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SCIENTIFIC THEORIES AS GOING CONCERNS:

THE DEVELOPMENT OF THE LOCALIZATIONIST PERSPECTIVE IN NEUROPHYSIOLOGY, 1870-1906

by

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DISSERTATION

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ABSTRACT

This thesis in the sociology of science describes the development of a scientific perspective as based in daily work, professional and institutional constraints. The development of the localizationist perspective in British neurophysiology from 1870-1906 is analyzed. This perspective developed from four lines of work (neurology, physiology, surgery and pathology), and with several intersecting contexts (the growth of medical specialization, lack of support for British physiology, professionalization of physicians and surgeons, the antivivisection battle, and patients' physical and economic situations). Its components included the ideas that mind is carried upon an anatomical substrate, within a hierarchically-ordered nervous system; that the individual mind is contained within the skull; and that nervous system functions may be observed by deleting parts of it.

The thesis argues that these multiple contexts combined to form a perspective that became both robust (drawing on and applicable in many sites) and obdurate (constructed in ways that were not publicly replicable). As a successful perspective, localizationism had five properties: inertia (once set in motion stayed in motion), momentum (rapid spread), progressive inseparability of parts, cumulative reification, and incompleteness at any one point.

The data for the study are drawn from several sources: laboratory notebooks of localizationist David Ferrier; records of the National Hospital for the Paralysed and Epileptic, London; nineteenth century medical and scientific journals, and modern secondary sources. A pilot study was conducted at a modern neurophysiology lab in order to develop theoretical sampling questions about daily work. The analytic perspective employed here is symbolic intraction/Pragmatism, emphasizing work and negotiatons as well as the material constraints upon them. The data are analyzed with the grounded theory method, a comparative, inductive technique used here qualitatively.

The six chapters of the thesis analyze different aspects of the perspective's development: the formal analytic properties of a perspective; institutional and professional contexts; uncertainty in daily work and triangulation of clinical and basic evidence; tactics in the debate about localization; the adoption of psychophysical parallelism by neurophysiologists; and the persistence of the perspective and an overview of its fate to the present. Two appendices present translations of excerpts from the work of antilocalizationists F. Goltz and M. Panizza.

for my parents,

Glenn and Shirley Kippax,

with love and respect

and for the memory of

Everett C. Hughes

(1897-1983)

with thanks for his work

I am neither a universalier nor a localiser...In consequence I have been attacked as a universaliser and also as a localiser. But I do not hold that the view I really hold as to localisation has ever been referred to.

--John Hughlings Jackson

The structure and working of the brain had been laid bare, and the stupendous fact had been established that to each of the cerebral hemispheres were allotted functions distinct and separate. These enthusiasts, pursuing their investigations under discouraging conditons with an untiring patience which invested their intelligence with genius, demonstrated that every individual portion of the seemingly homogeneous organ was allotted its own particular task, and in response to the probing interrogations of science every fibre and filament of the complex structure yielded up the secret of its being.

--Burford Rawlings, administrator of National Hospital for the Paralysed and Epileptic in the 19th century

As to them blooming doctors, we teach 'em a lot, I know. Lor ! how they do jaw about our insides to them blokes as sits and looks on.

--Patient, National Hospital for the Paralyzed and Epileptic in the 19th century

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PREFACE

It was a cold, grey London morning in 1875. A bell rang repeatedly inside the National Hospital in Queen Square, slowly rousing the night porter/guard who lay slumbering in his chair near the back ward. The guard opened the door to see two men, one a disgruntled fellow of middle-age, hat pulled down over his eyes against the cold, supporting another who slumped against him, eyes closed.

"This here's the fits hospital, right?," said the disgruntled one. Upon hearing the affirmative, he shook the fellow leaning against him and attempted to push him into the arms of the night attendant. "Here, what's this about?," said the surprised porter. The porter noticed that the arms of the limp man had been bound to his sides with rough rope. Suspecting foul play, he began to explain that this was no general hospital or emergency ward, but that patients could only be admitted here with the approval of a sponsor, with the diagnosis of nervous disease, paralysis, or epilepsy.

"The station-master give me the fare to bring this poor bloke over here, that's all I know," said the first man, evidently a cab driver. "He can't talk, can't walk, and shakes all over every five minutes. Got shipped down here from somewhere out in Yorkshire, the guy at the station said. Folks didn't know what else to do." There was no sign of recognition or affirmation from the bound man. The porter reached out and fingered a label that had been attached to the rope on the man's body, a sort of large, crude mailing label. In rough handwriting were simply the words, "Fits Hospital, London."

Suddenly the man began to shake, wrenching from the cabbie's grasp, and fell on the ground. The reason for the ropes became apparent as he flailed from side to side, arms straining against them. The cabbie and the night porter dragged him across the threshold. They attempted to put a couple of pillows around him so that he wouldn't hurt himself, and placed a bit of cotton wadding under his tongue.

The man could neither speak nor walk, and apathetically lay in the back ward for several days. Attendents administered pottasium bromide, and held him down when the fits came. Finally, Hughlings Jackson, physician to the hospital and expert on epilepsy, made his weekly visit to patients. The patient had been quiet for several hours and was resting docilely in the bed,looking blankly at the ceiling. As Jackson was making his rounds, however, a fit began. Jackson was quickly summoned to the bedside. He observed the sequence of spasms, which began in the man's left finger, moved up the arm, gradually involved the whole body.

He diagnosed epilepsy with a localized origin in the left temporal lobe of the brain. More potassium bromide, electrotherapy, hydrotherapy and massage were prescribed.

Subsequent fits were recorded in detail by the night porters and day attendants, as they attempted to replicate Jackson's observations about the origin of the spasms, duration of fits, and severity of spasm. The patient did not respond to the bromide, and both electrotherapy and massage frequently brought on fits, so the attendants tried to make these therapies as short and infrequent as possible. The porters tried to notice the details of the fits, but it was difficult. Often, the patient would begin twitching or flailing about in the middle of the night. Sleepy attendants would rush in with a candle and make sure the patient came to no harm. The light and noise wakened other patients in the ward, and calming the disruption took some time. Afterwards, the attendants would attempt to fill out detailed "fits sheets" which recorded observations of each fit including sequence, muscles involved, and where the twitching started.

After three months, the patient suddenly went into a coma and died. With no relatives in the picture, a rare opportunity for a leisurely autopsy was afforded to Jackson and the hospital staff. Jackson's thesis was that the tumor had destroyed parts of the speech and motor areas, causing the inability to speak and the paralysis. The fits had come from what he called the "discharging" properties of the tumor, which animating the muscles.

Thus, from analysis of the disordered behavior and presumably disordered mental capacities of the patient, Jackson and the other researchers at the National Hospital for the Paralysed and Epileptic, Queen Square, London, searched for, and found, the physical areas they thought caused the behavior. They were able, on this occasion, to open up the mysterious twitching "package" presented at their doorstep, and point to what they believed to be a juncture between the mind and the brain.

Meanwhile, down the street, in a dimly-lit laboratory without heat or running water, David Ferrier was performing his second operation of the day on the brain of a macaque monkey. The first animal, earlier that afternoon, had died from an overdose of chloroform; Ferrier feverishly hoped that this one would live long enough for him to observe the effects of the lesion he was making in the brain. If he could do that in the same area as Jackson's patient had had the brain tumor, and if the monkey stopped walking or moving, further evidence for the link between motor activity and a certain area of the brain would be found.

Ferrier was partly concerned because the monkeys were so expensive [1], and partly because the antivivisectionist movement was growing stronger and more politically powerful. If he could only get positive results before they moved to close down his laboratory...!

It had been a long week. Yesterday's experimental subject, a large female macaque, had been a most recalcitrant subject. She ran away from Ferrier, snarled and fought back when he'd try to apply the galvanic current to test her muscle movements.

Even when things went smoothly, it was often hard to tell exactly which functions had been impaired by the surgery. Were the limbs twitching, or moving under the electrical stimulus? Was the paralysis from impairment of an area of the brain, or was it shock from the operation itself? Ferrier often could not be sure.

Finally, the monkey began to come out of the anaesthesia. Wearily, Ferrier sat down across the room and waited for it to come to, after bandaging up the head wound from the operation. He lit a gas burner at one end of the room, and made a pot of strong tea.

A couple of hours later, the monkey irritably clings to the hot water pipes, the only source of heat in the cold basement laboratory. Ferrier gives the animal a saucer of tea, and notes that the animal is able to drink it.
Ferrier, like the night attendant in the hospital, tried to jot down an accurate record of the symptoms exhibited by

his subject, including twitches and epileptic fits. At night's end, Ferrier and the monkey stare at one another across the lab, drinking their resepctive cups of tea.

The circumstances described here in both situations are composites, drawn together to convey a vivid description of the work situations faced by these scientists [2]. Their aim was to discover the nature of brain functions, especially the link between brain and behavior.

An instructor in my first sociology class remarked that beginning sociology students are always amazed by the number and variety of breakdowns, mistakes and incongruities in the field sites they choose to study. "What never strikes them at first," he chuckled, "is that what is really amazing is that there should <u>be any</u> order, any regularity there in all that chaos ! Now <u>that's</u> what's really amazing !"[3]

The remarkable regularity investigated by this thesis is the development, late in the nineteenth century, of an extremely successful theory about the nature of the brain. The theory arose within the work circumstances described above. The thesis is the story of the scientists at work, including the formidable practical problems and uncertainties they faced, their resolutions of them, and the role played by their patients , institutions, professions, and political enemies.

NOTES

1. Cushing's 1901 visit to Sherrington's Liverpool laboratory notes that a gorilla cost "over $1,000" at that time. Even by 1901 working conditions were difficult: "Eight-hour observations in a room at 82 degrees-87 degrees...Hard work. The gorilla was anaesthetized in his cage with blankets around it. We also were almost anaesthetized at the same time. Grunbaum had a revolver so that if [it] came to the worst, there would be no scandal" (Fulton, 1946:198).

2. Information here is drawn from the following sources, and is more formally discussed in Chapter 3: Rawlings, 1913; Holmes, 1954; Ferrier, 1873-1883; Sachs, 1957; National Hospital Records (1870-1901).

3. L. Schatzman, personal communication, Fall, 1978.

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INTRODUCTION

1. Scientific Theories as Going Concerns

The thesis presented here is that successful scientific theories are going concerns (Hughes, 1971c:52-64). Hughes described going concerns as open systems (Hewitt and de Jong, 1982) of actors, in collective enterprise, with a present identifiable existence and a historical dimension. Going concerns have personnel, clientele, and financial support. They "occur in many forms, and may be in any stage of having, getting, or losing moral, social, legal or simply customer approval" (Hughes,1971c:54). They may be well or ill-organized, old or new, have a firm mandate for action or not. To say that a theory *is* a going concern is to emphasize that my analysis is of collective behavior, not simply ideas.

Going concerns are strategically informed and legitimated by perspectives. As defined by Becker, Geer, Hughes and Strauss (1961:36), these are:

> ...modes of thought and action developed by a group which faces the same problematic situation. They are the customary ways members of the group think about such situations and act in them. They are the ways of thinking and acting which appear to group members as the natural and legitimate ones to use in such situations...Perspectives differ from values in being situationally specific; they are patterns of thought and action which have grown up in response to a

specific set of institutional pressures and serve as a solution to the problems those pressures create.

By studying the development of a particular perspective in scientific work, I want to illuminate both how collective ways of thinking and acting come to be seen as natural and legitimate and what institutional conditons shape the problem-solving perspectives of scientists. The relationship between scientific work and scientific theories has been a relatively neglected problem in sociology. Recent work from a symbolic interactionist/ Pragmatist perspective has sought to fill the gap (Gerson, 1983a; in prep.; Volberg, 1983a; Star, 1983; Strauss, et al., forthcoming). Like other interactionists studying work, I focus on negotiated meanings within the work context, and emphasize comparative types of technical work rather than seeing science as a special case.

I also attempt to avoid idealized analyses of science which examine the logic or internal problems of a scientific discipline but which ignore human actors, material conditons of production or relations of production. Hughes (p. 54) said that one of our chief sociological tasks is to discover the relations between the present existence and the historical dimensions of going concerns--the choice of a historical problem here is a response to that mandate.

The historical problem which prompted my research was the emergence, at the end of the 19th century, of an extremely successful theory about the nature of the brain. Brain research has a long and chaotic history. From Aristotle to the present, its hallmarks have been bitter debate, disagreement and confusion in the face of enormous complexity. Broadly speaking, two classes of theory have been accepted at different times and places: localization theories and diffusion theories (Tizard, 1959; Swazey, 1970). [1]

Localization theories hold that the brain is composed of distinct parts, each with its sovereign function. The familiar term "speech area" reflects this view. Diffusionist theories hold that the brain operates holistically, without separation of parts or division of labor. Phrenology is a familiar example of a localizationist theory popular from the end of the 18th century, until about 1830. It was superceded by the diffusionist theories of Flourens, who held that the cerebral hemispheres were an undifferentiated functional whole.

During the last thirty years of the 19th century, in the West, a theory of functional localization in the brain was produced. Despite an acrimonious debate with diffusionists, and despite unresolved inconsistencies and anomalies in the theory, localizationists were so successful that they came to dominate work in medicine, physiology, anatomy, and neuropsychiatry until nearly the present day. Localization theory as a going concern formed the basis for the psychosurgery of the 1940s and 50s; for functional maps of the brain, still in use; for diagnosis and treatment of head wounds, brain and other nervous system tumors; and many speech and behavioral disorders (see e.g., Penfield and Roberts, 1959).

2. Approach of the Study

Many stories have been told about the development of localization theory during the last quarter of the 19th century. For some, its appearance is a relapse into phrenology, a revival of the doctrines of Gall and Spurzheim, never fully "weeded out" from medical thought. For others, association psychology lent its geist to the development of localizationism, and the functional regions of the brain were the logical extension of the philosophy of Bain (Young, 1970; Warren, 1921) and other associationists. For most medical historians, however, cerebral localization was simply a discovery of a fact that is there in nature. It was neither a debate nor part of a perspective, but a positive discovery resident in anatomy and function (see e.g., Brazier, 1959).

I began with the premise that scientific theories, regardless of how I would evaluate their truth status, are made, not born. That is, facts are not "unearthed," but, rather, assembled, proposed and defended. One important aspect of this process for scientists is managing the constant uncertainty and complexity which natural situations present.

Recent work in the sociology of science has indicated that there is an enormous variety in the methods used and conditions under which the complexity of nature gets made simpler and more manageable. Sometimes local politics decide how the field of investigation will be limited. At other times, a narrow focus on one "building block," seen as part of a larger theoretical whole, effectively screens out other complexities. Or the availability of a given piece of machinery will constrain the complexity of the problem. Without X-ray crystallography, for instance, it is difficult to perceive the complex form of the double helix. There is constant <u>reconciling</u> between <u>theoretical commitments</u> and the <u>constraints on material resources</u>, including time and staff.

