who wrote little but was highly influential, marks our transition into twentieth century theories of speech and language. His *Course in General Linguistics*, compiled from students' lecture notes and published posthumously, is the best document we have for accessing his ideas. The lectures were given between 1906 and 1911, and came after his tenure as secretary to the Linguistic Society of Paris (1881-1891), during which time he established his reputation and held wide influence. As we will see, Saussure believed that written language and speech should be viewed differently, but he never actualized the "linguistics of speaking" that was apparently in his thoughts before he died (Bally and Sechehaye, 1966).

The book begins with a sketchy history of linguistics. His major point is that comparative linguistics, established by Bopp, followed from logical and philosophical language theories and became an independent science. It failed to explain much about language because the comparative philologists did not ask the meaning of their comparisons. Saussure said that in the mid-eighteen hundreds, scholars began to seek out the principles that governed the life of languages. Recall that Gesner, a hundred years earlier, had
had a similar approach, but Gesner was not discussed by Saussure. Recall also that the mid-nineteenth century was the time of Broca, Wernicke, Jackson, Darwin, Spencer, and Schleicher. In particular, Saussure referred to William Dwight Whitney (1827-1894), who began the process of looking at language as a product of the collective minds of linguistic groups. Whitney also discussed the arbitrary nature of linguistic signs, which Saussure developed later. Saussure objected to Schleicher's idea of studying language like an organism outside of speakers because this approach actually blended two separate aspects of language studies. Saussure defined precisely the subject matter and scope of linguistics: not speech (langage), but its derivative called language (langue) should be studied by linguists.

Saussure felt that the construction of a system of signs (not speech) is what is natural to man. He mentioned Broca's area as the center tied in with this ability. The executive side of speech (parole) reflects only each speaker's passive assimilation of language and is therefore imperfect and fragmentary. Language, on the other hand, is a self-contained whole and a principle of classification; it is the social product of speech, a subset of the collectivity constituted by the speech of all speakers in a group. In other words, if the speech of every speaker could be studied simultaneously, we would be studying more than just the national tongue that they speak. Since this is not possible, studying language is
preferable to studying speech in individuals, and it will yield different information.

We will only mention a few of Saussure's general principles, but these should be enough to show that his thinking departed from that of the comparative linguists and that it provides perspective on the way speech and language were studied by physicians. First, he said that a "linguistic sign unites not a thing and a name, but a concept and a sound-image" (Saussure, In: Bally and Sechehaye, 1966). Although this is consistent with the abstract way that Wernicke talked about sound images, we have seen that subsequent neurologists continued to assume that words stood for particular things, and to test for the capability of uniting names with things. Further, Saussure said that "talking to ourselves," what others had called "inner speech," was the psychological basis for sound-images. The combined concept/sound-image unit he called a sign: and he renamed the concept as signified (signifie) and the sound-image as signifier (signifiant).

Saussure is well-known for his discussion of signs. Briefly, he said that they are arbitrary in that there is no predetermined relationship between the signifier and the signified. They are immutable, in the sense that speakers in a community must follow the conventions and use particular signs for their conventional meanings. They are also mutable, in that the relationship between signifier and signified changes over time "under the influence of all the forces which
can affect either sounds or their meanings." So, the word
"gay," for example, might mean "happy" at one time and come to
mean "homosexual" at another. These principles of linguistic
signs inform the present discussion for several reasons.
Aphasiology never adopted the term "sign" to identify a
significant unit of speech or language, nor did its theories
ever take the arbitrary nature of the sign into account. Some
observers, like Jackson, focused on the proposition as the
basic unit of speech. Freud, overtly, and many others,
covertly, focused on the word. Head, we will see, proposed
the symbol or act of symbolization. Although symbols are
abstract entities similar to signs, Saussure and others
differentiate them by saying that the relationship between a
symbol and what it stands for is never arbitrary. Another
point of interest is the fact that aphasic patients lose the
conventional use of signs, yet they continue to use signs.
Although this phenomenon was recognized by most observers,
they did not systematically investigate the nature of the
signs that were retained. Thus, Saussure might have suggested
to, say, Lichtheim and Freud that they limit their
investigations to the executive side of speech, and that they
focus on the linguistic sign as the basic meaningful unit.

A second group of principles, beyond the discussion of
signs, is his distinction between synchronic and diachronic
language phenomena. A speaker's knowledge of his or her
language deals only with the current state of the language,
the synchronic principles that govern language use. Laws in synchrony are really principles of regularity, for example, "I before E except after C ..." Evolutionary or diachronic linguistics, which was the focus of the comparative linguists, deals with changes that occur in a language over time, but are limited; for example, the dropping of "final E" from many words in Old English. This distinction helps to focus the investigation of language along two different paths and makes manifest the different principles that govern them.

Synchronic linguistics deals with signs as real objects that stand in opposition to each other. Words, for example, exist in phonic chains, and are made up of sounds. In order to divide the sound chain into units, we have to consider meanings: if we divided the chain into syllables only, we would not grasp the linguistic entity. Saussure, therefore, defined the unit of language as "a slice of sound which to the exclusion of everything that precedes and follows it in the spoken chain is the signifier of a certain concept" (Saussure, In: Bally and Sechehaye, 1966:104). Yet, meaning exists even when we think silently. For Saussure, the difference between thought and sound is that the latter makes things more distinct. He admitted that it is sometimes difficult to delimit the meaningful units of language in an utterance or writing sample, "n'est-ce pas?" Signifieds (concepts) and signifiers (sound-images) do not exist outside of the linguistic system, but conceptual and phonic differences grow
out of the system itself. He insisted that "differences" apply only to the comparison between concepts or between sound-images; signs and other linguistic units are not different, but oppositional. This formulation suggests that a breakdown in the linguistic system would be very much like a breakdown in perception; an inability to separate concepts would be part of the problem. An inability to separate sound-images would appear as a problem with expression. So-called "positive symptoms," such as the recurrent utterances Jackson discussed, might be investigated to see if the differences between the terms of signs and the oppositional use of signs were retained. Such an investigation would presume that the speech of an aphasic patient could be viewed synchronically.

Saussure was unclear about the role of synchronic analysis in speech. He defined an entity called "syntagm," made up of the linear combination of terms which derive their significance by opposition to the terms that surround them; a sentence would be an example. Saussure said that sentences belonged to speech, not to language, but that other syntagmatic units belonged to language. For example, forced expressions like "let's see" are syntagms in a way that fits with language analysis. Sentences allow too much freedom of combination to be dealt with in the same way. In addition to syntagmatic relations, he defined "associative relations:" words that are associated with each other based on shared signs. Words evoke, nonsystematically, an indefinite number
of other words in indeterminate order. This process, like syntagm formation, is based on opposition between terms. In the process of speaking, "our memory holds the complex syntagms and we bring in associative groups to fix our choices. In each series the speaker knows what he must vary in order to produce the differentiation that fits the desired unit" (Saussure, In: Bally and Sechehaye, 1966:130). This distinction between syntagmatic and associative relations as methods for organizing linguistic units raises many questions relevant to aphasia: would particular situations calling for the syntagmatic method lead to different results in, say, a test of word fluency such as naming animals in the zoo, than a similar task using the associative method? Also, can the two methods be separated? Saussure said that all language functions should fit into either the syntagmatic or the associative class, so he would probably answer yes to both questions.

Saussure's synchronic analysis predicted that the arrangement of the subunits of words would obey the same fundamental principles as the arrangement of groups of words or phrases. Although grammar includes morphology (which deals with different classes of words and inflections) and syntax (which deals with the functions attached to linguistic units), the division between the two is illusory. Grammar merges with lexicology. This view, like many of Saussure's, did not find its way into aphasiology, which still talks about a brain
"lexicon" and the various processes that are responsible for acting on the words in the lexicon (10).

Before concluding our discussion of Saussure, I will just mention diachronic linguistics as it relates to speech. Phonetic evolution, alternation, analogy and agglutination are terms he used to discuss the changes that take place in linguistic signs over time. All of these processes occur in speech because of the changes wrought by individual speakers. Speech is, therefore, "continuously engaged in decomposing its units." However, the diachronic changes are somehow irrelevant to speakers using the language. He suggested that literary language, which preserves language in writing, gives us the feeling that there is a "right" and constant language that we speak. Diachronic considerations, thus defined, would not offer much to aphasia studies. On the other hand, an amazing and exasperating finding in nearly every patient is the almost daily change in aphasic speech, particularly following an acute causative event. Without studying these changes separately, they cannot be taken into account. Usually they are subjectively "averaged" by the investigator and reported in the midst of a synchronic analysis. Saussure's views would argue against this approach.

Saussure did not view linguistic signs reductively, like Broca and the diagram makers, or purely psychologically, like Jackson, Head and Freud. Linguistic phenomena, for him, occur because "a complex acoustical-vocal unit combines with an idea
to form a complex physiological-psychological unit" (Saussure, In: Bally and Sechehaye, 1966:8). Jonathan Culler points out in his book on Saussure that the unconscious is a necessary concept in linguistics, which assumes that every speaker understands his/her language in a way that he/she cannot articulate (Culler, 1976) (11). Jackson, when he discussed automatic speech, also acknowledged an unconscious function for language. Although Lichtheim, Freud and Saussure were contemporaries, Saussure's work does not seem to have influenced them. Freud, it is well-known, went on after his aphasia work to develop a theory of the unconscious which is really a departure from his monograph on aphasia and uses little linguistic evidence. He did not even mention paraphasias when he discussed slips of the tongue, although this would be a natural comparison, indicating how widely he had diverged from linguistic concerns. It may be the case that his aphasia work was largely based on texts, as opposed to his psychiatric work which was based in speech. Probably, Saussure's influence was greatest within linguistics, where we can clearly see his position of transition from Schleicher to Sapir, who will be discussed below. Although aphasia studies did not take a Saussurian turn into semiotic analysis, they have crossed with linguistics many times subsequently, and the field of linguistics was radically altered by Saussure. Although this discussion is too broad to enter into presently, I will suggest that current investigations which seek to apply
the old functional localization methods to linguistically defined phenomena such as agrammatism or which look at particular classes of words by the same means might benefit from an historical view that included Saussure's far-reaching analysis of the system of signs. Such an historical view would more likely explain the failures than it would create new successes for these investigations.

The Sapir-Whorf Hypothesis: Head and Goldstein

As we discussed in Part I, Head and Goldstein redefined the subject matter of aphasiology. They tried to systematize observations of speech and, in doing so, had to include many behaviors not previously considered part of speech. Saussure's redefinition of linguistics resulted in several precise and often mutually exclusive categories, whereas the work of Head and Goldstein was inclusive and eclectic. Even more importantly, Saussure separated speech from language studies, which is more easily done when studying texts than when studying patients. The speech/language dichotomy that he introduced would later slip into aphasiology when it turned to linguistics for models to test localization of functions, and language, rather than speech, would quite unconsciously become the object of study. Even though Goldstein and Head were not primarily interested in localization, their emphasis on testing as a means for classifying language behaviors has been
absorbed into the localization wave that crested with intraoperative cortical stimulation and the development of noninvasive neuroimaging techniques, such as computerized tomography, magnetic resonance imaging and positron emission tomography: the result has been an attempt to localize all testable functions by means of sophisticated technology.

Although many patients do not fit the anatomical models, and diverse localizations are discovered in association with the same behavioral deficit, and deficits change over time in a way that would suggest that their anatomical correlates have changed, no alternative approach has seriously challenged the localizationist approach. On the contrary, there is an almost overwhelming bias toward retaining it because aphasiology has traditionally been part of neurology, and localization has been both necessary and successful for non-behavioral neurological conditions. Yet, when reading theories about language from outside of neurology, I am struck by the feeling that post-structuralist critics, linguists, and other behavioral researchers do not discuss speech and language as they are conceived by those dealing with aphasia. The neurological view may be hampered by having nothing more than the "unconscious," native speaker’s intuition for a model of normal speech/language; and no clear differentiation between speech and language or awareness of the perceptual biases inherent in our own languages that actually cast the logic of our arguments. Yet, the portrayals of speech and language
outside aphasiology, on the other hand, seem to have lost the cogency of real speech and do not deal with the systematic derangements of speech that result from brain damage. Although linguistic terms have consistently found their way into aphasiology, terms like "semantic," "syntax," "phoneme," and even "discourse," are used in a limited, non-theoretical sense to fit into traditional models. With these points in mind, I will look at Sapir (1884-1939) and Whorf (1897-1941) to see how their theories of the interrelationships between personality, language, culture and thought might pertain to aphasia. This should round out our view of the formative years in linguistics, when certain psychological dimensions gained force and credibility.

In his book called Language, Sapir, a contemporary of Head and Goldstein, stated that thought is the "highest latent or potential content of speech" (Sapir, 1921). Language, primarily an auditory system of symbols, is made up of sequences of sounds: words, parts of words, and word groupings. Sapir said that it is impossible to define a "word" from a functional standpoint. Words must, therefore, be formal units of speech with psychological validity that "take on as much or as little of the conceptual material as the genius of the language allows" (Sapir, 1921:33). From Sapir's example, a Paiute Indian of Utah might identify the following as a word: "wii-to-kuchum-punku- rugani-yugwi-van-ntu-m(u)," which would be roughly translated into many words
in English: "they who are going to sit and cut up with a knife a black cow (or bull)." Grammatical elements and sentences are the functional units of speech and occur universally. Although he thought that all language retained propositions made up of a subject (concrete cluster, something talked about) and a verb (activity cluster, something to be said about the subject), the various parts of speech vary from language to language. Even within a language, parts of speech can be converted into each other; for example, this is Terry's briefcase - Terry carries his papers in this case. This syntactical malleability and the occurrence of different parts of speech in different languages show that they are not intuitive, biologically predetermined entities, but conventional forms for thinking.

Sapir thought that linguistic research yielded six types of grammatical processes: word order; composition (uniting radical or root elements); affixation (prefixes, suffixes, infixes); internal modification of radical or grammatical elements; reduplication (repetition of part of a word); and accentual differences (word stress and pitch). Examining these processes, he concluded that every proposition expresses concrete concepts (objects, actions and qualities) and relational concepts (ideas that relate the concrete concepts to each other, like time, reference, number). We will not review his analysis of these concepts by which he divided all languages into four classes because his system is no longer
used and is tangential to our discussion. We will mention, however, that he viewed word stress as a controlling influence in the formation of words and word groups out of sound sequences. This attention to stress helps to make Saussure's discussion of syntagms more relevant to speech; sequences of sound, combined with stress, give rise to syntactic relations. We can go between two people or act as a go-between and word stress helps to create the grammatical categories and therefore the meaning in each case. In other words, stress breaks up the syntagmatic chain to express relations that simultaneously take on a grammatical form. We create the breaks that we are accustomed to in our expressed and perceived speech in order to make sense.

Sapir, unlike Jackson, Head, Goldstein and Saussure, gave a definition to the term "proposition:" a concrete, subject-cluster and a verb activity-cluster. His simple, language-based definition is common-sensical enough to apply to speech and may be universal enough to apply to all languages, although this point is still debated. All of these men agreed that the proposition was an essential part of speech and/or language. It might be interesting to reexamine aphasic speech in light of Sapir's definition and see if different aphasic groups, defined either historically or anatomically, have predictable difficulties with the different parts of the proposition. As linguists investigate how different types of words function within the subject and verb clusters (for
example, the integrity of roles, the modification of verbs, the agreement between verbs and participants with respect to roles, etc.) it would be interesting to see how aphasic patients manipulate these words and functions. Sapir, by making the proposition definable, allows an approach to observing language that might or might not relate to traditional observations of aphasia, such as naming fluency, effort, syntactical integrity, etc.. Even more importantly, his argument that parts of speech are conventional forms of thought rather than biologically predetermined entities is not as easily compatible with Darwinian notions of inheritable brain structures as the basis for linguistic abilities. For Schleicher, writing at the time of Broca and Darwin, words and languages could evolve in parallel with organisms. A one-to-one correspondence between a grammatical ability and a brain structure was not out of the question (12). Language universals, although not addressed by aphasiologists, were often assumed - for example, the importance of the "word" and therefore, the probability that particular words were stored in particular places, even in particular neurons, as suggested by Freud. With Sapir's discussion of syntactic malleability, another level must be posited: if structures in the brain can account for the forms of a language, there must be more than one manifestation for a given structure. We begin to arrive at the kinds of functionalist theories later developed by Luria and others, which will be left for a future discussion.
Sapir is perhaps best remembered for a later collection of his work, *Culture, Language and Personality*, in which he put forth his contribution to the Sapir-Whorf hypothesis: language interpenetrates with as well as symbolizes experience. Its forms predetermine our modes of understanding and interpretation (Sapir, 1957). He also noted that there is no general correlation between cultural type and linguistic structure, although vocabularies do seem to be sensitive indicators of cultures, since they reflect the distinctions made by the people. Language, because it allows individuals to abstract from common experience, yields a common understanding of things which constitutes culture.

Benjamin Lee Whorf, after getting his engineering degree at M.I.T. and studying languages on his own, including Hebrew and Mayan, met Sapir and enrolled in his course at Yale (Carroll, 1956). Like Sapir, he concluded that the structure of one’s habitual language influences the way one understands the world, the principle known as linguistic relativity, or the Sapir-Whorf hypothesis. Although all of his work is fascinating, we will focus on those pieces that went into the Sapir-Whorf hypothesis or are otherwise pertinent to aphasia studies. Whorf was at his best when comparing languages to show how they lexicogrammatically shape perceptions. He was an ideal proponent of the American Indian language because he was a scientist and could dispel the not so hidden bias that inhabitants of "advanced" and "scientific" societies are
better equipped by our languages to deal with scientific phenomena. So, when Whorf suggested that Indian languages were not primitive or inferior and were, in fact, superior to English and other European languages for representing certain complex aspects of reality, like relativity, he was difficult to discount.

The Standard Average European languages including English, French, German, and other European languages (except perhaps the Balto-Slavic ones), are the principle languages in which aphasiology developed. Whorf compared them to Hopi Indian and other American Indian languages with interesting results. SAE views time as a kinetic, one-dimensional perpetually flowing line within space; a static, three-dimensional, infinite region. This results from our grammar, in which time is either past, present or future, which gives the feeling of linearity; of seconds marching on into minutes, hours, and days. Hopi, on the other hand, has three different assertions; a past-present, or factual; future; and generalized or usitative. The speaker conveys through these assertions that he/she is either reporting or expecting something. Everything manifested, whether in the past or present, is in the factual assertion with its grammatical mark. Hopi grammar does not differentiate past and present; therefore, the distinction is irrelevant. Everything manifesting or subjective is either going to happen in the future or exists in someone's feelings and is, therefore, not
treated as fact. SAE has analyzed reality largely in terms of "things" (bodies plus quasibodies) and modes of formless existence called "substances" or "matter." Whorf pointed out that non-spatial existents, like thought, are imaginatively spatialized in these languages, so they can fit into the grammatical category of "things:" I may "grasp the thread" of an argument or find it "over my head." If I "lose the drift" of the conversation, my interlocuter and I "remain far apart." This spatialization tendency is a trait of all Indo-European languages and was even prominent in Latin. Hopi analyzes reality largely in terms of events, rather than things. Its grammar selects events that are objective - physical events or percepts, for one grammatical category. Since time and space are both objective in Hopi, something that is "distant" is removed in both time and space, and it places physical and nonphysical/abstract events in the subjective category. In SAE, time passes; in Hopi, time grows later, therefore, it accumulates. Whorf related this difference to the observation that repetitive activities seem useless to us, but the longer they go on, the more powerful they seem to the Hopi.

Conceptions of time vary more between cultures than conceptions of space. Whorf felt that the figure-ground concept of visual perception was universal: all observers divide a scene into figures and ground and then translate this perception into their own languages. Otherwise, he denied that the mind perceives and conceives the same in all
cultures. These beliefs raise the question of where in the nervous system external information is organized. Contemporary neurophysiology theorizes that the organization of visual information begins in photoreceptor and ganglion cells of the retina that respond to different bits of the visual field with an "on center" or "off center" signal. This information is organized to "center-surround" information, which is combined with information from the opposite side of the visual field at various levels on the way from the retina to the cortex. Finally, the cortex organizes information from zones of the visual field with further right-left information (to give depth perception) and information about color. So, the figure-ground universal that Whorf talked about would be a cortical level percept. With respect to language, external information is presumably also organized at various levels of the nervous system before the language cortex casts it into a culturally unique, lexico-grammatical form. The question remains, if the physiologic processes leading up to a percept are the same, as they seem to be in the visual system, why are ultimate conceptualizations different for different languages? In other words, if one wishes to use neurophysiology to explain language, conceptualizations of time and space, figure-ground relationships or other higher cognitive functions, one quickly meets with limitations of the model.

Returning to Whorf's cross-cultural analysis of language, he noted that grammatical categories are not defined by
morphology alone. In English, for example, we can form the plural by adding "s" as in "dogs," or by internally modifying a word, as in "geese." Some covert classes of words do not have a linguistic mark at all. These so-called cryptotypes are sensed by speakers, and vary from language to language. In English, "up" can be used with some cryptotypes of verbs to mean "completely, to a finish," as in "clean it up." However, it cannot be used with other cryptotypes, such as the verbs signifying "dispersion without boundary:" we cannot say "scatter it up." Categories are not defined by function alone either; everything that modifies a noun is not necessarily an adjective.

Although grammatical categories are not universal, Whorf initially asserted that the subject/verb distinction was. Later, after he studied Wakashan languages (Nitinat, Nootka, and Kwakiutl), he de-emphasized this universality to say that the usual utterances in these languages all looked like verbs (13). He gave an example of how this might seem, a translation from Nitinat to English that he paraphrased: "There is one who is a man who is yonder who does running which traverses - it which is a street which elongates," or more familiarly, "a man yonder is running down the long street." Because of the way English segments reality, it encourages us to think in "terms," so that "sky," "hill," "swamp" and "wave" are all treated the same as "table and chair." Hopi, on the other hand, has no word for an
individual wave, but talks about "waving." English divides words almost randomly into nouns and verbs, whereas Hopi does so based on duration. Anything brief is a verb, like lightning, wave, flash: longer phenomena are nouns, like thunderstorm and cloud. English is so wedded to the idea of subject plus verb that we create subjects when we feel the necessity: "it flashed," instead of the Hopi term "rehpi," meaning simply "flash (occurred)." Furthermore, we have a tendency to make all events into action verbs, even static ones like "I hold." It is no wonder that Aristotle made the noun-verb distinction the basis of "reason itself (Whorf, cited in Carroll, 1956:24)." The two group notion of subjects and the events that are dependent on them even shows up in our mathematics with entities like $x, y, z, 1, 2, 3,$ etc. and operations: $+,-, \times, \log,$ etc.. Perhaps this helps to explain why calculations are impaired along with language abilities in aphasics.

It is interesting that Goldstein did not mention Whorf in his 1948 book, although he mentioned other linguists, like Jakobson. Whorf wrote both scientific and popular essays about linguistics in the early forties that must have been available to Goldstein. Consideration of his ideas and the Sapir-Whorf hypothesis might have resulted in a significant change in perspective for aphasiology. First, aphasialogists might have seen that their own languages encouraged a localizationist view for language functions because of the
tendency to spatialize abstract ideas. Secondly, they might have asked what, if any, language behaviors were universal and studied their relationship to speech. This would probably have lead back to scrutiny of the proposition and propositional speech and away from a focus on naming and nouns in general. Although rigorous testing would probably have remained the chosen methodology, different tests might have been developed. Thirdly, investigators might have asked questions about the recovery and rehabilitation of aphasic speech. For example, if the culturally based strategy of conceptualizing in terms of "things" and "substances" was disturbed, would other strategies be operating and could they be exploited for communication? Finally, they might have gotten away from the tendency to view specific language functions as having specific locations, and resisted total submersion into localization theories with the advent of imaging technology.

CONCLUSION

It is always difficult to conclude a history. I have tried to show that aphasiology has developed from scattered observations of speechlessness to a selective universe of discourse, which gives us insight into speech through observations of its disturbances. Most of the syndromes and symptoms of aphasia were described by 1800. During the
nineteenth century, aphasiology was overwhelmed by the discovery that the left side of the brain functioned differently from the right. This pathologically confirmed observation, together with Darwin's evidence for evolution of higher functions, established the search for localization of specific aspects of speech/language that corresponded to inherited structures. Even those neurologists who were against localization, or not specifically for it, retained the bias of Standard Average European Languages when they wrote and lectured about speech: they spatialized their discussion of abstract language-related concepts, and their ideas were, therefore, gradually and easily assimilated into anatomical modes. Psychology also blossomed in the nineteenth century, and the more complex formulations about psychological behavior, which derived from men like Spencer, ultimately forced reexamination of the subject matter of aphasiology. This, in turn, lead to an emphasis on neuropsychological testing that has persisted into the present. Currently, in fact, neuropsychology is a self-contained field, separate from Neurology, with standardized testing procedures and standardized test batteries for most recognized human behavioral functions.

The anatomical model for speech, developed fully before 1900, is still retained today. Although many of the cases used to construct this model did not fit into it, the model seemed necessary to deal with those instances where particular
deficits did occur with damage to particular brain regions. Since there is no way to avoid entering the debate about localization, I will say that the left hemisphere clearly has a different role than the right; and that some speech deficits can be localized. This tells nothing about how normal speech is made and understood, and I think we are more likely to learn these answers by studying how normal and disordered brains organize information than we are by localizing deficits. It seems to me that it is valuable to be able to think in terms of primary sensory and motor functions, such as vision, somatosensory function, audition, and voluntary movement; but that even these functions are not localizable when they are involved in more complex processes, such as naming objects placed in the hand. When it comes to localizing speech elements, we collect exceptions more often than we do instances of the classic, anatomically-defined syndromes.

The process of developing the predominant model for aphasia was obviously not straightforward and smooth. At all times, there were those who dissented and succeeded in making a contribution to the process. Since the anatomical diagram model was not established before the nineteenth century, there was no significant aphasia dogma to dispute. Gall, however, went against another majority by trying to impart control of moral and intellectual faculties to individual human brains, rather than to divine will. His work, despite falling into
disrepute, made possible the debates entered into by Bouillard and Broca. Broca, initially opposed to those who more or less followed Gall's views, ended up founding the anatomical model. His interests, then, merged with the localizers. Jackson, it seems, did not actually oppose the model: he ignored it and decided to study behaviors whose classification came from psychology, rather than from aphasia theory. His theories included localization-based concepts such as brain duality, but his primary interests lay elsewhere. Meanwhile, the English and German diagram-makers kept incorporating cases and autopsy studies into their model, trimming the cases and adding connections between centers as necessary.

The twentieth century contained Lichtheim's refinement of Wernicke's diagram model and Freud's dissension, based on analysis of Meynert's work; exceptions that could not be explained by the model, and his own theory of how the brain functioned as a whole. The debate was carried on by Dejerine, representing localization, and Marie, who more than anyone before him challenged the tenets of localization directly: in one paper he said that there was no Broca's area, based in part on reexamination of the brains of Broca's two famous patients. Head borrowed methodology from Marie, but was not so utterly opposed to localization. In fact, he ended up with very precise anatomical locations to explain his four types of aphasia. Head and Goldstein were most original in their use of testing, and their willingness to include a wide array of
behaviors under the heading of speech and language. Rather than fitting with one school of thought or the other, they represent a culmination of both.

Linguistic theory, at least since Saussure, has suggested that language is a different phenomenon from speech, although many people, from Schleicher in the nineteenth century to neurologists in our own, have tended to ignore this. Speech and language are obviously related and need to be theoretically compatible. Yet, I am wary of attempts to take information about "language" and apply it to the brain. Generalizations about language should be examined first for their implications concerning speech, before they are used as probes to discern "language function" in the brain. Concepts like agrammatism, for example, are not necessarily relevant to speech, since much speech is not grammatically correct in the normal situation. Although it is probably useful to test for the systematic derangement of lexico-grammatical elements in certain brain disorders, this should only be done in those situations where the normal behavior is predictable or discourse-based. Language theories outside of neurology, if carefully applied, could conceivably change some of the speech behaviors that are studied within neurology, and even the classification of aphasia. Finally, it would be helpful if aphasioligists recognized the biases inherent in their own languages, an application of the Sapir-Whorf hypothesis, and if they took Whorf's approach of thinking in terms of language
universals, rather than specific grammatical categories. These changes could begin the process of relating speech to brain physiology, phenomena whose forms are currently incompatible.

There is another topic which I have only alluded to because it requires a complete and separate discussion. Most of the aphasic behaviors described in the years during which aphasiology blossomed (1600-1900) are literate behaviors: things you would notice in people who read and write. By the time Lichtheim created his model, these literate features had assumed reified status -- there were reading centers, writing centers, etc.. But there is evidence that literacy itself shapes cognition, and that even non-literate who interact with literates experience these influences. Contemporary models of language in the brain not only favor "language" (in its many forms) over speech: they also assume that the literate situation is the only mode of cognitive operations, which cannot be the case. Prior to the spread of universal literacy, early descriptions of aphasia had to do with human speech. Subsequently, our observations have been conditioned by literacy, as much as they have been conditioned by our amorphous objects of study and by the languages of our research.
1. Gustav Dax, in 1865, claimed that his father had established the case for left hemisphere language dominance in 1836, long before Broca. This may be true, but Broca apparently discovered it independently. For a full discussion of the controversy, see Joynt, 1964 and Henderson, 1990.

2. We now know that bilateral injury of the auditory pathway, anywhere from the cochlear nucleus in the lower pons to Heschel’s gyrus, is necessary to produce total deafness.

3. Jackson’s differentiation between automatic, emotional speech and voluntary, intellectual speech remains interesting. He said that the former is more preserved in aphasics, consistent with cortical pathology. The inverse may be the case for patients with Parkinson’s disease and other primarily subcortical processes that seems to affect emotional speech more than intellectual speech.

4. Lichtheim did not use the term conduction aphasia, but the category he described is now known by this name.

5. Subsequent neuroscience findings are more consistent with Meynert’s views about cortical representation,
particularly in the visual system, than with Freud's. The visual field is mapped precisely in the visual cortex, with macular vision at the occipital tips, superior visual field below the calcarine fissure, etc.. Other areas of cortex also show somatotopic organization that corresponds to subcortical areas, like the pyramidal tracts and red nuclei, as well as to the periphery.

6. The features of abstract attitude defined by Goldstein are currently attributed to frontal lobe function. In the current localizationist atmosphere they are viewed separately from speech and language by most authors.

7. Geschwind pointed out (Geschwind, 1964) that Goldstein's theoretical formulations in Part 1 are general enough to encompass any anatomical theory. I agree and have therefore taken the liberty of merging his clinical descriptions from Part 2 with his theoretical groupings, which involves a small amount of editing.

8. Actually, Pierre Marie, whom I have not discussed at length, claimed in the early 1900's that there was only one true aphasia: Wernicke's type, due to involvement of the peri-Sylvian speech cortex. Those patients who, in addition to comprehension difficulties, had problems with articulation also had a lesion of Broca's area or Marie's
"quadrilateral space" just deep to it. So, Broca's and Wernicke's aphasias are not strictly parallel in his version either, and his formulation is conceptually related to Goldstein's.

9. Currently, many linguists would identify three types of paradigmatic lexemic relations: those based on signs (like a common root or radical); those based on phonetics; and those based on semantics. Saussure merged the categories of sign-based relations and semantic based relations in his category of associative relations. See Davis, 1973 for further discussion.

10. It is important to note that Saussure only included lexemes and compound words in his conception of morphology. Sentences, now often considered in discussions of morphology were excluded from langue. Also discussed in Davis, 1973.

11. Although Culler makes this statement, with support from psychoanalytic theory, Saussure might not have agreed. The notion of linguistic competence, as Saussure used it, depends on what the speaker is aware of as a test for what is "correct."

12. Nor is it out of the question today, as researchers
suggest "behavioral genes" that are said to account for specific language dysfunctions, such as an inability to apply rules of grammar (e.g., making nouns plural or making verb tenses past). See Gopnik and Crago, *Cognition*, 1991;39:1-50.

13. Although the simplist utterances look like verbs, they are still composed of lexical units modified by prefixes and suffixes and particles that all function like other parts of speech within the verb-like structure.
SITUATIONS

Situated Knowledge
Situated Discourse
Situations Beyond Your Control
Situation Comedy
Academics often discuss discourses, which is different from our discourse practices. Post-modern anthropology, in descriptive modes, provides compelling critique of our representations, institutions and behaviors based upon the deconstruction of ontology and epistemology. But post-modern anthropology is problematic in its practice modes. Even, and maybe especially, post-modern feminists are likely to be critical of the practice aspects of postmodernism. Post-modern practices seem ambivalent about systems, whereas feminist and scientific anthropologies use them to advantage. Although it is an easy target for many forms of criticism, science may be a more successful practice because it resists the totalization of meaningfulness.