Philosophers of science and intellectual historians have focused almost exclusively on the development of theoretical commitments in science, omitting mention of material constraints. By contrast, sociologists and economists have often focused exclusively on those material aspects constraining scientific development. From either perspective, the reconciling process <u>per se</u> remains unexamined.

Scientists themselves lose sight of their reconciliation work both in execution and description. Scientific work is complicated. Any set of scientific tasks involves multiple problems, qualifications, exigencies, demands and audiences. To work without getting lost in endless contingencies, scientists must draw boundaries and exclude some kinds of artifacts and complications from consideration (Star and Gerson, forthcoming a and b; Lynch, forthcoming).

In other words, part of doing science is transforming problems with many contingencies into those simple enough to work on. In "The Structure of Ill-Structured Problems," Simon (1973) discusses this transformation. He notes that computer scientists ordinarily distinguish between "well-structured" and "ill-structured" problems. Well-structured problems are those for which all possible contingencies can be programmed--no new contingencies arise as a result of the problem-solving process. Ill-structured problems, on the other hand, develop new and unpredictable contingencies in the course of solution.

In fact, the number of significant well-structured problems in the real world is almost nil. Scientists break ill-structured problems into pieces which they work on <u>as if</u> they were well-structured, in order to get the work done. Creating well-structured problems requires ignoring complexity: uncertainties in the environment, subjects' or patients' reactions, unforeseen interaction effects. In order to actually do the research, lines and boundaries must be drawn around complications, implications, and exceptions. Goals, images and tasks simple enough to manage are developed. Simon (1973) notes that in the process of transforming ill-structured problems into well-structured ones, the relationships <u>between</u> well-structured problems are ignored. Learning to manage all of the difficulties that arise from these relationships is a major part of professional training and "maturity."

The process of creating well-structured problems from ill-structured ones is an essential part of scientific work. However, <u>in conjunction with</u> the deletion of descriptions of this process from scientists' descriptions of their work, scientific "facts" become reified and their production histories lost. Those histories are further obscured by the shorthand of formal professional modes of presenting results which are typically both brief and idiosyncratic within lines of work. Retrieving those histories, by observing the process of deletion, provides us with important data about the connection between work process and theories (Fleck, 1979; Latour and Woolgar, 1979).

The "radical program" in the sociology of science has begun to investigate some aspects of the "social construction" of scientific reality. This is a research program developed primarily by British sociologists. Much of this work (e.g. Latour and Woolgar's <u>Laboratory Life</u>) has focused on the "facticity" which occurs at the publication and analysis stages. For example, Latour and Woolgar developed a scale of measurement of reification of facts. This scale measured how scientists delete actions from written presentations ("deletion of modalities"). As others have noted (Collins, 1975; Barnes and Law, 1976), published scientific conclusions do not represent all details of the work scientists perform. Rather, results present partial or schematic maps of the original work. These maps emphasize simply theoretical or logical developments. They may form the basis for further research in other work sites, as other labs use them to help shape their ongoing work. But scientists also make the tacit assumption that scientific facts are fully detailed representations of the work, thus fully detailed maps. As many have noted (Dewey, 1929; Dewey et al., 1917; Stanley and Robbins, 1977; Garfinkel et al., 1981), the stylistic canons of scientific writing require that results be presented as _faits accomplis_ without mention of production or decision-making processes.

Another aspect of the deletion of work descriptions has been explored by Collins and Pinch (1982). They begin with the observation that there is no such thing as a fully-detailed rendition of scientific work. Variation and discretion are always present in the scientific work site, and complete replication is a chimera. Something differs in every experimental replication. Collins and Pinch examine the conditions under which scientists invoke this variability as a problem, or in which they apply more rigid rules for replication.

Mechanisms for examining the gap between work processes and work representations (publications) are informal and often unspecified. Thus, by manipulating the rules of replication, scientists can make legitimate and illegitimate designations about research. Collins and Pinch use the example of ESP research and its career in trying to become legitimate. Main line researchers have demanded that ESP experiments be replicated according to much more stringent standards than those upheld in "normal" science-making. When ESP researchers are unable to replicate, this is taken as disproving the existence of ESP. Wynne (1979) has documented a similar case from an historical perspective. Lynch (1982) has also discussed the idea of the lack of specification and the recreation of diagnostic categories in the clinical testing situation. In his research, the rules were manipulated according to the social status of the patients as well as the research needs of doctors.

This work in the sociology of science points to a wide, informal zone where bargaining takes place about how well-structured problems must be. This wide negotiation zone spans many stages of work, from research design to publication. There is ample room at any stage to lose sight of the process of transformation from ill-structured to well-structured problem. Law's work (1982) shows that the scope of this zone may be more than discipline-wide as actors black-box (that is, take what is apparent on the surface and trust that the rest is coherently there, ignoring content) the complexities of each others' results.

In this study, I examined published work for clues and evidence of simplified results, glossing over work processes, and black-boxing within and between lines of work. Like other interactionists studying science (Gerson, 1983a; in prep.; Volberg, 1983a; Strauss et al., in press), I also analyzed these processes at other stages of the work whenever possible.

The fitting process between theoretical commitments and the constraints on material resources can be examined at a number of organizational levels. Large-scale debates or historical change in scientific perspectives result in changed methods, technologies and problems. These large scale changes can also result in professional movements and changed institutional configurations (Busch, 1982). On a smaller organizational scale, individual laboratories or research programs can be studied to reveal how they "fit" theories with constraints. Similarly, scientists' representations of their work can be studied at many stages, from selection of problems to organization of project to final publication.

3. <u>Analytic Framework of the Study</u>

I employed a symbolic interactionist approach to the study of science. This means two things: a Pragmatist philosophical basis and a commitment to empirical studies of work.

Long before the radical program in the sociology of science began to examine science in a relativist light, Pragmatist philosophers had begun to do so. Peirce (1966), Mead (e.g.,1938), Dewey (1938), Dewey, et al. (1917), Bentley (1975a) and Dewey and Bentley (1949) were all concerned with delineating the relationship between scientific work, the representation of scientific process, and the development of "facts." They held that in science, as elsewhere, meaning is constructed from experience.

For Pragmatists, then, scientific theories are based on the constraints and problems posed by action and work. Theories about what something is are built by trying to use it to change something else. As Dewey (1938) describes the process of reification of "fact-making," scientists also regularly forget that meanings of theories arise in an ever-changing work context. As Mead (1917) notes, they freeze facts and attribute reality to them.

Symbolic interactionism grows from a tradition of empirical studies of work and conflict, as well as from Pragmatist philosophy (Blumer, 1969). One learns about people by observing what they do, including what they argue about and consider to be mistakes and difficulties (Fisher and Strauss, 1978; 1979).

The analysis employed here, like that of Becker (1961;1970;1982), Strauss (Strauss et al., 1964; 1978a; Strauss et al., in press) and Gerson (1983a; in prep.) is focused on work. Science is one kind of work. From a Pragmatist perspective, my analysis is also concerned with reification of that work: accounting for the obduracy of perspectives that workers use to solve problems.

My analytic premises are that: 1) one does not understand work simply by examining its products, nor 2) understand it _without_ reference to the products; and that 3) work involves joint effort over time, and thus is both interactive and processual; and 4) meaning does not inhere in the nature of scientific work, but is continuously renegotiated by workers and consumers (Hughes, 1971a). The units of analysis employed here are tasks and activities, not individuals or their allegiance to theories.

An additional set of analytic concerns which informs this study can be found in the _social worlds_ approach (Strauss, 1978b). This approach has its roots in the early Chicago school of sociology, especially in the concerns of Simmel and Park with group affiliations and their dynamics

(Simmel,1955; Park,1952). Later, Shibutani expanded this work (and included concepts from Mead and Dewey about group formation and inquiry) and put forth the idea of reference groups as perspectives which form the basis for action (1955; 1962). These are social groups with shared resources, constraints and work. Strauss (1959), Bucher and Strauss (1961), Bucher (1972), Becker (1982), Gerson (1976;1983) and Kling and Gerson (1977; 1978) have elaborated these ideas to describe reference groups with shared work. These groups continually change; they come together (intersect) and splinter apart as groups with separate kinds of work (segment). They argue with each other (debate), and occasionally form large-scale, stable debating structures, or arenas. The concepts of intersection and segmentation appear throughout the thesis.

3.1 Analytic Foci of the Study

Throughout the data collection and analysis, I focused on certain aspects of the work situation, including the following:

1. Tasks

During the data collection and analysis, I analyzed statements made by scientists or secondary commentators in terms of the tasks they referred to. These references were both implicit or explicit. For example, when a scientist claims that "my experiment proved _x_," I would decompose the phrase into the following sets of tasks:

a problem was posed
monkey bought
animal fed and housed
skills in surgery and chloroforming acquired and applied
incision made
electrodes applied
muscle twitches observed
twitches coded and written down
twitch codes organized and analyzed
results written up
hypotheses tried out
report submitted to professional society
report refereed and revised
revisions argued about, some included
report accepted for publication
report published

All these tasks are compressed into the phrase, "my experiment proved _x_." This is, even, a very short, incomplete list of the tasks involved in just one phrase. Scientific literature is full of such phrases. Nevertheless, the above list indicates the way I read such material, and the kinds of tasks with which I was concerned. At larger scales of organizational analysis, my lists of tasks included those which transpire across many lines of work, such as lobbying for a point of view, or ignoring a whole school of thought.

2. Anomalies and Mistakes

Throughout this study, I was concerned with the mistakes (as defined by themselves) that scientists made at work. I was also interested in how they handled anomalies in the course of constructing arguments and theories. For example, when the experimenter in the example above coded "muscle twitches" and wrote them down, how were ambiguous twitches coded? Were they screened out or included? If a completely unpredictable muscle twitched--one that could not be explained in terms of the extant theory--how was that coded? Did the experimenter screen it out or include it, use it to expand the theory? Does the ambiguous become a discovery or an exception which need not be included in the explanations of the phenomenon?

In attending to anomalies and mistakes, I focus especially on their management, that is, the work associated with defining, locating, and controlling them (Star and Gerson, in prep. a). I do not evaluate correct or incorrect science, medicine or theories. Scientists themselves make those evaluations. Because they do, and because their evaluations affect the outcome of research, they are an important source of data for the study of scientific work.

3. Merging Types of Work

Throughout the study, I attended to the ways in which scientists merge different types of work to produce a scientific theory. Localizationist physician/physiologists frequently merged clinical and basic research results to create a single, unified theory of localization of function in the brain. That merging, and its theoretical and practical consequences, is an important focus of the thesis, discussed as _triangulation biases_ in Chapter 3. This type of merging of realms of evidence was common practice for scientists developing localization theory; results from care of neurological patients, neurosurgical results, and case histories were combined with animal experiments and pathological data. In addition to combinations of clinical and basic evidence, other combinations I found included:

a) combinations of _mechanical_ and theoretical work, for example, the application of electrodes to the brain of a monkey, and the development of theories which would explain the consequences of the applications;

b) _political_ and _theoretical_ work combinations, for example, the work of lobbying for permission to do animal experimentation and the development of methodological rationales which included animal experiments.

c) combinations of _line of work_ and _arc of work_. Arc of work refers to the organization of activities within a single project. Line of work is a more abstract level of task organization, referring to the ongoing task organization of an occupation, discipline or profession (Strauss et al., forthcoming; Gerson, 1983b). In my data there was a constant interplay between activities addressed to the daily work (arc) organization of action and those addressed to the medical and scientific professions at large (line)--

for example, taking a single brain operation and making its procedures and results applicable for all surgeons.

4. Managing Uncertainty and Complexity

Scientists' situations are complex and uncertain. Scientific workers evolve strategies for getting the work done despite these uncertainties. The following strategies were important for the scientists I studied:

a) Simplification. Scientists simplify data or results by shortening the number of tasks (or ignoring some) it takes to reach an inference. They also simplify by choosing one result from among many and discarding others.

b) Substitution. Scientists sometimes exchange difficult tasks for simpler ones, or, instead of working on insoluble problems, substitute solvable ones. (Volberg, in prep., has developed this idea.)

c) Use of heuristics. Wimsatt (1980a) defines a heuristic as a procedure scientists apply to solve problems. It is discardable, evaluated in terms of its results, and produces systematic biases in results. I examine the kinds of heuristics adopted by localizationist researchers, and the biases which resulted.

4. Data Collection and Methods Used in this Study

4.1 Data Sources

The data about this scientific work were drawn from five sources:

1. The medical and scientific journals and books of the time, including _Brain_, the _Journal of Physiology_, the _Lancet_, and the _British Medical Journal_. These included case histories, letters, accounts of meetings and congresses, as well as theoretical articles debating localization theory and other aspects of brain research.

2. A selection of records from the National Hospital for Nervous Diseases (formerly the National Hospital for the Paralysed and Epileptic), London, and from University College, London, from 1870-1901. These records included accounts of brain and other nervous system surgery, treatments and observations of epileptics and other neurological patients, and accounts from nurses and ward attendants about their work.

3. Contemporary and modern secondary literature on the medical profession, the hospitals, and scientific and medical histories of neurology and neurophysiology.

4. Nineteen laboratory notebooks kept by David Ferrier, primarily detailing his localization experiments with animals. These experiments were designed to support localization theory and "map out" the motor and sensory cortex of various animals, especially monkey. The notebooks spanned from 1873-1884.

5. A field study at a modern neurophysiology laboratory. This was the pilot project for the study from which I drew many of my theoretical sampling questions. I

later asked these questions when analyzing written material. (See Star, 1983 for a report of this project.)

4.2 Methods

Dewey (1938:70) defines "quality" as that which pervades all elements and relations that are or can be instituted in discourse, and which thereby constitute an individual whole. Qualitative methods empirically specify the relationships which form a situation.

A situation is not just a geographical site or a moment in an individual biography. As Mead defines the term, it is an organization of perspectives which "stratifies nature":

> ...the existence of motion in the passage of events depends not upon what is taking place in an absolute space and time, but upon the relation of a consentient set to a percipient event. Such a relation stratifies nature. These stratifications are not only there in nature but they are the only forms of nature that are there (1964a:315).

Situations are the organization of perspectives. Mead's stance here is radically relativist: "...they are the only forms of nature that are there." Thus, qualitative research into science from a Pragmatist perspective means describing science-making situations, and not the attitudes or psychologies within a scientific work place (Gerson, 1983a).

I used grounded theory, and a qualitative analytic approach, to discover the situation described here. The details of the grounded theory method have been described elsewhere (Glaser and Strauss, 1967; Glaser, 1978; Strauss, in prep.). I especially relied on Strauss' (1970) exposition of "discovering new theory from previous theory."

Grounded theory has most frequently been used in conjunction with participant observation (see e.g. Fagerhaugh and Strauss, 1977; Crabtree and Glaser, 1969; Glaser, 1972; Glaser and Strauss, 1965; 1968). I found some technical modifications necessary to apply it to historical dta. However, the basic procedures of coding, memoing, constant comparison and integration remained essentially the same.