**THIS IS WHERE SCIENCE COMES IN**

Montaigne wrote about science in the sixteenth century:

"Whenever a new discovery is reported to the scientific world, they say first, 'It is probably not true.' Thereafter, when the truth of the new proposition has been demonstrated beyond question, they say, 'Yes, it may be true, but it is not important,' Finally, when sufficient time has elapsed to fully evidence its importance, they say, 'Yes, surely it is important, but it is no longer new'."

This particular illustration of the value placed on discovery, truth, and novelty in science is nice because it also alludes
to the skepticism inherent in scientific practices. Philosophy of science, social studies of science, and postmodernism have provided compelling critiques of scientific practices and representations. Those of us who believe that there is no Truth discovered by scientific methods and that so-called facts are always socially constructed have a lot of difficulty deciding how to write grants, what counts as data, and how to represent our results in papers. We are engaged and distracted by the relationship of rhetoric to power, the relationship of money to the creation of knowledge, and by gendered aspects of credibility, among other things. You will notice that most of us do not quit our professions, however. The representational aspects of science are a lot more troublesome than the process of practicing some version of it, and this intrigues me.

**NEUROLOGY IS SOMETHING THAT I PRACTICE SOME OF THE TIME**

To do so, I engage with writings based on scientific research, as well as with people in a communicative discourse that is very important to us. It is similar to the discoursing that goes on in fieldwork, but, in my opinion, more balanced. I am aware of medical anthropological critiques of medical discourse, as summarized in Joel Kuiper's work for example, (Kuipers, 1989) but now I am talking about my practice and my situated discourse. Generally, patients
come to see me because they are bothered, usually by something that could be called "cognitive" (for lack of a better word), since I practice behavioral neurology. In the process of understanding what they tell me, investigating what we decide are problems, relating the results of the investigation to other information, and deciding what to do, I find myself dealing with some of what I am told and not with all of it. Some published research studies seem useful for our purposes, and some do not. Some of the patients' statements seem important and some do not. What is and is not important will also change over time. Worse yet, I am consistent in my responses.

Postmodernism has taught me that scientific representations are totalizing, and that systems and structures, for example, are always founded on what they exclude. Derrida's notions of decentering and deconstruction are propelled by using what is excluded to take apart the system or structure (Derrida, 1978). Stephen Tyler has written that science, structural anthropology, and linguistics are all formalisms, divorced from the world and from speakers (Tyler, 1978). Even some medical anthropology reenforces the notion that everything must mean something, and any attempt not to deal with it all is somehow wrongheaded (Romanucci-Ross and Moerman, 1988). But if you try to put this into practice, you find that you cannot act, at least not in neurology, although I suspect this applies to other practices, such as
fieldwork as well. Science is a convenient label for the sorting out process by which sense is separated from nonsense. Postmodernism, like other hermeneutics, would seem to imply that there is something wrong with sorting sense and non-sense, and I will call this implication the totalization of meaningfulness, because it allows no way to escape from the situation in which everything means.

I do not really think that there is something hopelessly and especially wrong with all science practices, which is really a different statement than the ones I have made about scientific representations. But I am convinced that there is at least one important difference between scientific thinking and not-scientific thinking that has to do with whether or not everything matters, carries significance, signifies, counts as good data, or however you want to put it. Practicing science means spending a lot of effort deciding what is "real" which could be read as "useful", and what is artifact or just the product of chance, which means that some of what scientists do they consider insignificant. Not-science, like much of psychology, and art history, and philosophy, and anthropology (when it is not trying to be like science) assumes that everything matters. Even slips of the tongue. Even what goes into a collage, or remains unsaid or is otherwise absent but suggested by a trace. Derrida addresses the issue of too much meaning in some of the works he critiques, such as Freud's and Levi-Strauss's, although he does not acknowledge the ubiquity
and totalizing tendencies of meaningfulness in his own work (Derrida, 1978). Derrida might see the excess-trimming practices of science as empiricism, but if they are empiricism, the term needs to be understood oppositionally, in a pseudo-derridian chain: sense/non-sense, not-science/science, dissemination/empiricism, deconstruction/bricolage.

I have already discussed how sense, as meaningfulness, goes along with not-science, and how the indulgence of non-sense is practiced in science. Dissemination is the term Derrida uses to describe the changing that takes place after what he calls an oppositional hierarchy is overturned, such as the one in which writing is subjugated to speech as a somehow secondary form of language (Derrida, 1981). I find this concept highly useful, but no more entertaining than empiricism when it is chopping away at experience to fit it into representations. Similarly, deconstruction, in which arguments, systems, or structures are dismantled in their own terms is no more right or wrong than Levi-Strauss's notion of bricolage, where one uses the imperfect means-at-hand in a process of trial and error, for your present purposes.

I am utterly changed and convinced by my encounters with postmodernism through postmodern anthropologies. My personal life; family relationships, aspirations, what I call ethical practices, are all different. When I answer my 3-year-old daughter's questions about life, my primary aim is to suggest that things are not fixed: definitions of words vary; gender
configurations are multiple, and left up to her imagination; explanations of causality have alternative explanations, equally tenable. I do not think, read, teach, or conduct research the way that I used to, because my sense for the meaning of my actions is altered. But despite these major differences, my practice of clinical neurology has hardly changed. Is this a "blind-spot", as some who like visual imagery might suggest? Or is postmodernism better at making sense than it is at informing non-academic practices? Medical practice requires you to proceed with some desire to ignore, side-step, or defer some sense-seeking constructs in favor of others, and postmodernism is uncomfortable with these moves. A lot of arguments over postmodernism have been worked out on paper, but this is not an entirely textual debate, either in the narrow sense of text-as-written documents, used by Steve Sangren (Sangren, 1988), or the more convenient extension of text which includes "all" of a particular discourse. Some have taken the problematic of postmodernism with them into traditional areas of area studies, also known as fields, and some have acknowledged it in their situations at home.

TAKE FEMINIST ANTHROPOLOGIES, AS EXAMPLES

And here I rely on Donna Haraway, Dorinne Kondo, and those who wrote the book by Linda Nicholson called Feminism/Postmodernism. Most feminist theoretical positions try to
avoid essentialism; and they deny the supposed authority and neutrality of the academy in favor of more situated ways of knowing. Feminist theorizing sometimes shares with postmodernism a skepticism about the unity, fusion, and coherence implied by ideas of community. Skepticism about identity and community, then, includes skepticism about a community of individuals united by political purpose, just as Steven Tyler has made us skeptical about whether communities share linguistic rules, morals, values, or culture (Tyler, 1987). Nicholson expresses some worries that postmodernism may destabilize certain feminist desires (Nicholson, 1990). Does postmodernism, by pointing out deconstructed ontology, erase the body and the senses? Do the prohibition against essentialism and the assertion that there is no fixed truth do away with coherent politics? Does decentering the self make gender irrelevant?

In her attempt to engage disparate types of feminist science, Sandra Harding distinguishes feminist empiricism from feminist standpoint theory (Harding, 1977). Feminist empiricists act as if strict adherence to the scientific method, with only mild modifications to acknowledge that women are doing some work, and men are doing something else, will overcome the mistaken gender biases that currently exist in science. Standpoint theorists, on the other hand, begin with the assumption that science is grounded in gendered activity and social theory, which will influence the conduct of the
science as profoundly as the choice of scientific questions. Although these labels, empiricism and standpoint theory, work better in theory than in describing what people actually do, they offer one way to explore potential alliances between feminist science and postmodernism as a hypothetical construct.

Feminist empiricists, or those who lean toward empiricism, probably have to reject postmodernism as an impossible practice. Postmodernism suggests that one topic is as good as any other; that all scientific methods are just the search for so-called hidden truths; that scientific knowledge is as socially constructed as any other representation; that scientific representations are formalistic totalizations which exclude significant alternatives and meanings. Practitioners of scientific anthropology, with their reliance on cultural systems, data, methods of proof and formal determinations of significance would seem more compatible with feminist empiricists than with postmodernists (Polier and Roseberry, 1989).

Feminist standpoint theorists can probably work things out with postmodernism. Already there is agreement on the social construction of so-called facts, the illusion of epistemological truth and the problematic of locale, which must always be negotiated. The provisional nature of identity could be a little more controversial, but some compromise could probably be found. Also, the political agendas of such
standpoint theorists remain separate from postmodernism in general. Still, Harding suggests that feminist standpoint theory is already responding to postmodernism in its practices (Haraway, 1985).

Some feminists who consider themselves outside of Harding's groupings, Sharon Traweek and Dorinne Kondo, for example, are more than a little sympathetic with postmodernism as I have characterized it (Kondo, 1986; Traweek, 1988). It has changed their lives and their work. It is a matter of life and death. They are even so radical as to turn a postmodern critique back toward their more established male colleagues and ask how it is possible to subscribe to postmodern descriptiveness without having it alter your non-academic lives. Their critique is, of course, a political one, since politics is about what you do and do not do, and the relationship between what you do and what you say.

My point is that feminist postmodernists will use postmodernism, like gender, as a deeply self-shaping and political practice, which raises certain questions. If postmodernism is a set of practices as well as a descriptive mode, can you practice it sometimes, but elect not to at other times? And what is the relationship between postmodern practice and other practices that you might engage in, such as gender construction, assertions of identity, fieldwork, teaching, etc.? Postmodern descriptiveness is an effective, polemical, and often text-bound endeavor. So I have used a
discussion of feminism to move from description to the arena of postmodern practices.

In the beginning of this paper, I made the point that discussing discourses is different from discourse practices. Now I am discussing and describing some postmodern, feminist, and scientific discourses. I hope to convince you, if you are not already convinced, that postmodern anthropology offers important critique in its descriptive mode. At the same time, I hope that the examples of feminist discourses and the more uniform discourses of scientific anthropology and medicine will begin a discussion of postmodern practices. Living with gender is a systematic political practice. Scientific activities make use of systems, and other representations in order to exclude some things and include other things in the process of making sense out of non-sense. Postmodernism is, so far, better understood in terms of its descriptions than in terms of its practices. Many of its perpetrators are ambivalent about systems, and I think this is related to the notion that their perpetrations have been more successful with description than with practices. It is easier to critique ethnography than to write a postmodern ethnography: It is much easier to practice postmodernistic academics than to be conscious of postmodern ethos in the practices of everyday life.
You might wonder which postmodernism I'm talking about

By invoking postmodernism, do I mean a political movement of academic careerists—George Marcus, Mehti Abedi/Mike Fischer, Sharon Traweek, Steve Tyler... my teachers at Rice (Marcus and Fischer, 1986; Fischer and Abedi, 1990; Traweek, 1988; Tyler, 1987)? Or maybe I am referring to literary-critical, Jim Clifford-like texts (Clifford and Marcus, 1986), or to Paul Rabinow and self-reflexivity in general (Rabinow, 1988). Maybe you believe that all postmodernism takes place in texts, as Steve Sangren once charged, (Sangren, 1988) or that the term is better left as the native postmoderne, acknowledging a condition that is untranslatable. Some of you may hope that, for the purposes of communicating with each other, I do not consider postmodernism some sort of an evocation of a trace of the unsaid, which would not aid your comprehension of me. Although I have been accused of plainspeaking, which is bad because it is transparent and pretends to be clear, I am going to spell out my take on postmodernism anyway. It cannot be easily summarized: reference Lyotard, Haraway, Derrida, Tyler and Culler. Most would agree that it cannot be defined in general, but must be described within a particular discipline or practice as both a period after modernism in that discipline; and as a construct without temporal linearity that is informed by postmodernisms in other disciplines for the critique of
current practices. As I mentioned earlier, it has to do with rejecting epistemology (generalized, true ways of knowing) and ontology (the notion of present beings, defined by knowing).

Let me remind you of some of postmodern anthropology’s messages, messages which also function as descriptions: Be wary of knowing anything, because knowing is never impartial or complete. This is particularly applicable to studying individuals and cultures. Give up the idea of a self in communication with an "other"—there are only contingent, momentary selves through which forces flow, giving an impression of identify. As there is no Truth with a capital T, logic is no longer a desirable means to be an end. In other words, logic only helps to discover hidden truths, which are imagined in the first place. Science is particularly misguided thinking for this reason. Mistrust representation in any guise because it relies on the notion of the presence of things, and the trick of deferred presence. Needless to say, ethnography falls into the category of representation. Never totalize. Don’t essentialize. And finally, nothing is ever originary or original, not even postmodernism.

BEFORE CONCLUDING

I want to say something about systems and systematicity. Although postmodernism would banish systems as just another form of representation, the postmodernisms that I am familiar
with remain quite ambivalently systematic. Derrida's writings are full of words like "systematic", "scientific", and "efficacy." At one point, he suggests that writing should be more mathematical (Derrida, 1981). Tyler's rhetoric consistently turns on etymological references, despite his critique of the linguistic system. Lyotard, particularly in The Differend, systematically applies conventions of philosophical thought, including numbered paragraphs and referring to himself as the capital A for author (Lyotard, 1988). I bring these observations up, not because contradictions bother me, but in order to suggest that you can be systematic without reference to a totalizing system of representation. Being systematic in practice, even in academic practice, is probably inevitable because most people acknowledge that some of what happens to them is meaningful while some is not. I have tried to suggest that there is too much meaningfulness in descriptive postmodernism, and in critique, and in writing and related endeavors. I have also suggested that exclusionary practices, like sorting out sense from nonsense, and ignoring some things in favor of others are part of science, part of practice, and part of life.

Discussions of scientific, humanistic, feminist, and postmodern approaches to anthropology will not be interesting unless you practice something. I have sometimes said that I am glad that I practice medicine rather than anthropology, because it is easier to be informed by the postmodern
condition in my practice than in yours. At other times, I have been known to argue that what I do is anthropology, minus some of the colonialism and romanticism. Today I think that scientific practices are attempts to elaborate a response to the condition in which not everything means, just as humanistic and postmodern approaches, which take universal and minute meaning for granted, elaborate their responses. Fortunately, we can choose and change our practices to suit our rhetorical and ethical purposes.
NEUROLOGY AND ANTHROPOLOGY ENABLING PRACTICES

We are all many people involved in many things. Yet, we can clearly best describe our practices of everyday life, despite the temptation and academic necessity of theorizing who we are and what we have accomplished. In this article, I would like to portray myself along with some of my roles, but I would also like to grapple with representations of medicine and anthropology by those who practice them. Although I am generally counted among physicians and among anthropologists, the notion of physicians-as-anthropologists or anthropologists-as-physicians is hard for me to relate to, and reasons for this may help establish my positioning.

What counts as a physician? Someone with an M.D. ... or should he have done residency training, or practiced medicine for a living? Physician-anthropologists are profoundly different from each other because of what they have done and do now, so the term "physician" is too essentialized for this discussion. And which anthropologists are we talking about? Do they have master’s degrees or PhDs? Did they do fieldwork in remote regions or study elites at home? If someone practices some-thing called medical anthropology, can she readily be grouped with a postmodern anthropologist? Obviously, other distinctions between anthropologists de-essentializes this category as well.

Physicians-as-anthropologists and vice versa imply a unity
or set of common denominators to what we are, what we believe and how we behave. This unity is doubtful, although overlapping experiences, particularly in the training phases, may be significant. The hybrid designation of physician-anthropologist is only a rhetorical unity; and since all academic discussions are rhetoric motivated by relations to power, this does not bother me. The real world power of this rhetorical hybrid still remains to be seen.

One more reflection concerning our topic...lately, everybody in academia indicates that she or he does interdisciplinary work: a nurse working in a law firm; a literary critic who incorporates art history; a philosopher of science; or a molecular biologist who relies on histochemical techniques. Fragmentation goes along with the postmodern condition and we all do do a lot of things: we are each a set of different selves. So I do not feel particularly privileged or avante-guard to be an anthropological physician. Just confused at times.

Sources of confusion

Include: 1. Not being able to state the kinds of Anthropology I actually do when it matters to someone.
2. Discourse with others in which I must explain my relationship to Science.
3. Feeling radically altered by postmodernism in settings where people have either never heard of it or are openly hostile toward the notion.
4. Deciding whom to approach for grant funding, and how to frame the application.

Clearly, my association with both disciplines affects many of
my activities and discussions. I have been asked to address the "humanistic-technical dichotomy" and how it relates to me, as well as my methods for integrating "the biology of medicine with the social and psychological aspects of medicine." These are not lines along which I think, and my answers will have to be particular rather than methodical. Particularity is not meant to evade the issues, but rather to evoke them with some stated context.

An Example might be

Studying aphasia, which was a topic of my dissertation. The simplest definition of aphasia is "disordered language due to brain damage." In retrospect, I suppose it was odd to pursue studies in anthropology after a neurology residency in order to understand more about aphasia. Theoretical questions about aphasia are usually written about by psychologists and neurologists; and the remediation of these problems is usually left to speech therapists, also called communication scientists. Medical training introduced me to neurological ideas about aphasia where anatomical localization and the pathophysiology related to etiology are the guiding principles. Neurologists and cognitive psychologists writing about aphasia were not writing about the experience I had talking to aphasic patients. When they mentioned "language" it seemed to have little to do with people's speech. Communication scientists' work, which usually concerned the mechanics of speech had some bearing on my experiences, but
lacked theoretical interest.

It was easy enough to separate speech from literature, which was the focus of my undergraduate studies, and thereby exclude literary questions from investigations of the brain and language. It was more difficult to exclude ideas about language in structuralist and post-structuralist discourse. When I began graduate school in anthropology, I thought I would somehow come to understand linguistics better and would learn how to apply it to aphasic speech. At the same time, by placing myself within anthropology, I hoped to acquire a context for understanding postmodernism. This latter point might benefit from more explanation: most anthropologists I meet do not think of their discipline as the place to be if you are interested in postmodernism. When I was a neurology resident, I encountered Steve Tyler at a lecture by Derrida. "What are you doing here?" I said with undisguised surprise. Most people in the audience were from the humanities. "You are still teaching anthropology?" He said that various deconstructionist ideas interested him, and informed his work in anthropology. "But what are you doing here," he countered, leaving unsaid an implication that this was not the place for medical types. I found myself explaining that I thought postmodernism might inform cognitive behaviors which were not adequately dealt with in neurology, particularly those involving speech, but that I didn’t know what postmodernism was. We agreed to talk further.
In retrospect, I admit that I thought graduate studies would identify some-thing called language, constituted by the conjunction of literature, aphasiology, neurology, linguistics, and anthropology. I used to like Jonathan Culler’s notion about deconstruction — the dissolution of a subject as its functions are attributed to a variety of systems operating through it (Culler, 1980). Somehow I expected this dissolution to result in a nicely defined object of future study.

In graduate school, I experienced various critiques of linguistics which were quite convincing to me (Derrida, 1978, 1981; Tyler, 1978, 1987). They meant that linguistics would not provide an answer to my questions about methodology and put the life back into aphasia studies. I saw how the most cogent linguistic enterprises actually blurred boundaries between language and non-language, intention and effect, and certain canonical distinctions such as linguistic levels or parts of speech. I was introduced to social constructions of scientific knowledge (Kuhn, 1970; Latour and Woolgar, 1979), and the risky nature of epistemology in general. With respect to aphasia, I read historical texts on the topic as well as some anthropological writings about language and realized that aphasiology is a kind of "secondary orality" to borrow Ong’s term (Ong, 1982). We have created it out of categories brought about by literacy, and we then try to localize these literate behaviors in the brain. This object called aphasia
is predicated on another creation called "language", which supposedly exists outside of speakers and has little to do with speech. Without getting too far into this topic, let me just say that current attempts to localize or represent these things, aphasic disturbances and normal language, have not worked. Yet aphasia itself, because it is an aspect of speech, offers a critique of deconstructionist ideas about language and anthropological notions of discourse (Doody, 1991).

I got a job

Offer in neurology, after completing anthropology coursework and while I was still working on my dissertation. This offer had nothing to do with the fact that I had gone to graduate school in anthropology, and so anthropology, particularly in the beginning, assumed a back seat position to other activities that confer credibility in my department. So what do I actually do? I see patients twice a week who are my private patients, and therefore my responsibility. I teach medical students and residents. I direct the clinical core of an NIH funded Alzheimer’s Disease Research Center, and a 10-bed research unit for patients with altered cortical functions.

And I research questions mostly relevant to the care of dementia patients. Most of this research fits Thomas Kuhn’s definition of normal science (Kuhn, 1970): like, what features are associated with psychosis in Alzheimer’s disease,
what speech/language changes occur in Alzheimer's patients, and can single photon emission computerized tomography help differentiate Alzheimer's disease from multiple-infarct dementia? I also investigate experimental Alzheimer's disease therapies, usually in the context of multi-center trials of new drugs. I participate on some national committees where we attempt to standardize the diagnosis of various dementias, locally and internationally, as well as groups hoping to improve the conduct of clinical drug trials. Obviously, my exposure to anthropology influences the questions I ask and my take on projects with ethnic or international implications. My relationship to postmodernism affects my beliefs about the significance and ethics of the projects I participate in.

There is a more overtly anthropological side to my activities as well, which is harder to relate to any population of individuals. I go to AAA meetings and present on panels. I occasionally write papers for anthropological journals as well as for medical ones. Recently, I went to northern Thailand to investigate some effects of literacy on cognitive operations such as memory and non-verbal reasoning. Because I want to communicate with people practicing and researching in medicine, I used so-called standardized instruments, like a version of the Mini-Mental status questionnaire and selected parts of the Wechsler Intelligence Scales in my videotaped interviews with various Thai and hill-tribe people. Some anthropologists have immediately jumped to
conclusions when I mention this work. "Your tests were not culturally valid. The scores don't count. You must have induced psychological and cultural trauma in the participants." On the contrary, the subtests I chose were ones anyone there could relate to; like a puzzle of a human figure or an elephant. My Thai and Karen translations of the Mini-Mental status altered the questions or acceptable answers to make sense in context. Often, several people gathered around and participated in a task simultaneously. I did not care about the scores. There was a lot of joking and laughing during these interviews, and they felt like social events. Negative assumptions about so-called medical methodologies just highlight the typical working relationship between physicians and anthropologists. The response is, I am no stupider than you are: they are not stupider than we are.

Practicing medicine has taught me that much anthropological theorizing is subject to criticisms of logical positivism, even compared to clinical practice, which allows more room for uncertainty. Medical practice also reminds me that other anthropologies, particularly postmodern anthropology, put too much emphasis on meaning. Everything is meaningful in hermeneutics, which defends it against empiricism. But keeping everything in mind at once and with equal value doesn't always make sense for human exchanges. Postmodernism does not deal as well with practices as it does with descriptions, and this enables feminist and other
critiques: but this difference between postmodern description and postmodern practices is itself a major topic which I have written about elsewhere (Doody, 1992a, in press). On the other hand, my encounters with various anthropologies have convinced me that scientific representations, particularly academic ones, are empty, totalizing, and destructive, especially when they ignore local culture and the circumstances of their production.

**What about medical anthropology?**

Since I do not want to group all anthropologies together it might be worth addressing this topic in particular. And they do provide the most obvious forum for discussions such as this one.

Medical anthropology is difficult to get a handle on. It involves so many different theoretical stances, practices, settings, etc.. For may medical anthropologists (and physical/biological anthropologists) the scientific method remains as the locus-of-truth that guides all activities. These people might just as well be practicing academic medicine in its uncritical normal science mode. For others, ethnic diversity (or ethnomedicine) is the locus of truth-as-difference. Understandably, critical medical anthropologies may attempt to mediate between these approaches: by pointing out the reductionism and mind/body dualism that go along with the scientific method, or by acknowledging essentializing tendencies of ethnomedicine and failure to consider global/
local issues. Still, critique as a locus-of-truth usually feels more descriptive and theoretical than it does practical. One success may be Jeanne Favret-Saada's work on witchcraft which delocalized truth by mingling participation and observation (Favret-Saada, 1990). Another exception is Susan Brandt Graham's work on certain conflicts in obstetrical practice, which makes use of her special positioning with respect to participation and observation (Brandt Graham, 1991a,b).

One postmodern voice, Michael Fischer's, urges us all to move beyond "simple-minded anti-science carping," an activity that more than one postmodernist should own up to (Fischer, in press). Certainly, interesting things are happening around topics once identified with medical anthropology. Witness Rabinow's work on the human genome project; or Crapanzano's interest in the body as that "place where boundaries of self, family, and society are negotiated (Crapanzano, 1990)." Work like Traweek's study of physicists suggests other ways to conceptualize professional scientific groups (Traweek, 1988). Of course feminist concerns have opened up interpretations of the body. Scheper-Hughes and Lock are among those exploring a set of metaphoric relations between individual bodies, social bodies, and political bodies (Scheper-Hughes and Lock, 1987). We have to wait and see if the metaphors will lead anywhere besides to other metaphors.

Finally, Paul Atkinson tells us what we should already
know from encounters with health care systems: that there is no unitary voice of medicine (Atkinson, 1988). But I can tell you that this is not usually what sick people want to hear, and the realization of differences, opinions as opposed to truths, and of possibilities, sometimes called errors, still unsettle many patients and many theorizers about "Medicine."
DATA STORIES

Fieldwork amounts to having a certain attitude about the situation you find yourself in. Usually, you also intend to write it up, at least as notes, and ultimately to incorporate it into your ongoing work. Traditionally, the fieldwork attitude is something like "How can this situation help me address Big Questions about mankind, either directly, or indirectly by asking Big Questions about this particular situation? I do not think that prescribing elements for the fieldwork situation which will, after all, be different every time is helpful. But such prescriptions are implicit in many anthropological descriptions of ethnography. Examples of prescribed elements include: how far away you have to go to be in the field, whether or not you should speak a different language while there, and what form your report of the experience should take. At least in anthropology most anything can be counted as data, although practitioners will vary with respect to whether or not the data elements have to be selected in advance.

Writers like Clifford and Marcus have discussed ethnographic representations at length (Marcus and Fisher, 1986; Clifford and Marcus, 1986). Feminist concerns add a slightly new element to the discussion; that is, whether writing about something self-perceived as intensely engaging and constant, such as gendered aspects of experience, is any
different from writing about something engaged in intermittently, like fieldwork, or less intensely, like a habit or daily chore (Nicholson, 1990). The answer probably depends on the writer, and on the purpose of the writing. This hypothetical question about intensity of experience suggests that writing is a uniform medium which transforms experience in uniform ways. I prefer to think of writing as a skill that potentially serves one's rhetoric. Certain anthropological theorists, conscious of the power of rhetoric (Tyler, 1987) and/or reflexivity (Rabinow, 1988) are understandably exploring alternatives to traditional ethnography.

The usual methods of science, as opposed to anthropology, do not allow everything to serve as potential data. There are only facts and arti-facts - engaging, superficially beautiful, but misleading and therefore annoying "facts" of life. To ensure objectivity, data to be collected are identified in advance, the circumstances of data collection are specified and the method of analysis is selected before the experiment even takes place. Scientific enterprises dictate the written form of their productions even more strictly than traditional ethnography, and the form itself helps readers to discern fact from artifact. Sociocultural factors related to the conduct and reporting of the science are excluded as irrelevant (not hidden from re-view). Consequently, projects deemed anthropological versus scientific result in different kinds of
data stories, and I would like to explore some of these differences.
The Case of Case Reports

Case reports are brief discussions, usually centered around a single patient, that review some aspect of medical literature as it relates to the case at hand. Most journals suggest in their Guidelines to Authors that these take the form of Letters to the Editor or some version of a Brief Communication (usually 1000-1500 words, with a limited number of references). These guidelines usually result in very similar documents, despite a wide array of clinical phenomena being reported.

Informally, other aspects of medical culture shape what gets reported and who does the reporting. Generally, cases are only reportable if there are very few similar patients discoverable by a computer-assisted scan of the literature. New techniques which have been applied to relatively few cases are also acceptable. Case reports do not actually "count" as research papers for purposes of promotion and tenure, as it is assumed that anybody can do them and no particular skills are required. Consequently, medical students and persons new to the field are more likely to write case reports than established individuals. Since case reports can be submitted at scientific meetings, some physicians use them for quick submissions to major meetings and may or may not publish them later. Occasionally, more established investigators publish case reports in support of a hypothesis that they are
concurrently investigating by other means.

Overall, case reports are low status publications for the reasons mentioned. A recent editorial points out that although "the clinical anecdote, or that which is unpublished, has been the cornerstone of medical education since antiquity," clinical journals remain least interested in the most clinical articles (Menken, 1990). The fact that case reports are still written has something to do with the compelling nature of certain cases and an author's educatory impulses. Yet the forms that journals require in order to accept such anecdotes significantly shape the telling of the tale. Case reports are a good way to begin examining how clinical experience becomes data. Since I am borrowing the reflexive anthropological approach, I will discuss this process in the first person, which would never be acceptable for a traditional case report. I will also refer to a case report I have already written in order to discuss the outcomes of the process (Doody et al., 1992b, in press).

Electrical impulses seem to have something to do with brain functioning. Since at least the time of Herbert Spencer, we have used electricity as a metaphor for the expression and spread of information in the nervous system. Electrodes pasted to the scalp pick up weak electrical signals from various regions of the brain, deep within the skull. When the cortical surface is charged with current in an awake patient during surgery, there is no pain but often there are
semi-predictable changes in behavior such as movements or experiential phenomena. Even individual neurons, as demonstrated with microelectrodes, produce electrical currents. Seizures, which can also be detected with surface electrodes during an electroencephalogram (EEG) manifest as bursts of particular types of electrical activity. It therefore seems reasonable that some seizures, particularly those involving the "language centers" might produce aphasic speech.

The most common speech disturbance associated with seizures (and with intra-operative stimulation of the language areas) is not aphasia, but is speech arrest—the cessation of speech in a person who was talking when the seizure started. Other common findings are expressive speech problems (like Broca's aphasia), impaired comprehension of speech or written language usually due to confusion, and difficulty naming things. So I was intrigued when I saw a patient who, during his seizures, had fluent speech full of neologisms and mis-statements, and almost no comprehension during these episodes. His general behavior indicated that he was not lethargic or confused. His speech resembled a Wernicke's type aphasia, which has rarely been reported as a type of epileptic aphasia. The first time I saw him talking this way, and examined his other speech/language abilities, I knew that he was unusual and that this kind of case had bearing on understanding the relationship between seizure activity and speech. The fact
that these spells came and went as they did made seizures by far the most likely etiology. The most interesting things about him were his failure to realize that he made no sense during an episode, the fact that his communication abilities had been very good (although not perfect) before the onset of these spells, and the fact that he did not appear to be globally confused during the episodes, only aphasic. Also, as the days passed and his anti-epileptic treatment progressed, he developed paranoia, a symptom that some have associated with the temporal lobes. It has also been associated with Wernicke's aphasia in some patients. His illness raised the possibility that some limited electrical discharge accounted for this rare aphasic disturbance.

Before we even consider pursuing these thoughts about pathogenesis, let us look at what had to be done to make this case reportable. A computerized literature scan showed only a handful of patients with fluent aphasia (also known as Wernicke's or posterior aphasia) associated with a seizure focus (Racy et al., 1980; Bell et al., 1990; Wilson et al., 1983; Knight and Cooper, 1986). My case appeared to be rare enough. One author had suggested reasonable criteria for ictal aphasia, which I checked my patient against: the patient must be alert, must speak during the event, and must demonstrate dysfluency, dysnomia or paraphasic speech concurrently with electrographic seizure activity (Rosenbaum et al., 1986). By current medical conventions, I needed
formal neuropsychological testing to prove that he was alert and not confused or delirious during the episodes and that he was aphasic according to standard definitions. I also needed an electroencephalogram during an episode that showed an electrographic seizure pattern, and an EEG not during an episode which did not have the seizure pattern. Separate EEG's would do for a clinical neurology journal, but for an electrophysiology journal, state-of-the art technique would require continuous EEG monitoring in order to be publishable. Now I needed two co-authors: a neuropsychologist to do the formal testing and an electroencephalographer to come to the bedside during an episode (EEGs are usually done in the lab at scheduled times) and document a seizure. With some vigilance, we accomplished these documentations.