Coding historical material often requires "reading between the lines" to discover action, especially in the history of science and medicine. In participant observation, one sees people moving, arguing, coming and going. Reading scientific and medical history, by contrast, one is often confronted simply with a list of "great men" and a chronology of great discoveries--stripped bare of many of the actions taken for granted in the participant observation situation.

Thus I often found myself in a process of tracking down clues which would lead me to a reconstruction of the activities comprising a given event. For example, several secondary sources mention a particular brain operation as an important turning point for localization theory. In order to find out who did what in setting up and performing this operation, I read histories of the hospital where it took place, biographies of the surgeons and physicians, reports of the operation in the popular and medical press, and histories of surgery which would tell me the general conditions and techniques of surgery at that time.

This led to some mechanical difficulties in managing notes. At the beginning of the historical data collection, I was collecting bibliography notes, analytic notes, biographical and chronological notes. This was unwieldy. I then decided to treat the historical situation as I would a field observation situaton, and take "field notes" each day in the library. The format I eventually used was the following:

```
Ref: ______________________
     ______________________

analytic codes     |   narrative of events; my
                   |   observations as I wrote
                   |   them up; partial memos;
                   |   cross-references to other
                   |   notes
                   |
```

As the analytic codes were compiled, I wrote integrative memos or developed integrative diagrams on analytic themes, developed theoretical sampling questions, and began building a narrative and analytic framework around basic processes. The body of notes formed the data base for analysis.

4.3 Stages of Analysis

There were several stages in this investigation. First, I conducted a participant observation pilot study at a contemporary neurophysiology lab. Respondents at the lab were using EEG (electroencephalographic, or brain-wave) technology to infer relationships between the brain and behavior. The theoretical sampling questions which I derived from this study were:

1. What are the mechanisms of simplification which scientists use to reduce data? As discussed above, how do scientists reconcile the complexity of their findings with constraints and pressures to present simpler findings? How are ideas simplified or interpreted in simplified form?

2. How do institutional pressures encourage scientists to choose between kinds of theories? Initially respondents at the lab had held a somewhat diffusionist theoretical model of brain function. As government funding for scientific research was slashed, they began to look around for other sources of funding, including clinical funds which relied on application of a highly localized model of the brain. This raised the analytic issue of the

relationship between funding pressures, and other kinds of institutional pressures, and scientific theories.

3. What are the different ways anomalies can be managed in the scientific work place? I observed respondents manage anomalous data in a variety of ways: ignore artifacts, recheck anomalous data, screen subjects to minimize uncontrollable artifacts. Anomaly management was an important part of their work, a possibly critical link between work and results (see also Star and Gerson, forthcoming a and b).

4. What is the relationship between technology and theory development? I observed respondents on a daily basis doing routine work with various machines, obtaining measurements and recording them, calibrating the machines and occasionally substituting one kind of machine for another. Simultaneously, they discussed formal, very abstract theories about the nature of the brain and the mind. The connections between the choice of machine used to collect data and its ultimate interpretation were problematic for them. How were such connections handled by earlier neuroscientists? This became a concern for me.

After completing this pilot study, I designed a study which would compare modern neurophysiology research with that in the past. I hoped, by historical comparison, to answer some of these theoretical sampling questions. I read histories of medicine and neurology, as well as philosophy dealing with brain and mind relationships. The oscillation between localized and diffuse models of the brain became even more apparent as I read, and the importance of the late 19th century as a period of consolidation was clear. As I read more about this period, the complexity of institutional, professional, intellectual and technical concerns also became clear to me.

I decided to limit the scope of the thesis to this one historical period in order to explore these interplays. Comparison with modern neurophysiology is briefly discussed in the last chapter, and will be part of future work.

After extensive reading about the 1870-1900 period, and after developing a focus on the group at Queen Square, I went to London for several weeks to examine hospital and laboratory records from localizationist researchers. I also lived near Queen Square during this time. The hospital at Queen Square is still in operation. Walking around the hospital, whose wings and wards are named after the early localizationists, sitting in the surgical theatre, I was able to more vividly reconstruct the daily lives of those workers.

4.4 Limits of the Analysis Presented Here

What does this thesis *not* do? What are the limits of the data presented here? Several points are important in evaluating the argument I present:

- The thesis does not ask why this perspective was successful in some places and not in others. Todes' (1981) work on the development of diffusionist perspectives in Russian neurophysiology provides an important counterpoint to the case of Western Europe. Further work remains to be done across cases in different times and places. Todes' work was important to me in first suggesting that localizationism had not been adopted across the board at the end of the 19th century.
- The thesis does not attempt to link professional-level or work site-level data with national economies or other national level social structures. While I believe that the principles of laissez-faire and a capitalist approach to labor, extant in England <u>match</u> the metaphors used by localizationists in describing divisions of labor in the brain, I don't have sufficient data or comparative cases to talk about causality from one to the other.
- I was able to obtain very little data about the work done by patients, hospital support staff, or lab workers other than principle experimenters. From Strauss et al.'s (forthcoming) research on the work patients do, I believe that the work done by patients can be very important in creating the definition of the medical situation. And, coming from the

Chicago tradition of sociology, I regret that the extant data force me to focus solely on the doctors and physiologists at the top of their institutional hierarchies. They were the ones who left accessible records. Where possible, I have tried to flag the lacunae in my analysis where the input of patients, support staff and other workers seems logically important.

• I was unable to locate much information on the medical suppliers used by localizationists, on scientific instrument design, or on the sources of experimental animals. Thus, the possibly important impact of these lines of work on localizationism is unknown.

4.5 Narrative and Analytic Tension

Analyzing historical material from a sociological perspective presents a work management problem of its own. The continuity of the historical story must be maintained, and analytic concepts derived from this substance must be introduced in an organized fashion. Choosing a simply chronological organizing scheme solves one of these problems; choosing a logical analytic structure solves another. Combining both of them produces a narrative tension; the reader is asked to switch back and forth between the "story line" and the increasingly dense analytic concepts. There is no particularly graceful way out of this dilemma; I have tried to manage it here by flagging historical segments of the material under separate headings, and keeping the main organization of material analytic.

5. <u>The Localizationist Perspective</u>

The perspective which informed and legitimated localization theory was a set of approaches to:

- general methods of physiological investigation;
- the relationship between nervous anatomy (structure) and physiology (process);
- the mind-body relationship.

The elements of this perspective informed not only localization research, but research in many other lines of work as well. In localization research, these elements became the basic, often tacit assumptions of the theory:

1. <u>Anatomy carries mental functions</u> in the same way that electrical power lines carry electrical current;

2. The mind is contained in the <u>individual</u> nervous system, especially in the cerebral hemispheres;

3. The mind is <u>divisible</u> into a <u>hierarchically ordered</u> nervous system;

4. The mind/brain relationship can be investigated through a <u>logic of deletion</u>, that is, by removing or deleting parts of the anatomy that perform the function, and then observing changes in behavior.

The perspective described here was both production technology and product for localization theory. That is, it was a rationale and justification for the use of certain methods, the choice of questions and research directions (production technology). And, as the theory itself became a going concern, attracting clientele, personnel, and financial support, it legitimated the perspective, and imbued it with the property of being "natural" and "the way things are" (product). In this sense the theory/perspective relationship described here is dialectical: they cause each other and sustain each other. Together, they become a going concern which appears to be inevitable.

One striking property of the localizationist perspective is its obduracy. Despite conflicting evidence and many failures associated with the theory, the perspective has remained in wide use for decades. The conditions which caused this are complex. They include daily work routines, professionalization processes, the institutional structures of medicine and physiology in the 19th century, and the available technology (including arguments) for scientific investigation.

5.1 The General Situation of Localizationist Work

I studied the work of a group of British physician/physiologists and surgeons who provided much of the clinical and physiological data used to develop localization theory. This group was primarily based at the National

Hospital, Queen Square, London. It included David Ferrier Victor Horsley, John Hughlings Jackson, and William Gowers. [2]

Between 1840 and 1860 (after phrenology had become unfashionable in medical circles) dominant theories of brain function were diffusionist. The influential work of the French physiologist Flourens had demonstrated that the cerebral hemispheres did not have areas associated with particular functions. The hemispheres were believed to be insensible to stimulation and to operate as indivisible entities (see e.g., Walker, 1957). At the same time, however, reflex physiologists since the early part of the century had been seeking and finding various reflex "pathways" through the nervous system (Liddell, 1960; Fearing, 1930). A research tradition which sought anatomical specificty had begun which would later influence localizationists (Olmstead, 1944).

During the 1860s, a strong interest in localization of function in the cerebral hemispheres began to develop in two different institutional sites. Broca, in France, demonstrated his famous "speech area" in 1861. One of his aphasic (speechless) patients died, and autopsy showed a softening of the brain in the third frontal convolution (Schiller, 1979). Broca widely publicized this finding and claimed broad implications for the brain as a whole:

"there are in the brain great distinct areas corresponding to the great areas of the mind" (quoted in Riese, 1977:68).

This idea became a mandate for subsequent researchers. Meanwhile, in England, another researcher, John Hughlings Jackson, was working with epileptic patients and attempting to account for the bewildering array of symptoms they exhibited. Jackson was heavily influenced by Herbert Spencer, and attempted to apply Spencer's theories of evolution and dissolution of society directly to the nervous system (see e.g., Jackson, 1932b; 1932c). He focussed on the progression of symptoms in epilepsy. He tried to associate the sequence of epileptic spasms with specific parts of the brain in a complex theoretical model.

In 1870, a turning point in the trajectory of this research occurred. First, two German investigators, Fritsch and Hitzig (1950) discovered that electrical stimulation applied to various areas of an animal's cortex (hemispheres) would produce <u>specific</u> muscle reactions. The implications of this experiment were immediately seized on by medical experimenters in England. Efforts to tie together reflex physiology, clinical work with epileptics and aphasics, and electrical stimulation experiments really took off at this point (Spillane, 1981). Among those who contributed significantly to these efforts was David Ferrier, later a colleague of Jackson's at Queen Square. He began his physiological experiments with localization in the early 1870s (Ferrier, 1873).

While research in Germany, France, Italy and the United States was also proceeding along these lines, it was in England that the theory was forged in definitive form.

5.2 Finding Sources of Evidence for Localization

Researchers culled evidence for localization theory from a number of sources. The daily and institutional contexts for this work are discussed in Chapters 2 and 3. Here I present an overview of the types of evidence they collected and the logic used to justify them.

Autopsies

While performing postmortems was not routine by the 1870s, physicians and surgeons had for the most part become familiar with dissection techniques. Broca's original evidence, as noted above, came from his postmortem examination of the brain of a patient who could not speak. When he found an abnormality in the third frontal convolution of the brain, he declared that this was evidence for the existence of a speech center in the brain.

This strategy had appeared earlier in physiological work as vitalists searched for control centers in various parts of the brain. For example, Flourens linked breathing with a spot in the lower brain he called the "noeud vital." He proved his point by deleting various parts of pigeons' brains and observing the effects on respiration (Olmstead, 1944). Investigation into the location of this noeud vital, as well as into possible alternative centers for respiration, proceeded well into the 1870s (Kellogg,1981).

The experimental strategy of deleting parts of the nervous system and observing the consequences was adopted by English localizationists. The logic was used both on experimental animals and in observing what scientists called "natural experiments" made by disease in humans (Jackson, 1873a), where parts of the brain were "deleted" by pathological processes. The medical journals of the 1870s are filled with detailed examples of postmortem work in which researchers try to link softening, discoloration, or erosion of areas of the brain or nervous system with loss or disturbance of functions. In the early years, there are many reports of failures to find such correlations (see e.g., Ross, 1882b; Althaus, 1880; Hobson, 1881; Duncan, 1879; Ferrier, 1881; Gowers, 1878b). These failures provided important ammunition for the antilocalizationist attacks discussed in Chapter 4.

Researchers also used autopsies to try to find nerve pathways from the spinal cord to the brain. They tried to link these paths with localization of functions. Doctors often did autopsies on deceased patients from mental asylums for the poor. Several prominent localizationists (including Crichton Browne, Bevan Lewis, David Ferrier and

Hughlings Jackson) spent years working in the postmortem rooms and laboratories of "lunatic asylums" (Spillane, 1981). This had several advantages from the point of view of the physicians. Since impoverished lunatics did not have a very high "social value" (Glaser and Strauss, 1964) and were often abandoned by their relatives, objections to postmortems were more rare than with other groups of patients. The lunatic asylum setting also provided a large enough supply of cadavers to begin to make comparative studies. Many of those incarcerated had had syphilis, speech disorders, paralysis, or epilepsy. Therefore, for the purposes of linking functional disorders with areas of the brain, the brains of the insane provided an important source of material for examination.

The doctors who administered lunatic asylums during this period had a great deal of discretion. There were few standard forms of treatment, and there was even a bit of extra money for research at some of them (Viets, 1938). In an era when British physiology was completely unfunded, even at the university level, the lunatic asylums occasionally provided a place to do basic research. While funding was not lavish, at least the equipment and subjects to do experiments were available. The West Riding Lunatic Asylum was such a place. It was administered by James Crichton Browne, who later became a founder of the localizationist journal Brain. The other founding editors were David Ferrier, John Hughlings Jackson, and John C. Bucknill. Browne made it possible for Ferrier and Jackson to do research at West Riding (Spillane, 1981:389;Ferrier, 1873; Jackson, 1873a; 1873c;1876).

One goal of many of the autopsies that researchers did at the lunatic asylums was to prove that lunatic brains weighed less than other brains. While this is tangential to our discussion of localization, it did provide a kind of legitimacy for the autopsy work that was done at places like West Riding. This line of work continued through the turn of the century. It was often done by localizationists (Crichton-Browne, 1879; 1880). Although this line of work did not explicitly support localizationist theory, it helped legitimate the search for physical bases of mental disorders (especially in the brain and nervous system) (Gould, 1981).

Locating and Producing Lesions

Another kind of work used the same logic as that employed in the autopsy: the location and production of lesions. Lesions are injuries to tissue. *In vivo* brain lesions can be caused by blows to the head, bullet wounds, or diseases which erode tissue, such as syphilis or tuberculosis. *In vitro* lesions produced on animals can be created by surgical removal of part of the brain, electrical shock which damages tissue in certain areas, or the injection of substances which can destroy tissue.

Localizationists used _in vivo_ lesions as examples for localizationist theory when the lesions interfered with functions (see e.g.,Ferrier, 1882; Mills, 1879; Shapter, 1880; Fraser, 1881; Gowers, 1878a). For example, if a patient had a wound on the left side of their head near Broca's "speech area" and was also aphasic (speechless), then this was used as further evidence for the existence of a speech area where Broca had first pinpointed it. Between 1860 and 1910 the journals are full of cases of patients with partial paralysis after concussions or skull damage to various areas of the head. These cases were an important source of evidence for localizationists .

Researchers used lesions produced in the laboratory to adduce evidence for the theory in a similar fashion. For example, they would open a monkey's skull surgically and remove a part of its brain. If the monkey then developed paralysis on one side of its body, researchers used this as evidence that the piece of the brain removed had been originally responsible for movement on that side of the body. [3]

The anti-vivsection movement had an important influence on the production of lesions in the laboratory. This is discussed in detail in Chapter 2. The antivivisectionists were opposed to laboratory experiments involving animals. They were very powerful in Victorian England (French, 1975). Because of the antivivisectionists, and because of the lack of British funding for physiological research, British physicians who wanted to do physiology either had to go abroad for training (to Germany or France, which many of them did), or apprentice with someone already trained. This made the acquisition of skills somewhat idiosyncratic, and there was little agreement on standard equipment or laboratory procedures. For example, there was no agreement about whether to use faradic or galvanic current; whether to ablate animal brains with a scalpel or with jets of water; how much tissue to take out and how to account for complications in the surgical procedures(see e.g.,Dodds, 1877-78).

Electrical Stimulation

As mentioned above, the electrical activity of the brain was first discussed in detail in a report published in 1870 by two Germans, Fritsch and Hitzig (1870). They applied electrodes to the exposed brain of a monkey and observed movement on a galvanometer. The use of electricity in medicine was common during this period. Thus, the methods used by Fritsch and Hitzig were easily adapted by doctors doing research (Schiller, 1982). Fritsch and Hitzig's discovery was used to launch a new kind of work in England, which meant applying electricity to parts of the cortex and observing muscle movements. Researchers then said that the movements were caused by the part of the brain stimulated.

Electrical work on the brain became fairly detailed very rapidly. Ferrier applied electrodes to areas as small as 1/16", and made maps of the brains of various animals based on this precise measurement. (The contemporary form of electroencephalogram ("brain wave") technology was not developed until about 1910, see Brazier, 1961).

Surgery

Surgery on the brain and nervous system began to be developed in the 1870s, but techniques were extremely crude. Antisepsis had not become universal, and even where it was used, it was often applied unevenly (see e.g., Godlee, 1917). Patients almost always died of infection after these operations, even if they did not die of shock or hemorrhage on the operating table.

The earliest brain surgery was on skull wounds or abscesses, not on tumors. If someone had a head wound, a surgeon would occasionally be able to remove bone chips or drain off fluid without killing the patient, especially if the dura mater, the membrane surrounding the brain, was not punctured. These early operations were sometimes used to support localizationist theories. For example, a patient with a concussion on the left side who suffered from right side paralysis was cured when a piece of bone was removed from the broken skull. This was seen as evidence for localization of function (MacCormac, 1881).

In 1879, a surgeon performed the first operation using localization theory to locate a tumor in the spine. Victor Horsley, a surgeon, did the operation, with William Gowers, a neurologist at Queen Square, providing the estimate of where the tumor was located. In 1884, the first brain tumor operation using localization theory/neuorlogical indicators was performed in London by Rickman Godlee (Bennett and Godlee, 1884). Godlee was Joseph Lister's nephew, and was chosen, in part, to do the operation because it was believed by workers at Queen Square that he would follow the principles of antisepsis. Hughes Bennett, a physician at Queen Square, provided the neurological information for Godlee's operation. Both of these operations received wide publicity, and were immediately hailed by the medical worlds as turning points. They were also used to justify vivisection experiments (training for surgeons) and to legitimate localization of function (Spillane, 1981).

The relationship between mapping the brain and locating brain tumors is a complex one. While Godlee's operation was widely hailed as proof that localization theory worked, patients who entered the National Hospital for the Paralysed and Epileptic (Queen Square) with brain tumors were not used to verify localization on a routine basis

(National Hospital Records, 1870-1901). Exemplary cases where localization criteria and location of tumor matched were cited in the literature. But these were the exception, not the rule. Nevertheless, claims for routine diagnosis and location of brain tumors were made by localizationists. They helped to buttress the credibility of localization theory in a significant way.

Pathology and Histology

Microscopic evidence from pathology was used in an attempt to demarcate functional areas of the brain (see e.g., Campbell,1905). Locating tumors, distinguishing tumor tissue from healthy tissue, and classifying tumors according to their effect in the brain was an important additional source of evidence for localization of function (Lewis, 1878a; 1878b). By the end of the period examined here, new lines of evidence were also developing from histology to support localization. The distribution of different kinds of cells in the brain was correlated with different functions ascribed to those areas (see Campbell, 1905). Since much of this latter work took place after 1910, we will not give it much attention in this thesis.

6. Summary

The thesis is divided into several chapters, each of which explicates a part of the problem of development of perspective. Chapter 1 gives an analytic and formal definition of perspective and details the mechanisms by which the perspective developed. Chapter 2 describes the institutional and professional contexts of localization work, including professionalization processes and their relationship to the theory. Chapter 3 explores the effects of uncertainty and triangulation of evidence in making the localizationist argument. Chapter 4 describes the debate about localization, and the positions of antilocalizationist researchers. Chapter 5 describes the types of heuristics which became standard in localizationist research, and the effects of triangulation and debate on the problem of the mind/brain relationship, or parallelism. Chapter 6 explores the effects of the obduracy of the perspective.

By 1906, when Sherrington's _The Integrative Action of the Nervous System_ appeared, the perspective was virtually inviolable. Although Sherrington's work challenged localizationist theories in many ways, it upheld the _perspective_ that had informed it (Walshe, 1957). The work was dedicated to Ferrier, and, while it depicted the cerebral cortex as a labile, nonlocalized organ, it also applied the logic of deletion, a particulate approach to functions, and an individualist, parallelist approach to the nervous system (Riese, 1959). The impact of Sherrington's work was to simply organize anomalies which had appeared in localizationist theories into a framework which was entirely compatible with the research enterprise that had developed at the end of the century. Thus, the perspective at its most obdurate became plastic in the sense of accounting for a large number of anomalies as well as a modified diffusionist point of view.

The following chapter defines the formal properties of a perspective, and elucidates the mechanisms by which they become obdurate or robust.

NOTES

1. Tizard (1959:132) says that:

> A history of theories of localization of function in the brain can be found more or less briefly outlined in Boring and Lashley. Both writers point out that all such theories can be broadly divided into two types--localization theories, which hold that specific functions are controlled by specific parts of the brain, and field theories, which hold that the brain acts as a single functional unit. It is said that, historically, a swing of the pendulum tends to occur between these two positions. At one period the majority of informed opinion holds a localization theory, but a generation later this tends to be considered distinctly unorthodox.

2. Biographical dates are given in Appendix C.

3. In most cases, there was a "crossover" between the side of the brain ablated and the functional disturbance. For example, a left-side lesion would produce a right-side paralysis. This right-left crossover was recognized at the beginning of this period, although the contemporary "right-left" brain implications were not much explored.

CHAPTER ONE

ANALYTIC CONCERNS AND PERSPECTIVES

This chapter gives an analytic overview of the argument in the thesis and a formal definition of "perspective." Several aspects of a successful perspective are discussed. The chapter concludes with a discussion of the implications of perspective as a unit of analysis.

1. Introduction

The central problem of this research project has been to analyze the relationship between scientific theories and scientific work. There are two aspects of this problem which are elaborated in the thesis:

1. How do scientific perspectives develop from institutional and daily work conditions?

2. What mechanisms are involved in a perspective's success?

In order to make clear the ways in which this thesis answers those questions, perspective and its related terms are defined in this chapter.

2. Definitions and Terms

2.1. Perspective

Becker et al. (1961: 36) defined perspectives as "modes of thought and action developed by a group which faces the same problematic situation. They are the customary ways members of the group think about such situations and act in them." Four dimensions of perspective expand Becker's definition: standpoints, heuristics, political commitment rules and collectivities.

2.1.1 Standpoint

Perspectives contain standpoints from which natural events are stratified in terms of significance, connections and boundaries. This is what Mead (1964) called a "percipient event." Actors are located, temporally and spatially, in relation to a set of events. This location forms hierarchies of focus, significance, and horizons.

In more concrete terms, the scientific standpoint which I am examining here included questions such as: "What shall we study?", "Where, in a broad sense, shall we look for the phenomena of interest?","What are these phenomena like, and why are they important?", "With what can we and can we not compare these phenomena?"

Localizationists were interested in studying brain function within the individual; they chose to look for that phenomenon by looking at its breakdowns. They believed that brain functions were like many other natural phenomena: ordered, hierarchical, particulate in composition, well-articulated with the rest of nature. They also believed that brain function could and should be compared with other kinds of natural phenomena.

2.1.2 Heuristics

An important part of a perspective is a set of <u>heuristics</u> by which resources are allocated, including attention to problems and selection of methods for solutions. A heuristic is a rule or an argument for organizing information. It is discardable.[1] Heuristics are evaluated in terms of the results of their applications, not by inherent logical characteristics. They also have systematic biases, as Wimsatt has discussed in some detail (1980a; 1980b).

Some heuristics used by localizationists were :

- look for physiological damage where malfunction exists;
- look for anatomical differences between areas responsible for different functions;
- look first for organic causes of malfunction--if those cannot be found, only then look for social or psychological causes.

2.1.3 <u>Political Commitment Rules</u>

Perspectives have <u>political commitment rules</u>. These are patterns of constraints and resources (Becker, 1960; Gerson, 1976) which limit and support selection of problems and direction and level of effort (Becker et al., 1961). These patterns also shape where and to whom results will be marketed. Relationships with sponsors and institutional configurations are included in the phrase "political commitment rules."

In terms of the scientific work I am examining in this thesis, such rules consisted of a commitment to results with clinical value and with a medical orientation; results that would be acceptable to evolutionary biologists and vivisectionist physiologists; and problems that fit with an individualist approach to disease. Further rules of this nature were a pro-anatomical bias, deriving from the position of English physiology in the medical and surgical worlds (Geison, 1972;1978); and a need for results to be scientifically "respectable," which meant experimentally derived or legitimated.

2.1.4 Collectivity

4. Collectivity. Perspectives are collective, and arise from the shared tasks of social worlds (Strauss, 1978b;1959). An individual scientist may try out a given strategy on a problem, and have an informing idea or heuristic behind this strategy. I am using perspective here to refer to the collectivity of strategies developed from the shared work of a community, but not to such individual strategies. In this sense I refer quite concretely to shared daily work of doctors and doctor/physiologists who developed localization theory, as well as the less well-recorded work of nurses, patients and attendants. I also include the work of critics, philosophers and reporters who

helped shape the collective perspective in that they were integral to the localizationist social world.

Perspectives are collective both by virtue of being aggregate and by virtue of being resistant to the attempts of any single individual to overthrow or encompass them.

2.2. Facts and Theories

Facts result from the interaction of constraints posed by commitments to perspectives and interruptions to conduct (work) (Dewey, 1938). Facts have the following characteristics:

a. They are subject to rational rules of proof and disproof.

b. They may be subject to renegotiation.

c. They may be completely specified and local. Note that perspectives are neither of those.

d. They are also less robust than perspectives, in the sense that the proof or disproof of an individual fact does not fundamentally disturb the status of a perspective.

Theories are systems for sorting facts, including keeping some and jettisoning others. As this thesis shows, such systems are not simply logical rules extant in textbooks, but rather forms of collective action. Successful theories become going concerns, or ongoing systems which have personnel and clientele.

2.3. Robustness and Obduracy

Perspectives have the qualities of robustness and obduracy in varying degrees. Robust truths are defined by Wimsatt (1981) and Levins (1966) as "the intersection of independent lies." That is, robust findings are collections of statements which, taken singly, may not hold up as valid or reliable, but which collectively describe the world well enough for a number of purposes. Robust findings or approaches are not affected by local alterations or performance failures.

Mead's "pragmatic definition of truth" (1964b) also emphasizes the collective and collaborative aspect of robustness, and frames this in terms of jointly-undertaken action. He sees truth, pragmatically defined, as the ability for collective action to take place while encompassing multiple meanings.

> The test of truth which I have presented is the ongoing of conduct, which has been stopped by a conflict of meanings--and in meanings I refer to responses or conduct which the characters of things lead up to. The truth is not the achievement of the solution, still less the gratification of him who has achieved it...One is generally gratified by the solution of one's problem, but the test is the ability to act where action was formerly estopped (p. 328).

From a sociological point of view, this robustness may be described as theories or findings which are maintained by multiple commitments and standing negotiations (Becker, 1960; Gerson, 1976; Strauss, 1978a). Standing negotiations are those long-term arrangements which need not be questioned or renewed each time activity is undertaken.

<u>Obduracy</u>, like robustness, defines findings or approaches which are not subject to local alterations. Like the traditional use of the word, it means hard to move, stubborn, persistent. But obduracy is distinguished here from robustness by the procedures used to construct it. Perspectives become obdurate when their component parts are <u>privately validated</u>, or put together in packages whose contents are not accessible to investigation.

For example, when professions reserve evaluation of their work to members only, they make those results obdurate as opposed to robust (see e.g., Freidson, 1970a and 1970b; Bosk, 1979; Bucher and Stelling, 1977). When construction processes are black-boxed (by producers or consumers), or when results are tacitly constructed, obduracy also arises.

The distinction between obduracy and robustness is an analytic one. Perspectives which are purely robust or purely obdurate probably do not exist. Rather, all perspectives have components of both. Purely obdurate perspectives would be constantly subjected to challenges from researchers who found anomalous data; purely robust perspectives would acquire constituencies who would black-box or privatize components and make them obdurate. Perspectives may be successful either because of obduracy or robustness, or both.

This study emphasizes the interaction between obduracy and robustness in the construction of the localizationist perspective. Specifically, robust findings became embedded in obdurate commitment structures. Various obdurate political commitments directed research efforts toward making some findings robust and toward preventing the production of others. Yet by the criteria offered in this section, this is on balance more a study of an obdurate perspective than a robust one.

2.4. Contrast between "perspective" and "paradigm"

The definitions offered above of "perspective" are similar in many ways to the definition of "paradigm" developed by Thomas Kuhn in The Structure of Scientific Revolutions (1970). Since the publication of that book, there have been numerous exegeses on the definition of paradigm, Kuhn's ambiguous use of the term, and qualifications and limitations of his analysis (see e.g., Lakatos and Musgrave, 1970). It would be pointless to reiterate that debate here. Yet I have deliberately avoided the use of the term paradigm and that needs a note of explanation.

Perspective, as I use it here, is similar to paradigm in that:

1) It cannot be directly replicated or subjected to empirical test, and therefore there is a certain

circularity in its definition by proponents.

2) It is often tacitly established.

3) A perspective can only be replaced by another perspective, not by simple invalidation from facts extant in the "real world."

4) A perspective is adopted because it solves practical problems in a field or discipline.

A perspective is linked with scientific <u>work</u> and so differs sharply from a Kuhnian paradigm in that:

1) It grows from a daily work situation and is inextricably tied to that situation.

2) It grows from and within an institutional structure and is inextricably tied to that situation.

3) Scientists do not work out the ramifications of a perspective simply because of the intrinsic pleasure of puzzle-solving (as Kuhn says that scientists solve puzzles within paradigms). Scientists must sell their work to academic or commercial markets, clients and audiences.

4) No work is pre-perspectival in the sense that Kuhn says some scientific work is pre-paradigmatic. Rather, perspectives may become more well-articulated over time or more obdurate in nature, but they always change (dialectically) from existing work perspectives, however inchoate.