Meanwhile, the clinical aim was to stop all seizure activity as soon as possible, which we attempted by using various drugs. If we had succeeded right away, there would, of course, have been no case report because we would not have gotten the necessary documentation. This particular patient was slow to respond to drugs, but he eventually did. The electrographic seizure pattern disappeared, and his speech and language abilities returned to baseline. He also went through a phase of being quite paranoid, which was entirely new for him and therefore likely related to what had recently happened to his brain. It seemed that information about his course of recovery would be quite interesting to include in the case
report. But usually such information is summarized in a paragraph or put into a table, rather than described day by day. In order to create a table, which would hold the most information, we had to standardize his daily evaluations so that the table could depict changes in particular tested behaviors over time. During this period, we documented at least two different types of seizures, and gradual improvement in cognitive functions such as remembering, comprehending commands, performing calculations, and speaking abilities.

Overall, my co-authors and I learned from this case that a patient could manifest Wernicke's aphasia from a left temporal lobe seizure; that inter-ictally when he was having frequent seizures, his degree of aphasia could fluctuate; that the recovery phase might include paranoid ideation; and that after all seizures stopped he could return to a baseline of nearly normal speech and language abilities. When I reviewed the handful of similar cases, I hypothesized that this particular form of ictal aphasia was probably rare because it occurred in patients with circumscribed seizure discharges, usually patients who were partially treated with medicines, but not yet under control. It reminded me of a developmental disorder in children, the Landau-Kleffner syndrome in which children develop a progressive Wernicke's aphasia and, ultimately, a seizure discharge in one or both temporal lobes. The discussion in the written case report therefore reviews cases of Wernicke's aphasia due to seizures, mentions paranoia.
and the possible relationship between this symptom and the temporal lobes and speculates about relationships between fluent epileptic aphasia and the Landau-Kleffner syndrome.

Reading the case report is not nearly as fascinating as working with the patient was. For one thing, his case is summarized very briefly to allow room for the introduction and discussion: these usually determine whether or not the case report is accepted for publication. Also, some interesting elements of daily examination are not included in the standardized evaluation and so do not appear in the table labeled "Behavioral, EEG and Treatment Course": for example, the content of his delusional beliefs. At times, he was very angry with General Noriega, and his complex delusions about Noriega’s incursions into his life unfolded over several days, usually triggered by something on the news. There is no anticipation in the written report regarding his response to therapy, although there was such anticipation when we took care of the patient. Publication assumes that he got "proper" medical treatment sufficient to control his seizures, and if he did not, the report could be turned down on these grounds alone. We as authors are simply expected to document the accepted medical therapy in the case section of the case report.

Case reports illustrate one way in which experience becomes data in medical science. Since they deal with one or only a few patients and do not include a carefully controlled
experiment they are not given the same status as other research papers. Although their education value is acknowledged, they must educate according to certain conventions in order to be publishable. So why did I bother to write this case up? I have asked myself this a number of times. Quite honestly, it is because I would like others to read my hypothesis and maybe find ways to support or refute it. I would like somebody to explain the relationship between epileptic discharges and the Landau-Kleffner syndrome. I do not really believe that writing this report will accomplish these aims, or that this report is necessary to stimulate others to ask these questions. It was just my way of doing a little something more than treating the patient and teaching the residents. I enjoyed thinking things through and reading other case reports on the topic. I did not enjoy forcing the case into the proper format even for a fairly innovative and flexible journal, creating the data table, or formatting the manuscript according to journal specifications. Probably, case-reporting is an impulse I will not succumb to very often.
Semantics of Sentence Accent in Alzheimer's Disease

A couple of years ago, my neurology department applied for an Alzheimer's Disease Research Center, sponsored by the National Institute of Aging. Such grant applications must include, among other things, several proposals for pilot research projects that make use of the clinical program being proposed and that could lead to future studies. So I was invited to submit a pilot project to go along with the rest of the application. At the time, I was thinking about linguistic and discourse approaches to speech and language. I was also re-examining certain neurological assumptions about normal language, such as the different roles right and left hemisphere play in speech. I thought it might be interesting to study some aspect of speech in Alzheimer's disease patients.

Here it might be appropriate to mention the typical format of an NIA pilot grant proposal. It begins with an abstract which you obviously have to write last, since it summarizes the whole study. It includes a budget for the 12 month study period (maximum was $20,000.00), a budget justification, biographical sketches on all of the co-investigators, and listings of any other support you have for this or other projects in order to prevent overlaps. The core of the proposal is organized as follows: specific aims, background and significance, preliminary studies, experimental design and
methods, human subjects (who will participate, risk-benefit ratio) and a bibliography. Usually, it seems reasonable to get the background first, and so I searched the literature before deciding on my "specific aims."

Certainly I had spent a lot of time talking to patients with Alzheimer's disease and other dementias, and had noticed that these conversations were sometimes difficult. Several points in the medical literature on language and Alzheimer's disease (AD) seemed pertinent to this observation: almost all AD patients have some language dysfunction (Cummings et al., 1985; Appel et al., 1982); usually, syntax is preserved until late in the disease (Appel et al., 1982); semantic difficulties predominate (Appel et al., 1982; Bayles, 1982); often there is a typical longitudinal progression from anomic to transcortical sensory aphasia to Wernicke's aphasia (Cummings et al., 1985) and then global aphasia or muteness (Appel et al., 1982). Yet, attempts to characterize AD speech in linguistic terms or to differentiate AD from other dementias by the language abnormalities alone had been unsuccessful or had given conflicting results. There were also a great many articles that seemed irrelevant to my actual conversations with AD patients.

A striking aspect of the review was that many different language tests had been used with different patient populations and under different circumstances. Relationships between the tests were not addressed. For example, do word-
finding pauses in spontaneous speech have anything to do with anomia on confrontation naming tasks? Such a relationship was often assumed (both are called word-finding difficulties), but had not been addressed. If a patient has problems with word fluency on one task (e.g., naming animals in 60 seconds) will she/he have problems on other word-generating tasks (e.g., naming words that start with "F" in 60 seconds)? Different investigators used different tests, so this could not be answered. Can you pick up what is wrong with the speech by listening to a recorded conversation? One discourse-based study showed that by counting various types of cohesive errors you could not distinguish speech of AD patients from age-matched controls: but clinicians talking to the same patients clearly could distinguish them (Ripich and Terrell, 1988). How this type of discourse approach compares to standardized test performance, say on aphasia batteries, is unknown.

Another striking observation was that the terminology and even the linguistic concepts in the medical literature were not as meaningful to me as they had been in an anthropological linguistics context. This phenomenon is well-known for individuals participating in interdisciplinary work, and is usually attributed to a "time lag" while information from one discipline finds its way into another. But I disagree that it is all a matter of time. For one thing, the borrowed terms did not borrow any of the active critique of linguistic terminology that goes on within the field. The medical
literature seemed to take notions like "word" and "verb", or "syntax" and "semantics" at face value. Other rather interesting concepts related to these terms were missing. Some theories of semantics for example, highlight the semantic use of sentence accent: if I say John bought a new car as opposed to John bought a new car, the use of accenting conveys subtly different meanings (Davis, 1987). A built-in neurological bias that prosody has to do with the right hemisphere, and language with the left had apparently excluded sentence accent from the realm of semantics in all prior works.

Of course, I found lots of other things interesting too, and have only named those that I decided to pursue. This selective reporting makes it all seem more logical and straightforward than it was at the time. But deciding what to study is not nearly as difficult as stating how you will do it, as anybody who writes grant proposals knows. I do not wish to discuss strategies for study design or the details of how I arrived at mine. But a few points about the difficulties in this particular case are pertinent to a discussion of how behavior is conceived in a form that will eventually become data.

Scientific studies emphasize consistency and control, and the best way to accomplish these is with computers. Also, if equipment is requested in the budget, it must be clearly justified by the role it plays in the study. Nearly a year's
worth of research time, therefore, went into making a personal computer speak clearly accented sentences aloud while simultaneously displaying them on a screen. Despite their ability to digitalize sound and convert it back and forth into analog voice signals, computers still cannot understand speech. Therefore, recording devices were necessary to deal with the subjects' responses to the mechanized sentences, and for making regular conversations into data. Humans were also necessary, to transcribe the recordings, mark the sentence accent, and compare the normal controls to the people with Alzheimer's disease. These methods, then, helped to "control" the study and remove what would be perceived as biases if I had, for example, just sat down with each person, tested word fluency, had a short conversation, and then encouraged the subjects to manipulate and interpret sentence accent in various ways.

Data analysis is still ongoing, according to the prescribed specifications in the original grant. Subjects perform 3 separate word-generating tasks, and a score is generated for each. A mean score (for the 3 different fluency test versions) is also calculated for each person. Subjects' performances are compared across tests, and mean group values allow comparison between patients and controls. A ten minute conversation between examiner and subject is recorded, transcribed, and marked for sentence accent. Patients' performances are compared to controls'. The number of word-
finding pauses for each subject during the conversation are also compared to that person's fluency scores. Finally, there are 3 talking-computer subtests, and subjects get 3 separate scores to reflect their approximation to proper sentence accenting in each subtest. The patients and controls are again compared.

I have not discussed my specific aims and hypotheses in detail because it would be tedious to do so. Overall, I expect word fluency performance will be consistent for patients and controls (they will perform similarly on all 3 trials), and that patients will do worse as a group. These fluency scores will probably not correlate with word-finding problems in speech. And patients will have trouble using sentence accent to mark semantic information, particularly if they have severe Alzheimer's disease. I do not yet know whether these hypotheses are supported by the results. I do know that my piles of data sheets with numbers and words meet the specifications of a scientific study, and that they have moved quite far away from the intuitions that inspired the grant. The hope in most such endeavors is that the discussion section of the final report will use the scientifically obtained results to return to the intuitions, or at least the hypotheses derived from them, and make sense of the experience. This is a cumbersome way to get to such a discussion, but the cumbersome features are the rules of the discourse.
Regretfully, scientific reports usually minimize the problematic construction of methods and any analysis of their limitations; methods are instead presented as formulaic statements to summarize how the study was done. More regretfully, causal relationships between established methods for doing things and the outcomes of studies are not addressed. Both methods and motivations are more frankly discussed in person (at meetings or through telephone calls) than they are in papers. These are major differences between medical science and most anthropology, excepting that type of anthropology modeled on medicine or the strict scientific method. Conducting oneself and one's experiment in the field entails a structurelessness and subsequent analysis that predict the outcome differently. When I finish my study of semantics of sentence accent in AD, I will have carefully obtained and analyzed data to work with. The study has been quite straightforward to carry out, thanks to the study design. The leap of faith will come only when I try to relate these formal elements to my reasons for doing the study in the first place. The next section may serve as an illustrative contrast between this kind of scientific endeavor and a more anthropological project, in which the conduct of the study was more unstructured and therefore more difficult, and the tendency to invoke forms was more compelling during the work of interpretation rather than during the conduct of the research.
A Thailand Adventure

I remember when it first occurred to me to go to Thailand. I was talking to a fellow graduate student in anthropology who was excited about studying literacy and the Karen Hilltribe people. It seems the Thai government had decided to establish universal Thai literacy, including in their directive those hilltribes who were not literate in their own languages. Jeff and I had both read Walter Ong's Orality and Literacy (1982), and Jeff had read a lot of other theoretical works about literacy as an epoch, a political force, a cognitive organizer, etc. He also told me some folk tales about the Karen and their own explanations for being non-literate: such as the story that they had to eat their books when migrating long ago from China to Thailand, so they are wise in their hearts, but unable to read.

I really wanted to know if being literate shaped cognition in some way, because I suspect that it does. But since non-literate who are in close contact with literates adopt literate ways of thought (or so the theory goes, from Luria, to Scribner and Cole, Goody, and Ong), I too would have to journey somewhere to find such people. I asked Jeff, if he got his Fulbright would he mind a visit from me to study this, provided I also found funding?

Certain obstacles stood in the way. Most anthropology grants are for a minimum of 6 months, and I could not leave my
job for that long. And most would not fund an M.D. (they said so on the telephone). Medical grants are even harder to come by, would consider this non-medical as it had no immediate implications for disease, and they have very long funding cycles. I had been one of those people doing fieldwork at home, and had not given much thought to the more colonial approach. Post-modern, post-colonial discourse coursed through my veins. How could I possibly conceive such a project? Also, I am a neurologist and have developed my own feelings and intuitions about cognitive exchanges by doing what I do. I did not want to try and quantify these intuitions with questionnaires, as Scribner and Cole (1981) and Luria (1976) had done. Alternatively, Ong (1982) and Goody (1987) seemed a little vague when they spoke about cognitive processes. I wanted to be able to talk about the "results" with people in my field who seriously study cognitive behavior. This pretty much meant that I would have to use standardized neuropsychological tests, at least as a starting point for my discussions with non-literate people, although the tests were not designed for them (linguistically or culturally), and were notoriously bad at assessing non-literates, even in my own society.

One day I was discussing this pipe dream with a friend who immediately thought of the Ray C. Fish Foundation, a well-respected foundation in Houston that funds medical research, education, and the arts. He put me in touch with the trustees
who listened to my reasons for wanting to do this project and eventually agreed to fund it quickly, so that my field experience could coincide with Jeff's time in Thailand. With a field contact already in place, I thought I could accomplish something worthwhile in one month. Despite the Gulf War, a coup that displaced the Thai Prime Minister and installed a military government, and the difficulties arranging job coverage and home coverage for a month, I did go to Thailand in the summer of 1991.

The first several days in Chiang Mai seemed like a dream. Someone else's dream. I was introduced to Jeff's perception of the country, the city, and the culture and politics that had become the topics of his research. There seemed to be little room for my own perspectives and opinions to form, change, and grow. From talking to other anthropologists, I understand this is a "normal" field experience which many people discuss informally but tend not to write about. Jeff had recently been hospitalized with both malaria and Dengue fever, and he looked thin and ill. Still, he was a responsible and generous host, introducing me to everyone he thought might be able to help with my project.

I met American students studying Thai and their Thai student friends. An American restaurant owner and his Thai co-owner. Fulbright scholars and their families. Development workers, both freelance, and associated with the Mountain Peoples Culture, Development and Education Center (MPCDE),
which I learned had originally been funded by a women I remotely knew in Houston. We went to the Archives, presided over by a scholar interested in hilltribes, and to Chiang Mai University and Pyap University where I met professors nice enough to listen to my idea of a project. To me it seemed like I was getting ready to do a project: to interview non-literate Karen using some standardized pieces of tests as a starting point. To most everyone else, being there was their project, a difference which generated some observations. Why didn’t I carry my video camera around with me to document things? Why was I not taking copious field notes during conversations? Why did I choose to live in a western-style Thai hotel rather than in a dwelling more like where other people lived (no hot water, telephone, air conditioning, etc.)? These questions sometimes seemed like indictments at the time, but were valuable for helping me understand fieldwork mentalities. All of the individuals I met told me helpful things about Chiang Mai, or the Karen. And many of them provided concrete assistance: the restaurant co-owner helped me translate the Mini-mental status questionnaire into Thai, and a Thai woman who worked at the MPCDE eventually acted as my translator for Thai interviews. At any rate, within two weeks, thanks to Jeff’s facilitation, I was ready to start exploring the questions I came to ask formally.

It turns out that the Karen people and the Baptist church are very much associated in Thailand, and in Burma where most
of the Karen live. Jeff had become very interested in the Karen-Baptist connection, and knew all of the Karen-Baptist elders. These people talked with me about my project and prospective villages where I might interview people. They suggested older Karens living in Chiang Mai who might agree to take my "tests" and discuss them with me as well. So one day Jeff, his primary informant named Ralph and I set out for the Center for the Uplift of the Hilltribes (CUHT) to interview some people and videotape the exchanges. Ralph was Jeff’s constant companion and friend in addition to being his primary informant. He was half Burmese-Karen and half English, a former radio announcer in Rangoon who now taught English at Pyap University. Together they had evolved a relationship whereby Jeff provided much of Ralph’s physical support in exchange for the intellectual help Ralph provided to Jeff. Once again, my presence in this dream introduced difficulties. Ralph did not wish to be given cash for helping me, and so it was left to Jeff and me to decide what any given help was "worth" and I paid Jeff who then factored this into Ralph’s maintenance. Jeff and I split the cost of a rented motorcycle as our transportation, and I found it very uncomfortable and dangerous to ride with all 3 of us on the bike: yet, it was unkind to exclude Ralph. And Ralph provided me with some very important help. He translated selected terms from the Karen thesaurus at the Archives, and helped me to translate the Mini-mental status questionnaire into Karen. Ralph acted as
translator for my interviews at the Center for the Uplift of the Hilltribes (CUHT).

I was liberal in my translations of the standardized questionnaire known as the Mini-Mental status, which is used to screen for and rate the severity of dementia. It has a section about orientation (e.g., What day is it today?) and I allowed as correct any answer most people locally would agree on (e.g., The Queen's birthday). The subject must repeat and remember 3 common words; I used words common to them, rather than literal translations of the English words. They must follow a verbal command, repeat a phrase (adapted to one they would have heard), copy a drawing, do a sequence in reverse (I used days of the week) and write a sentence. The denizens of the CUHT did quite well on this test, and thought that it was good exercise for the brain. They also enjoyed object assemblies (putting puzzles together), visual reasoning (sequencing pictures to tell a story), block design (copying and remembering designs made with 2 or 3 red and white blocks), and I think they enjoyed the token test, although it was hard to tell. The token test uses 4 rows of tokens: Large squares, small squares, large circles, and small circles. There are 5 of each, all different colors. The examiner then issues commands, like "touch all of the squares except the black ones," which the subject carries out. Although I was not interested in the formal test scores for these tests, they were good stimuli to get people started at
a task, observe their behavior, and ask them questions about the test, their thinking and their responses. Because I normally see patients over 50 years old in my medical practice, I stuck to the same age group in Thailand so that my internal comparisons would be more appropriate.

I realized that if I wanted to know something about the effects of literacy on these tasks, I need to interview both literates and non-literates, even though this had not been my original plan. That is why I went to the CUHT as well as the jungle. I also felt I should interview some Thai people for a comparison group to the city-dwelling Karen. Although almost everyone I knew was young, we did find some older Thais (some were drivers hired in the streets) who agreed to be tested. Unless I was specifically told not to, I paid the people who agreed to talk to me and do these semi-standardized tests.

Before arriving in Thailand, I thought it would be easy to find non-literate people who were out of contact with literacy. But there was no one in Chiang Mai, not even the Baptist missionaries, who could direct me to an actual village of such people. They mainly knew the villages that they had been to and established schools for. They all said there were plenty of non-literate, remote Karen (sometimes they said backward or primitive), but they spoke of regions rather than villages where such people lived.

My anxiety level began to climb as I realized time was
running out and I didn't know where to go to do what I thought of as the most important part of my project. My friend Jeff suffered a relapse of his fevers, and began cautioning me against trips to far away or remote places. His pessimism was contagious. Finally, I called a Karen trekking guide who had originally told me he could not help because he did not know the regions where I would have to go, and told him that I wanted his help anyway. We would go west, toward Mae Sariang, where several people said there was a guest house accessible to the road. We would find it (we had no name or exact location) and ask there where to find non-literate Karen who, by all reports, should be all over the jungle.

On the way, my guide, Don, educated me about highway bandits, bribery in case of automobile accidents, and other perils I did not know about. It was the middle of the rainy season, so we experienced the power and peril of 4 wheel drive in mountainous regions on wet roads. We found the guest house, in a Lawa village, and walked several hours to a Po Karen village (Kong Pae) that was completely non-literate and had little to do with literate groups because it was so isolated. About 70 families lived there. After the 4 hour walk there, mostly uphill in the rain, and through rivers and jungle terrain, I was nearly too tired to conduct interviews.

I should have anticipated this, but when I was introduced people became very animated and began to bring me sick people to examine and take care of. They had not seen a doctor in
many years. I had very little medicine, and felt quite inadequate to their needs. So it was all the more amazing that they then agreed to sit down and talk to me about "thinking". I videotaped much of these interviews and took a few notes. But there was no tripod (we couldn't carry it), and I had to conduct the interviews through the translator, watch after my supplies which people openly explored, and film at the same time. Whenever we interviewed one person, a crowd of people gathered around, often shouting out suggestions to the one doing the tasks. Sometimes several people tried a task, such as replicating block designs, at once. There was a lot of laughter and we all generally had a good time.

We then went on to another village (Ban Pae), closer to Mae Sariang, that had some exposure to literacy. They had also been exposed to communists, Baptist and Catholic missionaries, and trade with literate cities, although most people did not directly participate in this trade or in any church. None of the adults had been to school, although many children were currently enrolled in a Baptist school. It was an old village, no one could say how old (older than the city of Mae Sariang), with about 150 families. To my eyes, it looked quite prosperous compared to Kong Pae.

My guide's sister-in-law's brother was a revered school teacher and development worker in the area, and took us around to meet several elderly people who agreed to be interviewed later in the day. He told a lot of jokes about intelligence,
or lack of intelligence, and in many instances kidded them into participating. I realized eventually that I was being introduced as a Christian doctor (no one ever asked my religious background), somehow associated with the Baptist mission. I am sure that this was meant as a compliment and a mark of acceptance, although in general I would rather not be associated with development or evangelism. These villagers were more private than those in Kong Pae: fewer neighbors climbed into the house to see what was going on. Family members usually watched quietly. They were equally tenacious at the neuropsychological tasks, and offered more opinions about the tests and other topics, like literacy. Still, it seemed to me that both sets of villagers used similar strategies, and that they differed from the city-dwelling Karen, even from the one uneducated man I had interviewed in Chiang Mai at the Center for the Uplift of the Hilltribes.

The night we set out to return to Chiang Mai was a special night. In little village towns and the few structures just outside the city, buildings were decorated with colored lights, like Americans use for Christmas. People were getting ready to celebrate the Queen’s birthday which would officially begin at midnight, and it was a national holiday. It would also be my birthday, and I felt mystically connected to the country for the first time. Many hours later I returned to my hotel, very dirty, longing for hot water, antibiotic ointment for various lacerations, and drinking water. In the city streets
and hotel lobby everyone was celebrating like it was New Year's Eve. My room was still reserved, my belongings were still there. A little pile of faxes sat just inside the door with birthday greetings from home. Amused, I kidded myself that I was older, wiser, and in better physical shape than when I left the States.

With only a few days left, I quickly arranged to interview my guide's father, a ninety-five year old educated Karen, and several Thai speakers. When finished, I had 4 interviews with Chiang Mai Karen, 5 with Chiang Mai Thais, and 12 with Karen villagers (6 from each village). These interviews seemed like my data to me. I also had 2 notebooks containing observations and practical information, like names and addresses which I suppose could be called field notes. While in Chiang Mai, I finished reading Doris Lessing's The Golden Notebook, which I had started in the States, and so my notebooks seemed more like field fiction than data. Of course, no representation of any kind is really the same as what goes on in the field.

I have not "written up" my Thailand data yet. I have presented bits of it to the Anthropology Department at Rice, to neuropsychologists in the medical center, and to a group of interested friends. It is not the topic of my dissertation, and so I do not have to write a book, like Literacy and Cognition: Thai Karen Speak for Themselves. There are many forms that I can invoke to shape the data into something recognizable and publishable: "A comparison of Mini-Mental
status scores in non-literate Karen hill tribesmen and women and non-literate veterans." Or "A cross-cultural adaptation of the Mini-Mental status examination." Anything I would write for an anthropology journal, such as a descriptive account ("Literacy Does Shape Cognition: Thai and Karen Approaches to Tests of Cognitive Functioning"), would not be publishable in neurology. Probably, I will invoke forms from both disciplines and write different kinds of papers. I hope to involve collaborators who will act as interlocuters and also help to anchor this work in their disciplinary structures. Although the conduct of the research was difficult, almost anything was permissible when it came to carrying it out. Day to day life had to be flexible and adapt to what seemed to be going on at the time. In contrast, the knowledge-generating enterprise involves a different kind of practicality. Without it, this work will be unknown to everyone except my close circle of friends and acquaintances, which seems like a disservice to those who participated in the project as well as those who funded it.

By the way, this work will be subject to various anthropological criticisms unless many details from the Archives and my notebooks are provided during the interpretation. Ubiquitous meaningfulness still prevails in anthropology, and any claim or observation must have several bits of meaning to contextualize and support it. If I say that the Po Karen village is poor, I must describe their
economy, and provide details about their diet, health, possessions, dwellings, etc. If I mention interviewing Thai people nearly at random, I will need to spell out my relationship to each and all I know of their biographies. It will be useful to provide information about the different social positions of men and women in these societies, even though I interacted with them as if they were equals. I should indicate that I did everything possible not to be held in a superior position, the pains I took to make the test questions culturally relevant and linguistically accurate, and the social positions of my various translators and informants. Although I agree with many of these guidelines, they also seem a little spurious as the answers are always some form of a guess, and do not hold the truth value they are usually accorded.

In Conclusion,

I hope that these data stories have provoked some thought about ways that lived experiences and people’s behaviors are supported as data. Although I do not believe in the superiority of science or social science or hermeneutics or any other approach, I am willing to honor the approach used by which ever discipline I plan to associate with at the time. It is really a matter of ethics rather than a matter of truth to do so. So long as we keep in mind that all data are stories, shaped by the forms we use to gather them, analyze
and report them, the production of such stories can be a satisfying way of life.
FRAGMENTS, OR THE POSTMODERN CONDITION
Once upon a time, several words were used to denote the breakdown in communication that happened when someone was not holding up his end of the talking. Since the impaired conversant could not say what was wrong, a number of theories appeared. In Greek, for example, they might have diagnosed a-phonos, or a "privation of voice;" see Herodotus, Sophocles, or Hippocrates for details (O’Neill, 1980:12-67). Or maybe it was a-naudos, "absent faculty of speech," a notion which is suspect because it was resurrected out of Homer by Galen, who needed it to fit in with other ideas (O’Neill, 1980:64-65). Or more appealingly, it could have been a-phanasia, often "with lachrymose eyes," which is the "speechlessness secondary to emotion" that poor Penelope suffered when she learned that her suitors planned to kill Telemachus and keep her for themselves (O’Neill, 1980:14-28). Anyway, there was once an oral past during which problems with speech were somewhat more vague than they are today. Between the sixteenth century, when von Graffenberg noted that speech could sometimes be disturbed by specific brain injuries, and the beginning of the twentieth century numerous examples of brain-damaged speech were described. Curiously, these maladies were constituted by problems with reading, writing, saying, and understanding, and are obviously conditioned by ideas of language brought about by literacy. Early attempts to systematize observations of
disordered language can be grouped into phenomenological systems and neuro-anatomical models. Neurologic folklore still refers to "Holists" such as Hughlings Jackson and "Localizers" or "Diagram Makers" whose work culminated in Lichtheim's map of the various aphasias, in the late nineteenth century. But to conclude this version of history, for the moment, between the turn of the (last) century and now we have been busy collecting exceptions to neurology's totalistic map of the aphasias and reifying his categories by creating explanations for these exceptions. Derrida, a believer in systems and science, and structure/deconstruct, and belief, and efficacy, and emergence, and work (intellectual)/play has said that the incision of deconstruction does not take place just anywhere, but depends on historical analysis (Derrida, 1981). I will go so far as to say that the history matters.

There is No Argument Yet

So, aphasiologists, mostly neurologists like myself, have gone along for centuries describing these brain-injured individuals whose speech is somehow disturbed. For a while, we focused on the "out-put" side of speech and recorded that our patients sometimes had "effortful," low-out-put speech, with slurred words and apraxic facial movements; we learned to match this description with an anterior location of pathology, thanks to Broca and those who came after him. Other patients
had "logorrhea," with recognizable kernels that we called paraphasic errors; unlike the first group, comprehension was grossly disturbed as well. Thanks to Wernicke, we included these posterior aphasics in our maps of aphasia in the brain. As I mentioned, centuries worth of information have gone into neurological representations of the aphasic syndromes and their underlying localizations. The representations have not changed much since 1885 (Lichtheim, 1885), although there has been a greater tendency to focus on "language" instead of "speech" in the representations. Freud contested the ideas that underlay these diagrams, but stayed within the representational system to do so. Head and Goldstein argued that more behaviors had to be accounted for, such as non-verbal symbolization, and launched the field of neuropsychological assessment. Still, their criticisms were subsumed into the model.

An exploration of historical texts suggests that people who wrote about speech and language, but not about aphasia, had ideas similar to the aphasiologists' about what language was like. Texts by Schleicher, Darwin, and Spencer show that similar ideas about mental faculties, similar metaphors about organisms, and similar romanticism in general were shared by these representatives of the natural/social sciences and their contemporaries studying aphasia. But then Saussure came along and changed linguistics with ideas that the aphasiologists did not so readily incorporate. Signifier/signified, sign,
synchronic/diachronic, mutability/immutability, and most importantly, his rhetorical strategy of focusing on langue instead of parole left aphasiologists behind in the dust. Sapir, who never referenced Saussure, had a lot of the same ideas about language as a referential system, although he talked about symbols instead of signs and used more comparisons between languages to make his points (more anthropological?). Unlike Saussure, he was interested in the "psychiatric" aspects of language which forced you to take a person's personality into account in order to understand her use of language. Whorf (also an anthropologist) established cultural relativism as immanent in language in the process of his detailed, structural, scientific analyses of cultures other than his own. Ideas of Sapir and Whorf have not greatly influenced aphasia theory. The referential basis of aphasia studies, however, ensured that they would get at least a murky bath in Saussure's semiotic theory once it was muddied by practices in linguistics and elsewhere.

I said "There's No Argument Yet," and there's not because being left behind is not insurmountable. We can always read faster, study harder; strike up a collaboration or two and turn out multi-authored studies that benefit from several areas of expertise. So what if Saussure said to focus on langue: he also said that linguistics should study all manifestations of human speech, and that langue was first only because it represented the norm of speech (Saussure, In: Bally
and Sechehaye, 1966). Besides language, not speech, must be what is represented in the brain. We can always let the linguists work on their version of language; meanwhile, we can refine our testing tech-niques (to create data) and our localization tech-nologies (to confirm our expectations). Eventually, what the linguists find out can be translated into the picture we have constructed about the brain, although we might each have to modify our models a little. So, apparently, thought the neurologists at the beginning of this century.

So We Went Our Separate Ways

A lot has been said about how writing differs in different disciplines, but less has been said about how different disciplines fashion their linguistic objects in the course of daily practices. Physicians, like everyone else, use normal speech to get things done, and good physicians are better at it than bad ones. But normal, everyday, two-sided, confusing speech has a special status in the medical interview. First of all, it is customarily part of the history as reconstituted by the history-taker. A "bad historian," invoked when explaining why a medical history is not clear never refers to the physician. It is the patient who was bad at telling his tale and the physician did her best with what she got out of him. Van Naerssen points out that actual quotations of what patients say are often resorted to in medical documents when
something remains unclear, and the history-maker may need to point to it later in self-defense (cited in Kuipers, 1989: 110). So patient/physician speech in an interview is transformed into a written history, often with the intermediate step of having been dictated by the history-taker. It is then subject to all of the criticisms of "plain speech," scientific prose and historical representation that have been raised by writers on writing [(Tyler, 1987) and (Derrida, 1981) for example].

If speech is meant to be turned into history, what then happens with aphasic patients? In them speech is one of the presenting problems, and there is no way to keep it out of the physical exam, which is supposed to be objective. When physicians turned their attention to speech-as-an-object instead of a means of communication they looked for scientific ways to test and analyze it. So long before the localizationist models, they started treating testable speech, or language, as something different from speech-in-conversation. Their ideas about language must have come not only from commonsense experience with speech, but from other people's models of language, such as the grammarians who taught them in school, the linguists whose work they happened to be familiar with, or the neuropsychologists who were perfecting ways to "elicit" illicit language behaviors. Well then, how can I write that aphasiologists were going along separately from linguists in studying aphasia? Pick's model
for language-in-the-brain, even in the eighteenth century, was a classic linguistic theory complete with hierarchies of "actualgenesis," almost as good as generative grammar (Goodglass and Blumstein, 1973). And the physician/anthropologist Broca knew about Schleicher's linguistic theories, and the neurologist Goodstein was influenced by hermeneutes like Ingarden, and probably even Habermas---(Did I write Habermas? I meant Husserl). Geschwind, the father of behavioral neurology, in his famous paper that established connectionism as an explanation for all of the behavioral syndromes, including aphasia, commented on his own anti-Whorfian stance (Geschwind, 1964:639). If all of this is not enough evidence against my statement, the British neurologist MacDonald Critchley even grappled with Saussure and Sapir, Jakobson and Bloomfield when he tried to advise us about how to define language and study aphasia (Critchley, 1970:1-16). Last, but not least, Caplan went to the trouble of training as a linguist under Chomsky before training as a behavioral neurologist under Geschwind. Who could be more informed about both fields?