5) Responses to anomalies from participants within a perspective may be widely varying, yet participants may be allied in the development of the perspective and further

its spread and expand its scale.

6) Scientists become convinced of the validity of perspectives because of a set of shared work conditions and the allocation of resources within institutions, not, as Kuhn sees it, by the promise and conversion of a few individuals.

7) Scientific communities do not develop perspectives as isolated or special cases from the "rest of society"; rather, science is subject to the same political conditions as all work and is not separable from them.

Finally, I have chosen not to use the term paradigm, but rather the term perspective as a way of acknowledging the contributions of Pragmatist philosophers. Their work predates Kuhn by 100 years, although it is not noted by him. The works of Peirce, Mead, Dewey, Bentley and James all describe and define the collective actions which form, and often freeze, perspectives. [2]

Mead's (1964a) definition of perspective begins with the idea of a series of <u>events</u>:

> The consentient set is determined by its relation to a percipient event or organism. The percipient event establishes a lasting character of here and there, of now and then, and is itself an enduring pattern. The pattern repeats itself in the passage of events. These recurrent patterns are grasped together or prehended into a unity, which must have as great a temporal spread as the organism requires to be what it is, whether this period is found in the revolutions of the electron in an iron atom or in the specious present of a human being...constituting thus

> slabs of nature, and differentiating space from time. This perspective of the organism is then there in nature (p. 307).

So perspectives are not only ways to "approach" a nature that is already there; rather, it is the <u>intersection of perspectives</u> which stratifies nature and makes it meaningful. A scientific perspective is one kind of this intersectional work.

3. <u>Characteristics of Successful Perspectives</u>

The next pages outline five characteristics of successful perspectives: inertia, momentum, incompleteness at any one point, cumulative reification, and progressive inseparability of parts.

3.1. <u>Inertia</u>

Inertia is a term I borrowed from physics: a "body in motion stays in motion unless acted upon by some outside force." Established, successful perspectives remain as ways of working until actively opposed or abandoned in favor of another perspective.

The reasons for this are not some self-propelling qualities of ideas, but rather consequences of multiple social acts:

a) Commitments to training programs, technologies and vocabularies develop within lines of work. Such multiple and overlapping "side bets" (Becker, 1960) are hard to disentangle or dismantle;

b) Intersection and segmentation processes (Strauss, 1978b) make comprehensive revisions impossible. As increasing numbers of researchers develop the ramifications of perspectives and adopt them in different kinds of work sites, the perspective becomes complexly rooted and thus entrenched;

c) The payoff for abandoning the perspective must be higher than the payoff for keeping it. There is an asymmetry involved here, since future payoffs are uncertain, and what one has in the present, while it may not be perfect, at least is known and tried. Thus, overthrow of a perspective must be based upon perceived and socially acknowledged contradictions and disadvantages of the present perspective, and must involve organized revolt by those not invested in the perpetuation of the current perspective.

3.2. Momentum

Perspectives do not develop in single sites, but diffusely. In conjunction with inertia, there is a tendency for change to occur across many sites, often rapidly. I call this tendency <u>momentum</u>. Momentum, like inertia, is not rooted in some intrinsic quality of the ideas at stake, but rather in the social organization of work. The reasons for momentum include:

a) Results across sites are simplified by workers (Star, 1983). When reports of results are made, anomalies, qualifications and difficulties are often jettisoned. (Latour and Woolgar, 1979 discuss this as well.)

b) Anomalies or difficulties are often perceived as more local than the impact of potential results. This means that there is an asymmetry between perceived problems with a potentially high-payoff solution and perceived advantages of the solution. A problem with achieving clear results is often seen as local to a laboratory; potential payoff for solution from that same laboratory would be seen as impacting the entire line of work.

c) Hierarchies of credibility (Becker, 1967) form rapidly, and good results from those at the top of one line of work are picked up and used as valid by others in other lines of work. Thus, when Nobel prize-winning physicists want to comment on developments in neurobiology, their word is taken more readily than a junior researcher in neurobiology--and the perspective represented by the physicist gathers momentum across work sites.

d) The learning curve holds in science as elsewhere. Thus, by the time difficulties and problems with a perspective emerge, inertia has already set in and hierarchies of credibility are formed. There is a "honeymoon" period in which a perspective may become successful before flaws in it are taken seriously.

3.3. Incompleteness at any One Point

Perspectives do not appear in entirety in any one site or situation. While some parts of the perspective will often be explicitly discussed in one site, a full elaboration of it can only be found on an aggregate level. Simplification and substitution processes (Volberg, in prep.) --and the other component processes of obduracy--preclude full description from any single standpoint within the perspective.

Perspectives include tacit dimensions, black boxes, reductionist heuristics (Wimsatt, 1980a) as well as assumptions like "that's only nature" or "of course, that's just the way the world is put together" (Garfinkel, 1967). All of these conditions help preclude comprehensive description. Temporal factors are also important in incompleteness. Perspectives are constantly in motion, often very rapid motion. Thus, simply keeping up with developments as perspectives are forming is usually impossible. No one has an "overview" because events are happening too rapidly--one can't stop the world to describe it in its entirety.

Pluralism of viewpoints also makes perspectives incomplete at any point. All participants in a perspective have different (albeit often only slightly different) views of what is happening. Recall Levins' definition of robustness as "the intersection of independent lies" (1966). The aggregate view that emerges from a perspective cannot be robustly represented by any individual viewpoint.

3.4. <u>Cumulative Reification</u>

As results are generated and made robust and/or obdurate by multiplying commitments (especially institutional, technological and sentimental), the <u>sources</u> of these abstract results in work processes are forgotten. The abstractions generated within perspectives are concretized, and the facts made nonproblematic (Dewey, 1920). As Mead described in his 1917 essay "Scientific Method and the Individual Thinker":

> Whenever we reduce the objects of scientific investigation to facts and undertake to record them as such, they become events, happenings, whose hard factual character lies in the circumstance that they have taken place, and this quite independently of any explanation of their taking place. When they are explained they have ceased to be facts and have become instances of a law, that is, Aristotelian individuals, embodied theories, and their actuality as events is lost in the necessity of their occurrence as expressions of the law; with this change their particularity as events or happenings disappears (pp. 197-98).

Wimsatt (1983) discusses another organizational source for cumulative reification. This is the occurrence of what he calls "frozen accidents." These are events which happen early in the development of a perspective/collective endeavor. These precipitate institutionalized ways of working or standard operating procedures. Like their analogue in embryological development, these events ramify throughout the system, becoming entwined in all its various aspects. Thus, changing or eradicating their effects at points later in the developmental trajectory is nearly impossible, if only because the expense of doing so is so much more than living with the effects of the frozen accident.

A similar situation was described by P. Becker (1983) in examining computer systems used by various firms. Many companies have computer systems which are unwieldy and outdated. If they could institute a new system from scratch, it would be more efficient.

However, the old systems grew up gradually, and many standardized ways of working around them have been built into the company. All of the company's data and the training of its personnel is invested in them. To switch to a new system would involve complete retraining, stopping production, and re-entering data. Many of these systems contain multiple "frozen accidents," in the sense that temporary, unofficial workarounds became integral parts of daily work routines. It would be too expensive to replace them since every aspect of the company's business is somehow involved (and involved differently) in the current system. While scientists (and the management of the companies Becker describes) well realize that they are living with imperfect, often wrongly reified results, they cannot afford to change them.

3.5. Progressive Inseparability of Parts of the Perspective

Perspectives are composed of many parts, all having to do with the actual work of scientists: theories, facts, approaches, strategies, technologies, and standard ways of working instituted in many sites. The component parts of a perspective become increasingly inseparable as it develops over time. Things which were not logically or practically associated at the beginning of the perspective's development become seen by participants as "necessarily connected" (Gerson, in prep.). For example, at the beginning of the localizationist perspective there was little or no necessary connection between vivisectionist practices and localizationist brain research. By the end of the period, vivisectionist methods were inextricably linked with this type of research; Sharpey-Schafer (1927) notes that it would have been impossible for an antivivisectionist approach to be applied to the questions involved (see discussion in Chapter 2, below).

4. Development of a Successful Perspective

This thesis presents a "worked example" of the development of a successful scientific perspective and its relationship to the theories it legitimated. The following aspects of the perspective's development are analyzed in four chapters:

Chapter Two: Contexts of Localization Work

The mechanisms of professional autonomy and segmentation help to establish localization of function as an important commodity for doctors in becoming professionalized and specialized. Grounds for evaluation are reserved to the medical profession; professionalization both legitimates and is legitimated by localizationist findings (Strauss, 1983). The mechanisms examined here contribute to obduracy through professional autonomy and specialization. They contribute to robustness in the sense of providing useful commodities for working professionals in different spheres.

Chapter Three: Uncertainty and Triangulation

Evidence is brought from four lines of work which have different standards of proof and different conventions and goals to prove localization theory. Obduracy is increased as conventions obscure biases and as artifacts are shuffled from realm to realm. Robustness is increased as findings are used in multiple realms and refined there.

Chapter Four: Debating Tactics

Methods of arguing establish taxonomies and help redefine anomalies. Certain questions become salient, others are derided. Debating tactics confound grounds for evaluating obduracy and robustness, as ad hominem tactics, demonstrations, cross-legitimation, and various forms of diplomacy are comingled by those on different sides of the debate.

Chapter Five: Coincident Boundaries and Parallelism

The philosophical problem of parallelism arises as boundaries from different work realms are assumed to coincide. Material and immaterial evidence is assumed to be coincident. Perspective becomes more obdurate as this mixture of material and immaterial, concrete and abstract categories becomes a tacit part of work. It becomes more robust in physically-oriented lines of work as work proceeds and technologies develop.

Each of the four chapters described here illustrates mechanisms by which the perspective became both more obdurate and robust. A number of different factors entered into the success--obduracy and robustness--of the localizationist perspective. The processes outlined here were also interactive; that is, inertia in one sphere contributed to inertia in another; momentum in one to reification in another, and so forth. Each chapter concludes with a summary of the mechanisms and conditions of inertia, momentum, incompleteness, reification and inseparability.

4.2 Development and Change in the Localizationist Perspective: An Overview

The localizationist perspective changed in several respects as it developed from 1870 to 1906. The four defining characteristics of perspective, mentioned at the beginning of this chapter, became more tightly tied together through the course of the development.

In 1870, localizationist researchers were in the following position:

1. They had an organizing schema of the brain and nervous system, but little data to verify it. This was true in two respects: they had little data to verify the overall view of the brain as divided up into areas and little data to then assign areas on the basis of function.

2. They had no clinical proof that localization theory would be helpful for treating patients.

3. They were working, clinically, with an extremely unclear nosology (classification of diseases), and a poorly-developed set of diagnostic tools.

4. Links to physiology from clinical medicine were mostly speculative.

5. Physiology was a hobby for physicians whose value was not universally acknowledged.

By 1906, many of these conditions had changed dramatically. Over the 36 years examined here, the main direction of effort was overwhelmingly addressed not to the problem of <u>whether</u> the brain could be divided into areas, but rather <u>what</u> areas could be located where. By concentrating on which areas were located where, researchers often sidestepped the question of whether there were areas at all.

They also achieved the following results in comparison with their earlier situation:

1. They linked localization theory with several clinical treatments, including that for epilepsy and, especially, brain surgery.

2. They clarified nosological and diagnostic uncertainties on the basis of localization theory.

3. They made alliances with professional physiologists (such as E.A. Schafer and Michael Foster), and evolutionary biologists (including Charles Darwin and T.H. Huxley).

4. They explicitly linked their programs of physiological research with the clinical phenomena being addressed by localizationist clinical medicine.

As this thesis illustrates, localizationists were able to construct a perspective and theory in which these elements were strongly linked. The picture of the brain and nervous system which emerged at the end of the period was that:

-the nervous system was constructed in an <u>evolutionary</u> hierarchy which could be investigated through disease or experiment (see e.g., Brunton, 1882).

-diseases <u>are experiments</u> made by nature (Jackson, 1873a), and if seen that way can provide basic information about the composition of the nervous system.

-breakdowns in behavioral function are <u>anatomical</u>.

-the localizationist researchers at Queen Square had done the exemplary experiments on localization of function, and were experts at treating <u>related diseases</u>.

We can see that localizationists eventually intertwined questions about the nature of phenomena (standpoint), the strategies for organizing information and resources (heuristics), political commitment rules and the collective endeavor.

5. <u>Implications of Perspectives as a Unit of Analysis</u>

Research on perspectives has rarely taken into account the processes and dimensions described above, especially in the degree to which these complex multiple dimensions are <u>interactive</u> and <u>developmental</u>. What are the implications for looking at perspectives in this way?

A conference with Anselm Strauss provided a partial answer to this question. As I was describing to him the many participants in the debate about localization, and the various kinds of work and uncertainties faced by participants, I began to frame the concept "inertia." I saw the questions becoming both extraordinarily <u>complex</u> and, at the same time, _taken for granted_ by participants. In the middle of explaining this, and while I was feeling overwhelmed with the complexity and interdependence of all the issues, Strauss asked me: what would it have taken to overthrow it?

Two very different answers for this perspective can be given at the beginning and end of the period examined. In 1870, a funded, well-organized counter-program with both clinical and basic physiological bases might well have defeated the localizationists in Western Europe (see Todes, 1981, for an example of this in Russia). By 1906, however, the perspective was so entrenched that overthrowing it would have taken something more nearly catastrophic. No counter-program could have found a strong institutional base since no need for it was perceived by either patients or other doctors; counter-evidence had already been produced in many other sites, and had often been partially assimilated into localizationism.

Understanding the processes and conditions under which such "obvious," "natural," and powerful theories about nature take hold is important in several respects. First, such theories about nature have enormous political implications. This has been well-documented in the case of racist and sexist theories (see e.g. Gould, 1981; Hubbard and Lowe, 1979; Caplan, 1978), where bias consists of inaccurately characterizing or excluding certain human groups.

Less well-documented are the political implications of perspectives without such connections to such groups. Current studies underway on the history of quantification (Hornstein, in prep.); on stylistic commitments in scientific research (Gerson, in prep.); and on the development and use of taxonomies (Volberg, 1983a and 1983b) suggest political ramifications of aspects of perspectives which are often not thought of as political. In order to understand the issues in any policy decision, these elements need to be examined and understood.

Second, perspectives are a useful unit of analysis in researching the history of scientific disciplines. Rather than studying the diffusion of ideas, or the intellectual history of a discipline without its constraining material base, the study of perspectives provides a new way to analyze issues that traditionally have been characterized as either "internalist" (solely the ideas and techniques employed by members of the discipline) or "externalist" (solely shaped by institutions and politics outside the workplace). The debate about internalist vs. externalist history of science has been a long and acrimonious one, parallel in many ways to the "macro"/"micro" debate in sociology. From the analysis of perspectives presented here, we can see that such distinctions are spurious, and that the relationship is dialectical and emergent, not parallel and additive. Problems arise in the course of work, and problems arise due to the location of and constraints upon work. Their solution may change both the way the work is done and the institutional forms within which it is done.

Third, the study of how perspectives take hold and become seen as "natural" is important in answering some basic questions in the sociology of knowledge and epistemology. This thesis argues that problems-theories-facts-perspectives are a form of collective behavior, and provides some data about the processes and conditions of that behavior. Implicit in this approach is an equation between knowing and working. I argue here that these two kinds of events do not proceed in parallel, but rather that they are the same activity differently reported.

In sum, the study of perspectives as presented here provides an analytic framework for understanding the politics and work of knowledge. While the data are historical, and based in one scientific field, the framework is more generally applicable--to science in general and to all forms of knowing that involve evaluating what is true.

NOTES

1. Although Gerson and Star, 1983, point out that the degree of discardability decreases as an argument becomes more privileged and tied in with organizational func-

tioning.

2. See e.g. Mead's (1964a [1927]) "The Objective Reality of Perspectives", Dewey's _Logic_ (1938), _Reconstruction in Philosophy_ (1920), and _Essays in Experimental Logic_ (1916), Bentley's _Inquiry into Inquiries_ (1975a) ;James' _Pragmatism_ (1928), and Dewey, et al.'s _Creative Intelligence_ (1917).

CHAPTER TWO:

INSTITUTIONAL CONTEXTS OF LOCALIZATION WORK: SEGMENTATION, PROFESSIONALIZATION AND THEORY DEVELOPMENT

1. Introduction

The central process documented in this chapter is the simultaneous development of a school of thought, a medical specialty, and the professionalization process in medicine. Because localization theory developed when it did, the mechanisms of professionalization and specialization were used in the service of the theory itself. Localization theory and a rising acceptance of "scientific medicine" also became entwined in complex ways. The remarkable success of localizationism, and the complex theoretical debate which surrounded it, must be understood in light of these "shared fates" (Bucher, 1962).

Strengthening a school of thought (or theory) by linking it with professionalization mechanisms is an interesting and little-studied phenomenon. Bucher and Strauss (1961) have described professions in terms of emergent, confluent segments, with different implications at different historical junctures in the profession. Bucher (1962) has described the emergence of segments within a profession based on a shared organization of commitments.

But the emergence of a research school simultaneously with professionalization has not been examined with respect to the shared mechanisms of schooling and professionalization. Several hitherto neglected questions about schooling are involved: how does _timing_ in the trajectory of the profession in which it develops affect a theory's success? Do theories which develop in already-established professions have different careers from those which are developed at the beginning of a professionalization process? What other dimensions are associated when a theory and a profession have a "joint fate"? How is specialization important here? And finally, since occupations often use research to professionalize, is professionalization a common way to make theories robust (Hughes, 1966)?

This thesis argues in part that the timing and joint mechanisms of professionalization and the development of a theoretical perspective are very important for the success of the perspective. As this chapter shows, localization theory was introduced at a time when many of the social contexts of scientific investigation were changing. The direction of change supported new, easily packaged theories. Perhaps more importantly, the development of professional autonomy and the rising status of specialization in medicine provided an important basis for _validating_ localization theory.

The _joint fates_ of localization theory and certain specialties within medicine greatly strengthened the localizationist position. At the same time, localizationist research was used to strengthen the position of surgeons and physiological researchers, as this chapter will show. Timing is critical here, since had localizationist theories been introduced later in the medical professionalization process they would no doubt have been subjected to a more competitive evaluation process.

2. Shifting Work Contexts

The principle investigative work into cerebral localization was done by British doctors. They did both clinical and basic physiological work, including neurology and neurosurgery. The contexts for this work were rapidly changing in the 1860-1900 period, especially the medical profession and the hospitals. The institutional contexts I describe here are not "backdrops" to localization work, but, rather, constraints and resources (Gerson, 1976) for those constructing the theory. As the theory developed, those constraints and resources underwent some dramatic shifts, most of which strongly favored the theory's survival and increasing robustness. Several key points about these shifts should be noted:

- The evolution of hospitals and medical practice in England over this period strongly supported, even required the development of medical entrepreneurship in the form of specialization.
- Scientific research was used to legitimate specializa-

tion.

- Medicine was highly competitive, and successful specialization depended on referrals and patient demand.
- Technical advances in medicine aided some previously incurable illnesses or injuries (or were seen to have potential to do so), and innovation was regarded as crucial in the profession.
- Medical reform restructured the profession to equalize surgeon and physician status, change licensing regulations, and expand representation on the regulating bodies.
- Medical education changed from an apprenticeship, craft-oriented occupation training to programmatic, institutionally-affiliated and modular format professional training.

Within this shifting context, localizationist doctor/researchers came to form a coherent group, offering a package of theory and therapy based on localizationist principles. It was *as* a coherent group, in situations highly favorable to such a theory/therapy package, that localizationists won the debate about the nature of brain function (Star, 1982a). That they won to the extent that they did is remarkable, and can only be understood in light of multiple situations. This chapter discusses several of them: the medical profession, the hospitals, patients, British physiology, and neurosurgery and medical technology.

2.1 <u>The Profession: Chaos, Reform and Entrepreneurship</u>

Hughes (1966), citing T.H. Marshall, defines a profession as: (1) work organized so that <u>caveat emptor</u> [1] [2] cannot be allowed to prevail and, (2) so that income is sufficient that practitioners can pursue a "life of the mind." He further notes that (3) research is often used to raise the status of a profession, and (4) that when professions are lodged in complex institutions, client-practitioner relationships are even more complicated than usual. [2]

English medicine in the 1860-1900 period contains these four strands, but they are often in conflict with each other. Self-regulation of medicine [3] and claims to scientific precision appeared to fight <u>caveat emptor</u>, but at the same time, competition for patients was fierce, standards were irregular, and poor patients often, without consent, became experimental subjects (Anderson, 1966). There was a fierce reform movement within the profession, especially during the 1870s and 80s, which sought to abolish the medical corporations (that is, the elitist Royal Colleges) and equalize professional membership (McMenemey, 1966).

At the same time, doctors were anxious not to be seen as "merely tradesmen." They cultivated "gentlemanly" hobbies and wrote philosophical tracts, emulating the aristocratic traditions of the medical corporations (Reader, 1966; Peterson, 1978:39). While research, including localization research, was used to legitimate changing medical practice and education (Peterson, 1978:260), it was also attacked by the upper-class dominated antivivisection movement (French, 1975).

And finally, medicine changed from apprenticeship/private schooling and private practice bases into hospital-based training and practice. This simplified some aspects of medical practice and complicated others. It made specialist doctors easier to identify by virtue of their association with specialist hospitals (Peterson, 1978:264-65). Thus referrals to specialists became easier. But medical care became more complicated because of the changing nature of medical welfare in England during this period, the rise of public acceptance of hospitals and the development of professional hospital administration and nursing (Abel-Smith, 1964).

So no simple narrative of professional changes or directions in English medicine of this period is possible. But several processes important to the development of localization theory emerged from these cross currents.

In 1858, the Medical Reform Act was passed after years of unsuccessful struggle (Little, 1932). Until this time, medicine had not developed standard, specialized training, coordinated collective action, or self-regulation (Peterson, 1978). The Act established a General Medical Council (GMC), which registered doctors and kept a central, alphabetical list. The GMC was empowered to prosecute alternative practitioners (Lyons, 1966).

This persecution of quacks was an important part of medicine's attempt to move away from a tradesman/salesman image. As Peterson notes, "the essence of quackery was tradesmanship" (p. 258), including claims for secret or unique cures, patent medicines, and unique systems of medicine. The 1858 Act also abolished regional medical licenses. This further weakened the London-based medical corporation's monopoly on licensing.

The 1858 Act was not as radical a reform as some would have liked. Radical reformers wanted to abolish the medical corporations altogether and create a "one portal of entry" into the medical profession. This would have meant that all practitioners would train in medicine, surgery and pharmacy (Parry and Parry, 1976), and the special organizations of physicians, surgeons and apothecaries would merge and thus equalize.

Licensing requirements at this time were extremely chaotic and nonuniform. Even by 1886, there were over nineteen different licensing bodies which could examine in either medicine, surgery, or apothecary (Poynter, 1966b). Requirements for licensing by the medical corporations were also widely varied, and often included heavy fees (Roberts, 1966).

Licensing and exam inequities were exacerbated by the rapid growth of scientific knowledge. It was impossible to definitively establish what had to be known for a given examination (Poynter, 1966a). The combination of high fees and need for "insider" knowledge to pass exams made the medical corporations quite elitist.

From 1832-1860, there had been a gradual switch in medical education from an apprenticeship system or private medical school training course to hospital-based training (Newman, 1966; Little, 1932). This had several important consequences, but most important was the access of medical students to a wide variety of cases (and especially of a number of different patients with the same disease), and the beginning of a more uniform professional socialization process.

As Peterson notes:

> centralized medical education gave the elites of the medical world a time for training them not only in the skills of an occupation, but in educating students to the mores, values, and loyalties of a potentially self-regulating and autonomous profession (1978:89).

As the change in training became hospital-based, curriculum could be much more standardized. There was a move away from complex assessment of the individual patient toward quantitative assessments geared toward particular symptom-diagnosis links (Ellis, 1966). Because medical students in the hospitals, unlike apprentices, could rapidly "walk the wards," they could compare cases and derive a basis in practice for more modular types of education (Abel-Smith, 1964:18). Hospitals in general became increasingly oriented toward teaching and research (Peterson, 1978).

Changes in the curriculum included more formal course work, more lectures and lab work, and more supervised practice. There was a decreasing emphasis on observation and private practice experience. Hospital staffs became jointly responsible for medical education, and students could mix curricular requirements <u>between</u> different hospital schools (Peterson, 1978:60-77). This latter change made a standardized curriculum between institutions important.

As hospitals became the locus of much medical care during the 1840s and 50s, an organizational bottleneck developed. The "consulting" physicians from Oxford or Cambridge, who were in the medical corporations, controlled both medical education and patient fees. There was a sharp division between staff doctors and the consultants at the top of the hierarchy. Staff doctors with junior appointments were on (fairly low) salary, and were forbidden from private practice. Even these staff positions could only be obtained by payment to the consulting doctor (Parry and Parry, 1976: 137). The consultant, meanwhile, also had control over the medical students being trained in the hospital, as well as the chance to develop a lucrative private practice from patient referrals. As Parry and Parry point out:

> The significance of a hospital appointment for the physician or surgeon lay not in the paltry honorarium received, but in the prestige which such an appointment conferred upon him. This was especially the case in London where the appointment connected him to the aristocratic and Royal patrons of the hospital. They would call upon his services in time of need and recommend him to other members of their family as well as friends and acquaintances. By this means a consultant could be assured of a lucrative private practice among those in the highest social circles (1976:137).

By 1860 there was a large number of frustrated junior staff in the hospitals, which were increasingly becoming the locus of medical care. These workers were in fact doing much of the patient care, but getting little in the way of monetary rewards, prestige or hope for advancement. Abel-Smith (1964) estimates that in 1860 there were 15,000 practitioners in England. Of these, 1200 worked in voluntary hospitals, and only 579 were physicians and surgeons in charge of inpatients. The rest were house staff and assistants. In the large teaching hospitals general practitioners who had taken charge of much general patient care were gradually squeezed out, replaced by junior staff who fed cases to senior consultants (Abel-Smith, 1964:21).

One way out of the frustrating junior role was to set up an independent specialty hospital. Between 1860 and 1870, some thirty specialist hospitals were founded, the majority of them by doctors (Evans and Howard, 1936: 153). Sixteen of these were in London. The range of specialties included cancer, nervous diseases and epilepsy, fever, maternity, bladder stones, electrotherapy, homeopathy, and even a special hospital for ulcerated legs.

These specialty hospitals developed in part from the career frustrations of junior doctors with no hopes for advancement in regular hospitals (Newman, 1966). They also were another opportunity to isolate types of patients in order to amass a body of comparative cases (Evans and Howard, 1936). Specialist hospitals had to precede specialist research, given the hierarchical "funnel" structure of hospital organization in the general hospitals.

The specialty hospitals, not surprisingly, drew criticism from those at the top of the hierarchy. A letter from the founder of the London Galvanic Hospital, in response to such criticism, illustrates the situation:

> the London Galvanic Hospital was not established until after I had failed in my endeavour to become connected with my "alma mater," St. Bartholomew's. I offered to undertake the treatment with galvanism of a certain number of picked cases from the outpatients to be handed over to me by the assistant physicians. This offer was backed by the sanction and with the

> encouragement of the whole medical and surgical staff of the hospital with the exception of Mr. Lawrence (whose antipathy to all change is so well known), and the apothecary with whose privileges it would in some way have interfered...
> I am prepared at any time to relinquish the London Galvanic Hospital upon being placed in an established London Hospital, in an honourable and independent position...(Lancet, I, 1863:219, cited in Abel-Smith, 1964:29).

The National Hospital for the Paralysed and Epileptic at Queen Square, while not doctor-founded [4], immediately came to serve the multiple purposes of doctor-founded specialist hospitals. A chance for career advancement and monetary gain, escape from the consultant-dominated general hospitals, and a chance for patient research became available. The localizationists used the specialty hospital context to link localization theory with the burgeoning specialty hospital movement.

Ironically, the organizational structure of the specialist hospitals, while it was rebelling against the bottlenecked hierarchy of the general voluntary hospitals, often replicated that hierarchy. Consulting physicians and surgeons at Queen Square, for instance, were not paid for their services to patients there. Rather, they developed private practices and teaching appointments from the prestige garnered by publicity of the activities at Queen Square. They culled cases for publication, met wealthy hospital sponsors, and brought their patients to exhibit to classes at local medical schools (especially University

College) (Holmes, 1954). Salaried house staff did most of the caretaking, and men like Jackson, Ferrier, and Gowers visited patients only once or twice a week.

Outside the hospital sphere, another important strand of professional development which influenced localization research and practice was the rise of private practice specialization. By the 1870s, there was a lot of pressure within the profession for an exclusively consulting/specialist class of doctors and surgeons, modelled on the legal profession with its lawyers and consulting barristers (Abel-Smith, 1964:102-12). This setup would have taken these specialists completely out of competition with general practitioners. While this never fully evolved, a significant number of doctors did begin specializing and some standing arrangements between specialists and GPs were established (Pound, 1967). In part, specialist practices drew patients with prestige gathered in the specialist hospitals. But specialization itself was legitimated by the research and continuing success of the specialty hospitals.

The development of specialization was important for localizationists. As patient-doctor systems of referral were established, and as specialization itself became legitimate, Queen Square was able to establish a solid place as the specialty hospital for nervous diseases. Within that context, localization theory flourished, unchallenged by theories of medical competitors in general practice.

2.2 The Hospitals: Sponsorship and Acute Care

English hospitals, like the medical profession, were rapidly changing during the last half of the 19th century. The various types of hospitals and their legal and bureaucratic conditions were similarly byzantine in organization. A number of reforms also established or changed the nature of free medical care for the poor, including the establishment or abolishment of workhouses and asylums. Since Queen Square was a specialist voluntary hospital, most of this discussion will emphasize changes in those organization structures.