All I can do at this point is note that, in my experience, people who study aphasia primarily by talking to actual patients have different notions about it than people who study transcriptions and other representations of aphasic speech which do not talk back. And people who like linguistics end up fitting aphasia into linguistic models which purge out the
feeling of trying to communicate with aphasics. I can try to
survey these differences more systematically or in greater
detail, or at greater depth, whichever metaphor for
"additionality" serves best.

"Systematically speaking," ... there are two general
groups of researchers who have dealt with aphasia in the last
30 years or so: behavioral neurologists and others with a
clinical bent; and cognitive psychologists and others with a
linguistic approach. Into the first group I would throw
Alajouanine, Luria, Critchley, Geschwind, and most of the
people who publish in the Archives of Neurology and Neurology.
Into the second cluster I would put, let's see, Goodglass and
Blumstein, Crystal, and Caplan and those who write in Brain
and Cognition and Brain and Language. Some people might make
similar dichotomies using terms like: "clinical aphasiology"
and "neurolinguistics/giving rise to linguistic aphasiology"
(Caplan, 1987:5) or "clinical linguistics/a part of applied
linguistics" versus "neurolinguistics" (Crystal, 1984;30),
although both of Crystal's groups would fall into my second
cluster. It seems that what differentiates the clinical from
the linguistic approaches is that the clinical writers do not
have a specific or explicit representation of language in
mind, and think in terms of an intersection between cortex or
subcortex or some other level of the neuroaxis and a
behaviorally specified and localized field, such as the
frontal or parietal lobe. They seem to think of language as
something we all know and recognize, that is subject to systematic disturbances that result in the aphasic syndromes. The linguistically-minded start with a hierarchical model of language, often separated from pragmatics/use, an idea of what counts as data from aphasic speech samples, and a method for predictably linking the data to the model. Caplan points out the inevitable need to control speech circumstances through "proper experimental and observational techniques" so that particular linguistic subcomponents can be isolated: only then can he fit them together into the models he originally theorized (Caplan, 1987:136).

Let me supply a more "detailed example." Alajouanine identified, with some indebtedness to Broca and Jackson, what he called "verbal stereotypies" in aphasic speech (Alajouanine, 1956). He noticed that some aphasics involuntarily uttered recurrent words or phrases, like "yes...yes" or "I don't know" which did not necessarily mean what they said. In fact, these words or phrases kept coming up whenever the patients tried to say anything. About half the time they eventually went away, leaving the patient with either Broca's aphasia or agrammatism. When the symptom regressed, it usually did so in stages: first, the recurring utterance became invested with intonation appropriate to circumstances; then the patient was occasionally able to suppress it; then additional utterances became associated with the initial one, expanding the patient's choices; and finally
the recurrent utterance disappeared altogether. Although he was not primarily interested in localization, Alajouanine recognized a polarity of aphasic disorders that, on the one hand, was represented by trouble producing speech, and on the other hand, showed disturbances in the syntactic/semantic value of words, and he accepted these as being due to anterior and posterior pathology, respectively. So, even though he consciously adopted some of Saussure's linguistic terminology in order to talk about language (phoneme, morpheme, semantics) he felt comfortable limiting his analysis to a functional, diachronic, clinical survey organized around certain speech characteristics as opposed to the systematic, synchronic method Saussure had advocated. Contrast this to Goodglass's description of his "Studies on the Grammar of Aphasics," which "exemplifies the neurolinguistic approach to the study of grammatical deficits in aphasia; highly structured test materials are used to test specific hypotheses concerning grammatical impairments...(Goodglass and Blumstein, 1973: 183)." Using this method, he found that aphasics' performance on items such as inflecting word endings and recognizing grammatical forms illustrated by pictures was similar irrespective of severity, and irrespective of whether expressive speech or comprehension was more involved. The patients' symptoms cut across grammatical categories, affected all levels of linguistic hierarchy, and failed to differentiate patients who fell into separate groups as judged
by clinicians talking with them. From this he concluded not that the clinical categories were wrong or the linguistic model faulty, but that the symptom of agrammatism reflected an underlying psychological component that was deranged prior to its transformation into language by the linguistic system.

"Deeper explorations" into the differences between "clinicians" and "linguists" lead me to my "semiconscious feelings" of difference and to an understanding involving kinesis versus mimesis. It irritates me to read that each level of linguistic function has its own corresponding syndrome or deficit; that if different linguistic units are affected differently by the same process, they must have different functions; that a single patient with the right combination of deficits can make or break a linguistic model that applies to normal people; that competence can be separated from performance; and that gross behaviors, if analogically related to a theory provide evidence that the theory is right...like the ability to substitute new words into a retained sentence pattern supports the existence of transformational grammar (Goodglass and Blumstein, 1973). For some reason, these assumptions based on the sacred truth of mimesis, bother me more than another set of tales. Such as the allegory of a family of famous signals, originating beyond the horizon, that find their way into the very body of the observer and traverse his conscious and unconscious terrains. Or the one about the "neural nets" that are hard to pin down
to a structure or a function, but are now the simulacra that brain centers once were. Even though such attempts to represent the kinetic nature of speech have resulted in "treatments" as bizarre as cauteries applied to the neck in order to stimulate a "lame tongue" (Critchley, 1970:54), at least I can imagine some kind of a process and get some sense of involvement. Of course I am not arguing that clinical aphasiology has escaped the trap of representation. Just that with linguistics as its friend, who needs enemies? But,

What Right Have I to Criticize Linguistics

When I don’t even understand it? And, to be perfectly honest, would there be any postmodernism if there hadn’t been linguistics? Imagine Derrida without Saussure ... no signs or oppositions, no signifier/signified to dominate each other, no grams to pun or privileges to hand out in the phonic or non-phonetic realms. And how could Tyler write opaquely if not for the ancient practice of etymology?

Ungrateful though I am, for my purposes it is all about the same. Which is not to say that linguistics has nothing to offer: it is just that, as a system and a whole, it smothers rather than furthers aphasia studies. Saussure, Hjelmslev, Bloomfield, Pike, Jakobson, Firth, Lamb, and Chomsky ... in his historical and formal analysis of these authors, Davis does a good job of showing how much alike they all were in the course of exploring their differences (Davis, 1973).
Linguistics has always worked at being science, which makes it appealing to those neurological practitioners who want to legitimize behavioral neurology, considered too "soft" by mainstream neurologists. Now that we have been given some serious critiques of science and scientism (Latour and Woolgar, 1979; Tyler, 1987 and others), this convenient relationship between aphasiology and linguistics is called into question. What is linguistics anyway (see Word Picture No. 1)? A tangled partonomy related to comparative philology and hermeneutics; a discipline organized around the right to control what is and is not language, that has a comparative and a theoretical branch; a computer virus that has inserted itself into the unconscious and replicated in various fields, like anthropology, literary criticism, sociology, medicine, and cognitive psychology (its preferred natural host). It is not surprising that linguistics seems to be finding a culmination in artificial intelligence, because there is nothing natural about separating language from speech communication. But words are cheap, and of course these criticisms of linguistics apply to other things. I showed my word picture to a linguist who said that it was not parseable; and that she has no realization of comparative philology or hermeneutics in her work and does not think they have anything to do with her. On the other hand, there are people who say that her work isn’t really linguistics because she is interested in things like narrative that are more sociological
than linguistic. This all reminds me of the constant bickering about "what is science?" To a basic scientist, it only includes basic science. To a clinical researcher, it includes clinical and basic science. To a biologist, everything from botany to basic science, and certainly not social science or humanities. To a linguist, everything mentioned so far is science except, of course, sociology. To a painter, everything from literary criticism to basic science might be included. Each term automatically excludes all of those to the left of it in its own definition of science.

Getting back to the point I wanted to make, linguistic theories seem to share a lot of assumptions. First, language is data and you have to decide in advance what significant units you will use (words, pauses, sentences, nonverbal communication...) and what your principles for organization and analysis will be (testing a model, introspection, pattern or other semiotic analysis ...). Second, they are based in various dichotomies such as those between signifier and signified or sound and meaning, between noun phrase and verb phrase, between grammar and phonology, and between superficial and deep. Third, most of them depend on the idea that there are levels like phonology/morphology/syntax/semantics and that these levels are either real or are necessary parts of theoretical constructs (Crystal, 1984). Even ideas such as Halliday's "inter-levels" (mentioned by Crystal) or Lakoff's generative semantics (discussed by Tyler) which challenge the
categories continue to adhere to linguistic methodologies. Even theories that try to deal with pragmatics, the speech situation, and conversation/discourse analysis in general do so in conventional linguistic ways that are based on representation.

Tyler has critiqued additional linguistic assumptions about representation and conventionality and proposed intersubjectivity as a better starting place for speech/discourse analysis (Tyler, 1978). Because we know more than we can say and say more than we know, language cannot be equivalent to thought. Likewise, young children and animals do something in their heads without the benefit of so-called language. So much for the hope of early twentieth century neurologists that linguistic analysis of some conventional language object would refine our representation of language-in-the-brain. Tyler has pointed out that language could never be both the object and means of consensus. Although his use of schemata in The Said and the Unsaid to represent unconscious knowledge kept him within Kant’s tripartite metaphysics, with sensation being transformed into schema being transformed into language (being transformed into iconic sign being transformed into indexical sign, being transformed into symbol), at least he tried to invest his schemata with action under the sway of ethics and to get rid of logic as the only underlying principle for making sense. Common-sense. Because, when you stop and think about it, people don’t really
organize their utterances logically before saying them, even when they speak normally. It doesn’t make sense to expect this kind of organization from aphasics, as is often presumed in analyzing their speech. Consistent with his return to commonsense notions, Tylerダウンplaグ linguistic reference. You can have utterances that are meaningful but do not refer to anything in the "real" world (unicorns have one horn) or utterances that refer but have no meaning (give me the watch, ma call-it over there). He insists that speakers, not words, do the referring. This, too, has implications for aphasics who are often asked to name things, and their failure to do so is used as evidence that some-thing, some image or word-representation, is missing from the lexicon stored in their heads. Even though they can name under some circumstances and not others and may be able to recognize objects they cannot name, which Head demonstrated long ago. Alternative theories about "access" to semantic representations or "decoding" of such representations are still, obviously, tied up with the idea of word-like things stored in the brain. Tyler says that an answer to a question about word meaning will take into account who wants to know and why, since the match between words and things is indexical, analogical and inferential rather than fixed.

Besides his criticism of linguistics’ formalisms and fictions about conventionality, Tyler finds fault with it for being biased toward the hearer’s "point-of-view" (a term he
would most certainly reject if asked today) and with its dependence on the idea of a translatable and transcendental signified. But I do not really want to go into the metaphysics of transcendent presence at the moment. I do want to highlight the idea of meaning schemata as some kind of mediators between conventionality and subjectivity, recognizing that even these terms are wrought with the idea of presence. What I like about the idea is that individuals can manipulate the collocations or meaning schemata that comprise semantic structure, and can manipulate the vocabulary at their disposal in order to express and interpret relationships between what he calls predicates and arguments, as well as to infer what is not spoken or not heard. What was that? I mean his idea includes these patterns called collocations, like:

"____ is a kind of ____" or
"____ is a noun(er) of ____" or
"____ precedes/follows ____" or
"____ is to ____ as ____ is to ____,"

which seem pretty useful to me. I also like the idea that these processes help individuals to infer unsaid information. And who can deny Tyler’s claims that certain structures are conventional in the process of discourse? Like speaker/hearer turn-taking, sequences that are somehow related to topics, and certain "utterence sequences" which include entailment, presupposition, equivalence, anaphora, and synonomy.

Tyler wrote that science, structural anthropology, and
linguistics are all formalisms divorced from the world and the community of speakers (Tyler, 1978:464-65). Sure, I agree, but what is the problem with science anyway? Is it just that science is not clear about what it is describing, as Tyler suggests? Yet, he acknowledges that reference is not essential to the meaningful use of words in rhetoric, so science might be described as just another rhetoric. Or is it that scientists think they are creating new knowledge and overcoming the past, when Kuhn and Latour have shown that it doesn't work this way. Maybe science is a failed way of thinking because it is too totalizing, and destined for deconstruction by the other discourses it has taken over (Porush, 1989). Science, it seems, is more problematic for anthropologists (who have had to balance it with relativism [Mertz, 1987]) and for practitioners of the humanities (who have sometimes feared it) than it is for Derrida, who would like to see a little more of it informing the system of signification (Derrida, 1981:34-36).

I guess I do not really think that there is something hopelessly and especially wrong with science. But I am convinced that there is a difference between scientific thinking and not-scientific thinking that has to do with whether or not everything matters, carries significance, signifies, counts as good data, or however you want to put it. Science spends a lot of effort deciding what is real and what is artifact or just the product of chance, which means that
some of what it does it considers insignificant. Not-science, like literature and psychoanalysis and art history and philosophy and anthropology (when it is not concentrating on being like science) assume that everything matters. Even slips of the tongue. Even what goes into a collage, or remains unsaid or is otherwise absent but suggested by something not exactly absent and not exactly present. Derrida addresses this issue of too much meaning and how it is managed in some of the writings he critiques, although he does so without acknowledging the ubiquity and totalizing tendencies of meaningfulness itself (Derrida, 1978). According to Alan Bass in his introduction to Writing and Difference, Derrida thinks that "every totality is founded on what it excludes" ... and totalities exclude all "that would be in excess for a reductive analysis." Systems don't work because they can't deal with all of the meaningfulness that doesn't fit in. The interesting thing is that systems work better in practice of science than they do in not-science. Maybe this is because scientists always lag behind in understanding themselves and the epistemes that govern them, but I doubt it. Or maybe it's because they have found some ways to throw out the excess and are more comfortable with randomness and with a life in which some-things are meaningless. Derrida might dismiss these excess-trimming practices as empiricism, but if they are empiricism, the term needs to be understood oppositionally (rather than dismissed categorically), and could be tooled
into a pseudo-derridean chain something like this: sense/non-sense, not-science/science, dissemination/empiricism, deconstruction/bricolage. If pressed, most people would probably agree that some of what happens to them is meaningful and some is not, but these biases of "to mean" or "not to mean" seem tightly wound up with proper practice in most, if not every discipline. Some fields even fragment themselves into sub-fields on the question, like psychology which contains both cognitive psychology, most comfortable with a single subject in whom everything is meaningful, and neuropsychology, with its battery approach to human behaviors.

Unfortunately, modern science, in trying to decide what does and does not matter, has tied the decision process to a quest for hidden truths. It often believes its own rhetoric passionately enough to resist notions such as the social construction of scientific facts (Latour and Woolgar, 1979). When belief fails, it falls back on irony and longing for an idealized past before money had corrupted research and before the government began dictating what could be studied. But modern not-science does not really fare much better, what with its insistence on explanatory representational truths, its totalizations, and its penchant for attributions such as pretty/ugly, (primitive or simple)/(sophisticated or complex) and right/wrong. Of course, science has its misguided representations too, and not-science falls into all sorts of method-related traps. That is why postmodern critiques seem
to work for everything from physics to literature. I'm just saying that the different activities in science and not-science lend themselves to different semiotics/not-semiotics, and so their critiques are likely to take different rhetorical turns. They are probably even necessary to properly deconstruct each other, if that is what the postmodern perpetrators decide to do.

Getting Back to Speech

Would be a welcome break from these generalities that seem so obvious. Derrida has said that speech is tied up with a "phonocentric" bias in Western history, a bias that privileges the voice, especially within writing, and that privileges presence/self-presence (Derrida, 1981) Oh, and, he says that writing, "archi-writing" is the root of both speech and common writing. But other than these observations, he does not seem to deal with speech, as in people communicating with each other very much. So, regarding this idea of "voice" as what distorts writing (his major interest) by creating a transcendental subject, Tyler points out to my satisfaction that voice is not the problem (Tyler, 1978:8). Derrida gives priority to the signified -- no, I meant the signifier -- which has lots of implications. He thinks that pushing the signifier to its limits risks meaning nothing (his idea of play), and that this might help us get at "thought-that-means-nothing" which is better because it "exceeds meaning and
meaning-as-hearing-oneself-speak" and is neither graphocentric nor phonocentric (Derrida, 1981:12). Also, by overturning the "hierarchical" opposition speech/writing, he creates an interval before "new concepts" emerge and the exploration of this interval is called "la dissemination (Derrida, 1981:42)."

Another thing ... there is this word "différence" in which the "a," which is read but not heard (at least in French) gets at deferred presence, the production of oppositional concepts, the differ-ence between being and beings (which is not tautological because of the "a") and is related to "grams" and "traces" which also suggest nonpresence. How does the "a" do all of this? Etymologically, because difference with an "a" (différance) suggests différent which does not exist but would be like the present participle of différer - to differ and to defer; sort of the words differing/deferring used as an adjective. Différence also suggests différence, of course, meaning difference. Clear as mud? Derrida, maybe, explains it better than I do. If it is clear, it is because this is all written down which testifies to the success of the derridean project.

Derrida’s gramm-atology, his exploration of the structure of nonpresence, is supposed to free writing from speech and open up the whole "scientific field of the graphic substance (Derrida, 1981:26)." It does so through and because-of privileging signification. His implication is that by transgressing one’s own voice-of-oneself and the voices of
others we will learn something "new" about thought, the "thought-that-means-nothing" that I alluded to a minute ago.

But I thought I was getting back to speech, and instead I'm writing about written language. If you give up people—in-their-voices, what is the basis for intersubjectivity? Certainly not some transcendental signified, since no-thing is there. This questions about intersubjectivity reminds me of discussions about fieldwork. When anthropologist-subject and native-subject speak different languages, the lack of transcendental signifieds is easily attributed to problems with language. When use of particular languages seems more balanced, the mis-statements and uncontrolled references arising from conversation can be attributed to cultural differences. Tedlock suggests that dialogical asymmetry is a prerequisite for ethnographic research (Tedlock, 1987). For him, evocative translations across this asymmetric field, especially if informed by multivocality (in the "original" conversation, and in the re-presentation) are the desired aim and object of fieldwork. Tyler counters that this amounts to another attempt to representation, and is therefore doomed not only to failure, but to repeating the mistakes of representation, including political repression (Tyler, 1987). Tyler sees in dialogial writing the whole "conundrum of participant-observation" where the ethnographer pretends to have participated in what he later tries to evoke through representation (Tyler, 1987:341). Meanwhile, because he
ultimately aims to represent his participation, there is no chance to actually participate. This conundrum applies as well to the aphasiologist whose pre-conceived categories of aphasia condition all dialogue with aphasics. The hope that dialogue itself can be used or captured is demonstratively misleading.

Derrida says, on the other hand, that subjects are only constituted by being divided from themselves ... an example of what he calls deferral; and subjectivity/objectivity, which is different from subjects/objects, are effects of différance. These are useful ideas when we're deconstructing presence. But where is intersubjectivity; or, how are subjects constituted to each other? Derrida seems to rely vaguely on that old linguistic notion of "conventionality" in which the shared use of signifiers allows "communication," although his version of it allows for a pervasive mobility of signifiers that would bother most linguists. And since he is not writing about speech as communication, he must be writing about technology: writing liberated from speech but leashed to his own purposes. Kristeva asked Derrida if language is non-expressive, isn’t it just a logical-mathematical notation? And he said that allowing a little science and math into writing, which has kept them out for so long, wasn’t such a bad thing (Derrida, 1981:34-35). Tyler has critiqued the ideas of "rebus writing" and the "crypto-positivism" of Derrida in general (Tyler, 1987:2-59). So with a mental note
that Derrida’s crypto-positivism may be a struggle within himself over excess meaning, I would like to focus on something else, the "free play of signifiers" as another application of privileging the signifier and as a concept which, when considered in relation to aphasic speech, shows some of what is wrong with the derridean project as a means of finding out about thinking. Because if there is ever a free play of signifiers in speech, it happens in aphasia, and it does the opposite of getting at the kind of evocative thought Derrida implies by thought-that-means-nothing.

I have never seen a good representation of aphasic speech written down. Most leave out the physical setting, the relationship between the speaker and everyone else around, the identity of people who were there when the speaking was said or who had something to do with making the saying into a writing sample. They cannot quite convey the pauses, pace, implications or anything else that is unsaid; even though Derrida implies that words alone are sufficient to convey the traces of words not there. Written representations of aphasic speech are therefore like all attempts to report dialogue or the representation of someone’s speech in an ethnography. Film may be a little better, but not if it pretends to be free of entextualization with all the weaving and reweaving problems that infest written webs of words. Dialogue, says Tyler, is the work that makes no work and leaves no traces (Tyler, 1987:58). Discourse is the object and the means of
postmodern anthropologies as espoused by Tyler, and a similar object/means conjunction applies to aphasia studies which, I hope to have suggested, have been a sort of ongoing field work that neurologists have been engaged in for a long time.

Maybe I’m suggesting that the joint and separate ventures of neurology and linguistics to represent anywhere (in the brain, on the page) aphasic speech, to identify the parts (signs, clinical or convenient; signs, immanent or conventional) and fit them into represented wholes have already failed. Aphasia has become a "secondary orality," modeled on written speech, that can only reflect back the metaphors and allegories of representation. All of these statements are in keeping with Derrida’s critique of representation. Except that his critique of phonocentrism claims for writing the freedom of sign play that happens in aphasia, and doesn’t really happen in writing. And this free play of signs con-signs communicants to abnormal roles of exchange; to misunderstandings; to coercion; to reductive gestures; to slavish dependence on visual images; to parent-child relationships and many more undesirables. Aphasic discourse becomes work that makes more work. But people with aphasia do talk: and since they are talking, they sometimes signify with speech even though a particular signifier might be used in different ways -- to refer to different things, to get speech started, to evoke a memory in an interlocutor, to mean its opposite. And different signifiers might be used for
the same purposes, as when yes or no might mean yes, no, or maybe, or something else entirely and you may never figure out a way to predict when either word will mean what, or mean nothing at all. Where is the play that Derrida promises when he spells out a program for pushing signifiers to their limits? The free play of signifiers, if it allows for the absence of signifieds (as in writing) and ignores the circumstances of intersubjectivity does not make sense, common or otherwise.

Since I've already indicated the difficulty of representing aphasic speech, I have no choice but to examine some arbitrary examples of aphasic writing for the points I want to make about Derrida, which might initially tend to serve his purposes more than my own. Writing is not all that important to most aphasics. Some didn't do much of it before becoming aphasic; many acquire a right hemiparesis along with their speech affliction and, since the vast majority are right-handed, they don't want to spend the effort to write with their left hands. Still, conventional wisdom holds that "almost without exception, every aphasic suffers some difficulty in writing (agraphia) (Benson, 1979:36)." Benson notes that, although many patients retain their signatures, most have a combination of disturbed orthography (especially when using the non-customary hand!), abnormal syntax and/or semantic content, and disturbed spelling. He doesn't include many samples in his book; just a few scattered words and
attempted sentences (see Word Picture No. 2). You can see his concerns in the explanation that goes along with the samples: things don’t look as expected and don’t flow as expected. I should mention that the third sample seems to be a written description of "The Cookie-Theft Picture," a standardized picture used in language tests that caricatures events in the life of a housewife and her family. Note that the graphic substance of these examples is free from the expected voice of the author and is at odds with the voice supplied by the reader. To borrow more of Tyler’s terms, the author’s irrational subjectivity doesn’t quite become the objective subjectivity of a rational transcendental ego (Tyler, 1987:8). In other words, the illusion that you the reader and the person who wrote the text are merged in some kind of understanding breaks down. Derrida might say that the flawed writing just highlights the infringement of aphasic voice into the realm of writing because it intrudes the patient’s "voice." But let’s go on.

MacDonald Critchley devoted a chapter of his book to the "creative writing of aphasics (Critchley, 1970)." Unhappily, I am reminded of other thematic collections, such as "paintings by the mentally retarded," but leaving the connotations of his grouping aside, Word Picture No. 3 once belonged to a young man whose failed suicide attempt resulted in aphasia. Afterwards he preferred poetry to prose because he found it easier. In "Eyes" we begin to sense the
signifiers straining out of control, especially in those instances where he scratched something out and replaced it, like "wind from run/run from wind," although we don't even know which scratch-outs happened when he actually wrote the poem down and which are revisions. The x-outs in "Sleep" would suggest that he made them the first time through, since the poem is typed. But, what kind of typist was he? And, since anything passes for poetry it's hard to say what has gone wrong with his language. His poems, read aloud, give a feeling for the way some aphasics put words together in speech. The reader will have to judge for herself his degree of artistic play.

But what am I getting at? Certainly not a new kind of textual analysis. I am just supplying words around the figures, as is customary. I am writing about writing and speech and circumstances and play. Or the differences between fieldwork and library work, or sense and non-sense. I have the feeling that things would be clearer if I had been there to watch him writing ... or, even if I could just ask him about a few things like "did you mean to type that?" or "what do you mean by this?" What I should do is sit back and acknowledge the signifiers at play ... see what I repress by assuming that speech is somehow important to writing a poem or that writing is somehow secondary to speech. Look at Word Picture No. 4.

"on the on xx the on Justly"
(it’s writing all right)
"saw the was xx xx sooly the xx"
(words on a page, and erasures)
"swowly swifty xx the far xx xxxx"
(evocative)
"showly think xx were sut/blied if any
(but it doesn’t make sense...)
"offense if is fateful xx xxxx in forgets"
(alliteration! Finnegan’s Wake)
"Cowels anything something xx xx pvcocanel"
(contradictory)
"All attlhough announsid xxxx openly"
(open the text)
"Sho?l whish jest bripow sh xx anthidrol"
(dialect-dialectics)
"?hithises thypestric xxxx xxxx"
"losir xxxx power xx loure"
(power/rhetoric)
"xxx a la xx maison a la xx demsion"
(bilingual signification)
"xxx larbit xxxx proving drowng"
"xxxxx admitting that at least power"

Presumably, the patient who wrote this understood what to do — to write. Or, to write a description of his own illness. It could be read as a free play of signifiers, almost a poem with "erasures," but only if you make no identification with
the writer-at-his-task.

But just a minute here. Derrida never perpetrated a theory of aphasia, and he was not talking about "communication" when he established his deconstructive project for speech and writing. He purposely overturned the hierarchy of speech-dominating-writing to allow dissemination, and in doing so he came up with a lot of interesting notions about presence in western metaphysics, about resonant traces of absence, and many other influential and important ideas. Unfortunately, his strategy is reminiscent of Saussure's when he separated speech and language and decided to make the latter the focus of his systematic, efficacious, historical and synchronic, scientific analysis. This sort of rhetoric is what grams Derrida in philosophical discourse and gives his readers traces of a feeling that what he is all worked up about is a little removed from their everyday lives.

Postmodern anthropology argues that sign and signified are mutually constituted in a kinetic process of saying: mimesis, on the other hand, emphasizes the differences between signifier and signified, especially when it entertains the possibility of either being absent (Tyler, 1987:194). This is why Tyler thinks that Derrida is still tied up with mimesis, and based upon my "derridean" brush with aphasia, I'd have to agree. Tyler argues for postmodern ethnography to act as a performative break with everyday speech in order to evoke the ethos of a community and to provoke its hearers to act
ethically. This project is appealing but one can honestly ask for a demonstration of its success, especially since Tyler also perpetrates his belief that science fails to reconcile the competing demands of representation and communication, and banishes it from postmodern ethnography. He points out that discourses of work (politics and industry) and value (aesthetics and ethics) have taken over science, an observation that points out the descriptive power of postmodernism that is, however, separate from its contra-modern rhetorical acts, such as perpetrations and banishments, overturning hierarchies and related activities. Science has not "failed" because it is different from not-science. It will continue to elaborate a response to the condition in which not everything means, just as those disciplines which take universal and minute meaning for granted will continue elaborating responses. Aphasiology is one discourse through which these counter-currents flow, and may yet be informed by both. It is a discourse whose history rewrites other histories through examinations of what sometimes count as speech and sometimes as language. Aphasiology should have no use for the science of linguistics, even as it probes with little bits borrowed from linguistics, which, I am convinced, cannot be avoided. And it should enjoy the evocations of postmodern rhetoric so long as some echo of the old doubting scientism remains to suggest that they may not matter. Because everything done is an approximation to what will
happen next time, unless you change your mind.
List of Figures

Word Picture No. 1
Toward a DiaSynchronous Mapping of Linguistics

Word Picture No. 2
Aphasic Handwriting Specimens
(from Benson, F., Aphasia, Alexia and Agraphia p. 124)

Word Picture No. 3
Poems written by an Aphasic Patient
(from Critchley, M. Aphasiology p. 253)

Word Picture No. 4
Aphasic’s Description of His Illness
(from Critchley, M. Aphasiology p. 267)
Toward a DiaSynchroonic "Mapping" of Linguistics
Word Picture No.1
The woman is emptying a desk while looking out the window. She forgot the water was on, and it is overflowing on to the floor.

The little boy falls from his stool while trying to get some cookies for his sister and himself.

Figure 12-1. A. Handwriting specimen from patient with Broca aphasia: note the big, messy letters and misspellings. B. Handwriting specimen from a patient with Wernicke aphasia showing well-formed letters but incorrect spelling and word use. C. Handwriting specimen from a patient with bifrontal damage; note multiple o's, i's, r's and m's in an otherwise adequately written description of a picture.
Aphasiology

Eyes
The single eye [looking] in the portrait and landscape
from the tree, a part by writer.
Our eye outside and inside in color face,
right body and symmetry proportion.

Mountain, stream, forest and always time,
river, stream, brown from and exact structure.
Smiling mother [was] face - not eye,
yellow light and right shadow, saint, kingdom.

Artistic eye
A shot from portrait, see the single eye;
Smiling [was] natural, light from single eye;
A ball - tennis, soccer, fiddle, five - game, from single eye;
Bamboo, oriental, [teepee] - mangled through single eye.

Psychological word, sleep and dream & democracy -
"Vile, try - anything," "Th music is Die,"
Evil, a pure eye.
and death.
Old school, limited time, shadows in time gap.

(a)

to sleep

valley

be the [of]eaves the azure rose of the twilight,
stars, twinkling thousand stars - ares, leads, zeus
a silent star of earths, callows of night.
Diet, quiet time of the petals of a flower, open rose


The breathing sleep, a quiet, silence man u. till
The - at island, eden, a mournful woman - grave.
earth the six feet, of churchyard the windy sea and fields;
spike, sailors dance, priest, jigs,
and silent peasants unto the grave, bell, church and priest.

Sleep, sleep t time, to end moreover dream,
Sleep, myriad brain speak echo, echo.
Sleep, the exchange silent cellbrain.
Sleep, die tomorrow night to G d heavenly reign.

(b)

fig. 9 (a) Manuscript of a poem written by an aphasic patient (Case 2).
fig. 9 (b) Poem written directly upon a typewriter (Case 2).
patients are quite incapable of writing anything else. The personal and individual qualities which are attached to the act of signing one's name mean that it is not much more than a motor automatism.

A slightly more difficult act, but still one which occupies a specific and limited place, is the execution of one's address. This task is facilitated if the command is given in the form ... 'write down your name and address'.

...theon...on

FIG. 16 Aphasic patient's description of his own illness.

Higher in the level of written performance, is the act of writing to dictation. Obviously the performance will depend in part upon the relative difficulty of the test-situation. Thus the speed of dictation, and the linguistic and syntactical nature of the text in question will very much influence the ease and correctness of the performance.

Yet another test of writing is the ability to copy a given text, whether it be printed or in script. Some aphasic patients utterly fail to carry out such a request. To this defect the term 'acopia' has at times been applied. Others succeed, but in diverse ways. For example, one patient may reproduce a text with faithful exactitude delineating precisely every graphic peculiarity of the model before him. Much the same would not be beyond the powers of a European who is asked to copy a paragraph of Chinese, Sinhala or Arabic. Understanding is lacking and the exercise is no more than a fragment of representational drawing. Other patients may be able to go further. Thus they can perhaps copy a piece of writing but in their own individual manner; or they may be able to convert a printed text into cursive writing, or vice versa.

Far more important as an index of writing-ability is the act of semi-spontaneous essay-writing. This is done by instructing the aphasic patient to write a few lines, or a paragraph, or a page, upon a set theme. Thus he may be told to pen a few sentences à propos of the weather; or a short account of his journey to the consulting room; or the clinical history of the course of his illness. If need be, the patient can be spared the embarrassment of supervision, and he may be left alone to
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