However, it should be borne in mind that _all_ hospitals were in the organizational turmoil at the time. It was often not clear, for instance, who to admit as a "pauper" vs. who was unable to pay but not technically a pauper; who to screen or admit as incurable or acutely ill; who had what kind of disease; what kind of specialist had jurisdiction where; what funds should go to what hospital for what purpose (Abel-Smith, 1964).

In general, voluntary hospitals were charitable hospitals for the working-class poor (not paupers). Patients were admitted with a letter from a subscriber, who promised to pay for their care. Often, patients were servants or tradespeople known to the wealthy sponsor. By the 1880s, an additional source of patient funds was the hospital "Saturday" and "Sunday" funds. These funds came from broad-scale fundraising drives to collect general funds for hospitals. They were sponsored by the Royal family.

The number of voluntary hospitals rose rapidly in the last half of the century. There were 11,000 beds in voluntary hospitals in 1861; by 1891, the number had doubled (Pinker, 1966). By contrast, the specialist hospitals were quite small, with an average of 36-89 beds apiece. Queen Square had about 60 beds during this period. Thus, the "sieve" for specialist patients was fairly fine, reflecting both crude diagnostic techniques for some of the special illnesses, such as brain tumors, and the control exercised by the larger hospitals, and the size of the smaller specialty hospitals.

An important influence on the nature of hospital care in this period was the emergence of "professional" nursing. Nurses from the Nightingale school, who were for the most part upper-class, educated women, began to take over much of the care formerly done by uneducated porters, nurses, or even surgeons (Dainton, 1961:125-26). These women formed alliances with the upper-class hospital governors and patrons, and thus were able to establish nursing "empires" within the hospitals and displace many existing staff. The hospital governors were not inclined, for the most part, to involve themselves in clinical decisions. Thus the nurses could take over much of the daily care, and doctors also became increasingly free to do experimental and specialist/consulting work (Abel-Smith, 1964:68). As mentioned above, junior staff also participated in this process by feeding interesting cases to the doctors at the top of the hierarchy.

The shift to acute care and the development of situations where doctors could do more experimentation was advantageous for localizationists. The more rapid turnover of patients allowed localizationists to develop taxonomies of localization-related illnesses, and a legitimate context in which to combine clinical research and patient care.

2.3 Patients: The Shift to Acute Illness and Patients as "Experimental Material"

During the 1860s and early 1870s, localizationist researchers had conducted research at the West Riding Lunatic Asylum (Viets, 1938; West Riding Pauper Lunatic Asylum, 1871-74). This asylum was administered by Crichton-Browne, later a founder of the localizationist journal Brain. Ferrier's early animal experiments were conducted there. Pathological observations of the brains of dead inmates, speculations about the physiology of mental illness, and Jackson's early observations of localization and epilepsy also took place there. From this site, localizationists moved to Queen Square, which had a higher class of patients, although it was still a charity institution.

The original founders of Queen Square had wanted the hospital to be a place for "incurables," which no one else would admit except for the workhouses and pauper asylums. However, a doctor who was in on the early planning stages insisted that the hospital allow <u>only</u> acutely ill patients (Holmes, 1954). This move, in 1860, reflected a widespread movement on the part of voluntary hospitals to deal exclusively with acutely ill patients. There were two reasons for this, one rooted in the financial and political situations of doctor/scientists and the other rooted in research needs.

First, since the voluntary hospitals were dependent on the charity of rich sponsors for patients, it looked good for them to be handling the largest number of patients possible. As Abel-Smith (1964:39) says:

> When appealing for funds it was an advantage to be treating acute rather than chronic cases. The more acute the cases admitted, the greater were the number of inpatients that could be treated in a given number of beds during the year. Such statistics were valuable for appeal purposes.

On an individual level, there was sometimes conflict between doctors and patrons about this policy. The doctors wanted acute cases, and the subscribers would try to send in a chronically ill servant or tradesperson for care (Abel-Smith, 1964:37). This fight for control of admission was resolved in doctors' favor by the gradual development of emergency rooms, where a small number of acute patients could be admitted and the sponsor-letter system bypassed. Patients thus admitted could be paid for out of general hospital funds (Peterson, 1978:175).

The second reason doctors wanted to admit only the acutely ill was for research purposes. Comparative cases were of rising importance in medicine, beginning in the early part of the century but sharply rising during the 50s and 60s. There was simply more turnover and more chance to compare patients when treating the acutely ill.

Institutional care for the chronically ill poor was relegated to the poor workhouses until 1866, and sick paupers not admitted to voluntary hospitals. Between 1870 and 1900, there was a gradual conversion of pauper workhouses and infirmaries to general hospitals (Abel-Smith, 1964: 127-30). After 1889, doctors began to learn more about infectious diseases in the poor population, hitherto neglected because of these administrative arrangements.

As is characteristic of much medical history and sociology (see criticisms of this by Gerson, 1976b; Strauss, 1979), the story of patients' experience and participation in their care or diagnosis is missing from reports I read of nineteenth century British neurology and surgery. In reading accounts of brain operations and treatments for epilepsy, and in looking at photographs of brain tumor patients in the records at Queen Square, I tried to imagine the patients' experience, daily lives, and participation in their care. While descriptive material is unavailable, several bits of information can be inferred from the records and organizational arrangements.

First, patients and sponsors, as well as general practitioners, were convinced to associate care for certain diseases or symptoms with specialist hospitals. Patients seeking admission to Queen Square learned that inability to speak, severe headache, and tremors were nervous system diseases, in addition to more obvious symptoms like epileptic fits and paralysis.

More important, patients with nervous diseases became trained in describing their symptoms in ways that would fit evolving medical taxonomies. Hospital records from the 1860s had simple, narrative descriptions of fits, including patients' estimates of predisposing conditions (riding in an open carriage and getting caught in a draft were two possibilities cited by patients in the National Hospital records). By the 1880s, doctors had developed standardized forms describing fits and reflex responses. Patients learned to report fits in far more detail, with attention to localization-important signs. This response to doctors' theories required self-monitoring by patients, as well as faith in the doctors' systems.

Some patients carried this faith fairly far. Horsley records an instance in his casebook (Horsley, 1904) where the mother of an epileptic son requests a brain operation for his condition. From his notes, it is clear that she desperately hoped that the surgeon could provide a cure for the illness. Extrapolating from contemporary experience, and data that suggest how patient demands affect doctors' practices, and bearing in mind how much competition there was for patients during this time, we can guess that the belief and participation of patients was not trivial to the success of localizationist-based practice.

As mentioned above, patients at the voluntary hospitals were usually charity cases. In later years, a few paying patients were accepted at some of them. At Queen Square, a paying patients ward was developed in 1875, but it was almost never fully utilized (Holmes, 1954:18). Abel-Smith suggests that a tacit bargain was struck between specialist doctors and charity patients: their free care was predicated upon cooperation in experimental procedures (1964).

At Queen Square, this cooperation was certainly also predicated on desperation. There was simply nowhere else to go besides asylums for those with epilepsy. Since one symptom of brain tumor can be epileptic fits, this helped channel brain tumor patients to Queen Square. Although effective fit-controlling drugs were developed in the

1840s, their use did not become really widespread until the 1860s (Temkin, 1945). By the turn of the century, Queen Square itself was dispensing some two and a half <u>tons</u> of potassium bromide to its patients every year (Rawlings, 1913).

2.4 British Physiology: Weakly Established Research Programs and a Common Enemy

From 1840 to 1870, British physiology languished, especially by comparison with developments in France and Germany (Geison, 1972; 1978; Rothschuh, 1973:305-6). Geison attributes the lack of developments and discoveries, and the slow growth of British laboratories and professional positions, to a combination of social and institutional factors. England lacked the kind of interlinked, competitive and state-supported university systems that France and Germany had developed. Institutions were isolated, and the state was not particularly interested in supporting basic science. Rather, science was seen as a hobby for gentlemen, and that model continued through about 1870.

One consequence of this lack of institutional base was that there was virtually no professional physiology outside of medical schools before 1870. In that year, Michael Foster was appointed praelector of physiology at Cambridge. Burdon-Sanderson was appointed professor of practical physiology and histology at University College, London, with

Edward Schafer (who later changed his name to Sharpey-Shafer) as his assistant (French, 1975: 42).

But even after 1870, institutional support for physiology was slow to develop. Before 1870, English medical physiology had had an extreme anatomical bias. Anatomy or dissection formed the basis for analysis of function. During the early part of the century, this was not much at odds with physiology as practiced in other countries, whose practices were also anatomically-based. But this trend continued in England past the point where it had been transformed in France and Germany by investigators like Bernard and Ludwig. So, while jobs for physiologists began to open up after 1870 in English medical schools, they were still tied to these methods of investigation, which did not include good experimental laboratory facilities (Geison, 1978).

For those who wished to pursue medical/physiological research on a full-time basis, then, the situation was limited. As French (1975:151) describes it:

> In contrast to the strength and entrenched position of the medical professional as a whole was the position of those individuals within it who considered themselves medical scientists, who wished to expand opportunity in medical science, and whose ultimate goal was the establishment of an at least semiindependent scientific profession. This group had a low degree of autonomy in the mid-seventies, for the institutionalization of experimental medicine as such had only just begun. Insofar as they considered themselves scientists rather than healers, their political strength as an occupational group or nas-

> cent profession was negligible. Physicists, chemists, geologists, and astronomers did not share a meaningful professional identity with biological scientists, since virtually none of them rallied to the side of professional medicine. In any case, the political experience of various scientists was far less extensive than that of medical men... .

Where in Germany and France physiologists had begun to base their science on chemistry and physics, and to make alliances and design laboratories on this basis, this was not happening in England. The fate of English physiology then, certainly through the 1870s and in my opinion through the turn of the century, was dependent on the fate of medicine. As Geison says:

> In so pragmatic a setting as Victorian England, it should not surprise us that physiology could expand and ultimately secure its independence from anatomy only by exploiting its traditional role in medical education (1978: 153).

This was true both in the available range of problem selection, the treatment-oriented nature of problems selected, and the kinds of equipment and laboratories that were made available. And nowhere was this more true than in the study of localization of function in the brain.

Investigators into the localization of brain function came into a scientific institutional context in physiology that was theoretically disorganized, scattered throughout the various medical schools and hospitals, and biased toward anatomy/function investigations with practical payoff. Equipment for physio-chemical or metabolic experiments was not available, nor was the training or support from physicists or chemists. _Insofar as they remained within the constraints of medical investigation_, then, investigators into localization had something of a clear field with good political backing in which to establish physiological claims.

The single major opponent of localization research _per se_ was not established physiology. Rather, it was the antivivisection movement, which condemned all research on live animals. Ironically, however, antivivisection succeeded in _uniting_ English physiologists rather than decimating them. The Physiological Society, formed in 1876, was a direct response to antivivisectionists' harassment of investigators. Its first purpose was to protect the interests of physiological researchers from antivivisection. In the original Physiological Society were Burden-Sanderson, Sharpey-Shafer (a.k.a. Schafer), Michael Foster, David Ferrier, and Hughlings Jackson--all of the extant professional nonmedical physiologists in England and two of the most prominent localizationists (Sharpey-Shafer, 1927).

The Cruelty to Animals Act of 1876, engineered by the antivivisectionists, stipulated that anyone performing experiments upon living vertebrates must submit an application to the Home Office. This application had to be endorsed by one of the medical corporations or the Royal Society, as well as by a professor of medicine or medical science. Experimenters had to perform experiments at a place registered with the Home Office, and these places could be inspected at any time (French, 1975: 143). Thus, physiological investigators employing vivisection were _legally_ bound to the approval of the medical profession after 1876.

In 1881, an International Medical Congress was held in London, which was widely publicized and in which the Physiological Society took an active part. Many of the statements (including Michael Foster's opening address) read to the Physiological Section, concerned the importance of vivisection, and deplored the antiscientific backwardness of antivivisection (MacCormac, 1881). At that conference, as discussed in detail in Chapter 4, Ferrier confronted German antilocalizationist Goltz. In a widely-publicized debate, he was said to have "defeated" Goltz (MacCormac, 1881; Thorwald, 1959). One other important outcome of that debate did not concern localization _per se_, but rather the position of research with regard to vivisection.

After reading about the Congress and the Ferrier-Goltz debate, the antivivisectionist Victoria Street Society decided to prosecute Ferrier under the 1876 Act for having performed surgery on his monkeys without a license. Ferrier was arrested and put on trial. It turned out at the trial that Gerald Yeo, his associate, had done the actual operation. Yeo _did_ have a license with the Home Office, so Ferrier was acquitted. Meanwhile, his trial attracted the attention of the medical profession and press. The profession was outraged, and decided to take care of its own (see e.g. discussion in notes of medical society of University College, where the association voted to support Ferrier, Minutes of the Medical Society, University College Hospital, Vol. 8, Dec. 7, 1881;editorical in the _British Medical Journal_, 1881). "The costs of Ferrier's defense were undertaken by the B.M.A., whose solicitor represented him in court" (French, 1975:202).

Ferrier's arrest shocked the medical profession, and served to mobilize them in favor of physiolgical research, and, coincidentally, of localization. An even firmer alliance against a common enemy was forged between the Physiological Society and the profession of medicine (French, 1975:203), with localization research at the heart of it. Thus, the mechanisms of professional autonomy and self-defense were entrained in service of localization theory and vivisectionist physiological research.

The Act of 1876 also helped vivisectionists gather allies from other sciences, particularly from evolutionary theory. Among the biologists who supported vivisectionist research were Charles Darwin, T.H. Huxley, George Rolleston, and Joseph Hooker, then president of the Royal Society (French, 1975:152). Ferrier was elected FRS in

1876 (Holmes, 1954).

An interlinking social world partly based on vivisection and the fight described above can be traced here. Rolleston was among the referees of the Royal Society who accepted Ferrier's first paper for the Society in 1873-74 (Referees Report of the Royal Society, May 12, 1874). Burden-Sanderson hired Ferrier to come to London to do experiments, after his work at West Riding (Spillane, 1981). Charlton Bastian, physician at Queen Square, was Herbert Spencer's literary trustee, and a friend and coworker of Darwin, Russell Wallace and T.H. Huxley (Holmes, 1954:39). Hughlings Jackson, also a physician at Queen Square and a leading localization theorist, was heavily and explicitly indebted to Spencer for his theoretical assumptions (see e.g. Jackson, 1932c). (Jackson also had complete access to the <u>Lancet</u>, and control of what he published there. According to his biographer, James Taylor, he would often write a short fragment and get the <u>Lancet</u> staff to include it even after they'd done the pasteup for an issue. [Jackson, 1925:19].) Horsley,the first brain surgeon at Queen Square, was a student and collaborator of Schafer's at University College.

The publicity achieved in the antivivisection battle by Ferrier (and later, by Horsley) was a two-edged sword. Many of the governors of Queen Square were antivivisectionists, and did not approve of the experimental work done by its staff. Rawlings (1913) says that the hospital lost "thousands of pounds" in donations because of the publicity given to Ferrier's experiments.

At the same time, the sheer publicity afforded Ferrier by the trial did wonders for his private practice, which burgeoned. This presumably helped him to finance his physiological experiments. These were all privately funded except for a small grant from the Royal Society. Perhaps the hospital governors were assuaged by the success of brain surgery, which purportedly used principles derived from Ferrier's experiments to locate brain tumors.

2.5 Technical Advances in Medicine and the Rise of Neurosurgery

The first successful removal of a brain tumor based on localization theory was performed in 1884 by surgeon Rickman Godlee, with neurological localization diagnosis by physician Hughes Bennett (Bennett and Godlee, 1884). Before this time, brain surgery had been seen as impossible, except in cases of concussion or abscess which did not penetrate the brain tissue itself (Cushing, 1905). Following the 1884 operation, localizationists and surgeons collaborated on a number of operations (Horrax, 1952; Sachs, 1952). These were closely followed by the medical press. By 1900, Queen Square was performing 100 operations a year for brain and other nervous system tumors

(Blackwood, 1961; Critchley, 1949).

Much medical history reconstructs a straight line between the development of localizationist theory with animals by Ferrier and others in the 1870s, and the locating and removal of a tumor from the brain, a natural outgrowth of this theoretical development (see e.g., discussion in Horrax, 1952 or Sachs, 1952). This is nonsense. Surgery on the brain, including brain tumors, was part of a long series of technical improvements on surgery of the nervous system and skull, including the control of bleeding, and crucially, the development of antisepsis.

Once these techniques were developed, however, there was still certainly no 1:1 correlation between "localizing signs" and the location of a tumor. The uncertainty pervading neurosurgery will be discussed in detail in Chapter 3, but for now the point is that brain tumor operations were <u>claimed</u> to prove localization. These <u>claims</u> were highly successful and benefitted both surgery and neurology.

Rickman Godlee, Joseph Lister's nephew (Godlee, 1917), was selected by the localizationist physicians (including Ferrier and Jackson) to perform the first brain tumor operation in large part because it was certain that he would use antiseptic techniques (Thorwald, 1959; Spillane, 1981). Ferrier, Jackson, and, some say, Lister himself were present at the operation. Bennett and Godlee's report of the operation in the Lancet was widely hailed and cited, especially by the doctors at Queen Square. The patient lived only six weeks, but this was longer than any previous such patient. The operation heralded a new era and the cementing of a neurology/surgery intersection based on localizationist theories of nerve and brain function (Cushing, 1905; 1910;1913).

This intersection grew in part from the technical strength of surgery on the one hand, and its lack of physiological theory on the other. The state of affairs in 1884 with neurologists and surgeons included the following elements:

Neurologists had great theoretical riches in localizationism, but were quite impoverished with regard to clinical techniques. They relied heavily on iodides and elec trotherapy for treatment, neither very successfully (Cushing, 1910).

Surgeons were developing increasingly good mechanical/technical skills for nervous system and cranial operations (Rogers, 1930). MacCormac, in Scotland, had performed a number of successful trephining and brain abcess operations using antiseptic surgery (MacCormac, 1880). But surgeons lacked physiological theories or pathological training which would help move their status from mechanic to clinician (Cushing, 1910).

Patients with brain tumors or brain abscesses faced incorrect diagnosis and sometimes incarceration in lunatic asylums; near-certain death if they had a brain tumor removed (Cushing, 1905).

After the 1884 operation, yet more surgical techniques were invented to improve patient mortality rates for brain tumor and abscess operations (Rogers, 1930). Horsley, who was appointed to Queen Square in 1886, developed a new bone wax that helped stop bleeding from the skull (Paget, 1919). He was also a user of antisepsis, having worked with Pasteur on rabies research. Topographical methods were invented which would allow surgeons to find areas of the brain under the skull, and to drill the smallest possible holes in the skull. "Listerism," or antisepsis, gradually became standard operating procedure in hospitals by the turn of the century (MacCormac, 1880). And surgeon's assistants became more expert in administration of chloroform, thus decreasing accidental deaths from anaesthetic overdose.

The combination of the surgery/neurology intersection and increasingly good technical skills became a powerful source of argument for localizationism. The application of localization theory to brain tumor removal was claimed as a clinical justification for vivisection techniques, and the "cure" of a hitherto mysterious and hopeless set of diseases claimed as a potential, if not actual, victory for localization theory.

3. Summary: Perspectives and Work Contexts

From the description above of the multiple institutional and professional contexts of localization work, it is clear that localization theory was favorably positioned. Localizationists had multiple organizational supports and circumstances for their theory. Diffusionists, on the other hand, were widely scattered and had no such supports.

Diffusionists were not an organized group in any sense, and I found no diffusionist research or critics based in England after 1870. The three major diffusionist critics of the group at Queen Square were based in different countries: Goltz in Germany, Panizza in Italy, and Brown-Sequard primarily in France and America. Although they cited each other, they did not collaborate or organize professionally (see discussion in Chapter 4). These critizers were thus not intertwined organizationally, nor did those attacking the theory have organization authority over localization researchers.

The elements of successful perspectives outlined in the last chapter (inertia, momentum, incompleteness, reification, and inseparability) were embedded in the work contexts outlined in this chapter. I will discuss the mechanisms involved in each briefly here.

1. Inertia. The mechanisms of professional autonomy

contributed to inertia through the privatized control of evaluation of results. Once instituted, results became difficult to challenge. Medical reform movements and the need for a saleable commodity in medicine also contributed to inertia in the sense that new products immediately became valuable to these professionalizing groups. The influence of antivivisection on inertia was considerable, since localizationists and all vivisectionist researchers felt the need to take an unwavering and united stand against a common enemy.

2. Momentum. The alliance between the development of the theory of localization and the segmentation/specialization processes in medicine contributed to the momentum of localizationism. There was a common legitimation that speeded up adoption of the theory in different sites. The rising position of physicians and surgeons also helped the theory gain momentum, as it was picked up and used by these rapidly rising groups. The general rising prestige of medical research also helped it gather momentum.

3. Incompleteness. Rapid segmentation processes, again, professional autonomy, turf battles between specialists and general practitioners, and the rise of specialty hospitals with their own separate domains of expertise contributed to make the perspective impossible to fully describe from any single point.

4. Reification. Several aspects of the work contexts

described here contributed to cumulative reification. First, the need for a commodity in medical practice meant that theories had to be developed that were easily described, not terribly complex, and which would remain fixed. The movement toward standardized diagnostic categories also contributed to reification, partly due to the need for modular hospital training and other similar shifts in medical education.

5. Inseparability. Because localizationists were banded together against a common enemy, political alliances forced intellectual alliances and logically necessary connections between vivisection and localization, evolutionary biology and localization. The institutional base of physiology in medicine and the overlapping social worlds investigating localization also meant that parts of the localizationist perspective became increasingly institutionally intertwined over the development of the perspective.

These elements were also rooted in the daily work context and conflicts of localizationists, the actual process of hammering out the theory's details and developing conventions for carrying on the various work that comprised it. The daily work context, and its uncertainties, are discussed in the next chapter. The details of the debate are discussed in Chapter 4.

FOOTNOTES

1. "Let the buyer beware."

2. I am aware of disagreements in sociology about definitions of "profession" and agree with Roth's (1974) caveat that we need historical, developmental studies which focus on working groups' interests and their conflicts and locations in society, rather than fruitless demarcation battles about what is a profession and what is an occupation.

Similarly, Light's (1983) discussion of the development of professional schools in America points to an enormously complex set of processes in "professionalization," which cannot be simply or easily characterized as the same across situations. Light states that many professional training schools are in a structurally ambiguous position; they do academic research and vocational training at the same time. This leads to an irregular and "layered" development of professional autonomy, in which clients play an extremely important part. The description of professionalization in this chapter attempts to encompass some of these complexities and ambiguities.

3. In the form of prosecution of "quacks," licensing restrictions, and attempts to abolish competitive fee-setting (Lyons, 1966; Poynter, 1966b).

4. The National Hospital for the Paralysed and Epilep-

tic at Queen Square was founded by two women, the Chandler sisters, in 1860. They raised the money for the hospital by selling hand-strung necklaces door-to-door. The lack of medical care for a paralytic relative was the catalyst for their fund-raising effort.

CHAPTER THREE

UNCERTAINTY AND CONVENTIONS: TRIANGULATING CLINICAL AND BASIC WORK

1. Introduction

Professional autonomy and specialization helped localization theory become accepted in medicine. Once accepted, these same processes helped legitimate and sustain the perspective described in Chapter 1. Moving to another level of analysis, it is important to remember that localizationists faced daily work uncertainties which had to be resolved in ways acceptable in those larger institutional and professional contexts described in Chapter 2.

In this chapter, I examine those daily work uncertainties and the responses of workers to them. Each type of work discussed here--clinical neurology, physiology, pathology and neurosurgery--had different purposes and was attended by different kinds of uncertainty. In collectively dealing with these uncertainties, those in each line of work developed conventions for making their work more routine and certain (Becker, 1982). These conventional solutions to the problems of uncertainty in daily work contained biases. That is, certain data or problems were systematically included and others were excluded through the use of conventions. Some things would be included at the expense of others.

These biases were exhibited in the <u>choice of heuristics</u> to solve problems, in the <u>modes</u> of argument chosen to frame problems, and in the management of <u>anomalies</u> arising in the course of problem solving.

As localizationists framed their arguments for the validity of localization theory (either in debates with diffusionists or in arguing fine points with each other), they combined evidence from these four lines of work noted above. I call this combination of results from different lines of work <u>triangulation</u>, a term traditionally used in methods to refer to using different kinds of measurement of the same object to minimize error and maximize available detail and generalizability.

Each kind of evidence contained the conventional biases adopted by workers in each line of work, who faced different daily uncertainties. The triangulation of results in arguments for localization theory incorporated biases in three ways:

1. By <u>shuffling anomalies</u> which appeared in one realm into another--that is, dismissing anomalies arising in the course of problem solving in one line of work by presenting and accepting solutions from another line.

2. By <u>overlapping or fitting pieces of evidence</u> from different realms into a common <u>a priori</u> framework, and thus

creating a complete picture using parts from different realms (see Kauffman, 1972).

3. By _attributing certainty_ to results from other lines of work: accepting results at face value and ignoring uncertainties found in the process of generating results.

Arguments which contain triangulated evidence are especially difficult to decompose in a simply _logical_ way. There are two reasons for this:

1. _Conventions obscure biases_. Adding conventions from multiple lines of work within a single argument makes the location (as well as the obscured content) of the biases difficult to ascertain.[1] In evaluating evidence from a single line of work, one can begin to locate and describe biases by looking at what is glossed over, decomposing reports which lump together several kinds of tasks (see p.14), and constructing histories of the work processes, whether or not they are reported in publications. Where multiple lines of evidence are adduced, it is difficult to figure out who is glossing what, and what kinds of packaging have been created by whom (see Law, 1982 for an excellent discussion of these issues).

2. _Triangulation rules have never been well developed_ between (or within) the disciplines which I am examining here. [2]

Certainly, combinations of clinical and basic research evidence to make arguments for positions are still common today. (See Pinch, 1980 for a discussion of theoretical and applied work.) Formal triangulation or combination rules which would guide that process also remain absent. Since the period I am examining, medical researchers have developed the methodological conventions of "clinical trials." This method employs statistical controls and double-blind procedures (Lilienfeld, 1982; Shapiro, 1945). But incorporating case histories, clinical anecdotes or experiences into reports in physiology, epidemiology or other basic research remains common practice. While it is a source of some concern in the small field of philosophy of medicine (see e.g., Heelan, 1977), the lack of debate or methodological attention to its consequences in most of the medical or physiological literature is characteristic of the low priority of such concerns in general. The unaccounted-for consequences of this are thus unknown. This chapter is essentially a case study of triangulation and its consequences.

In this chapter, an historical, work-based decomposition of the triangulation (and combination) procedures used to develop localization theory is presented. The consequences of <u>triangulation biases</u> in this case are large. They determine the approach to many important philosophical questions (detailed in Chapter 5) and are key contributors

to the obduracy of the localizationist perspective in modern neurophysiology.

In 19th century England, as noted in Chapter 2, physiology was predominantly a hobby pursued by doctors. Not until late in the century did it begin to gain purchase in the university systems as integral to curriculum (Geison, 1978). This situation certainly provided no challenge to the lack of formal triangulation rules for combining results from clinical and physiological work. However, this does not fully account for the lack of formal rules which persists to the present day despite vast differences in institutional situations.

Heelan (1977) provides a convincing argument that the exigencies of work and available technology form the triangulation situation and frame its biases. (As this chapter will illustrate, I agree with his analysis.)

> Clinical science then is, for the clinician, not just basic science applied to the diagnosis and treatment of the pathologies of individual patients. According to the grand tradition of the philosophy of science, science or general systematic knowledge operates to explain and elucidate concrete particulars by making inferences from general principles. In contrast...clinical science does not typically operate that way. Instead, clinical science deploys new powers of observation and direct interpretation made possible by clinical instruments and through them reaches directly and observationally the scientific components and processes of the human body. In clinical science, the intermediacy of knowledge is not basic science, but typically a skillfully designed instrument based on good basic science and good technological principles of design.

> Basic science then ceases to serve an explanatory role for the clinician. It determines instead a space of possible scientific events in which concrete particulars are made manifest in the clinical experience of the physician or revealed directly to him through the text these concrete particulars write for him with the help of suitable technology. Clinical science is basic science incorporated nonobjectively...(p.30).

I would add here, however, that the unidirectional flow of information Heelan describes did not hold in my data. Rather, clinical data also provided particulars which legitimated the "space of basic research events" localizationists produced. Incorporation worked both ways, a critical and often-overlooked flow of validation and information in scientific work.

In a lucid description of the relationship between diagnostic taxonomies, nosology (classification of diseases) and symptoms, King (1982) describes this two-way incorporation and its basis in work and segmentation:

> Specialists create their own particular frameworks and make them ever more detailed and precise. Increased precision in one branch may influence the classification in some other branch. Thus, increased knowledge of microscopic appearances of tumors can influence the diagnostic and therapeutic choice that the surgeon makes. However, the surgeon or internist will draw on the diagnostic categories of various specialists only when <u>usefulness</u> has been proved. For the clinician, "use" refers to the value in <u>promoting cure</u>. In research, however, "use" might mean the value in <u>promoting understanding</u>.
>
> Specialty groups will elaborate their own arrangement of classes <u>according to their particular needs</u>. We must appreciate, however, the complex interactions affecting diagnostic categories. Of the different variables one would be the intrinsic sub-

ject matter of the specialty; another, the degree of precision useful in that specialty; another, the cross-influence exerted by other groups. There is no sharp line of division, but all flow one into the others as the occasion requires (pp.103-104; emphasis added).

It is precisely this flow, and the requirements of the occasions, that this chapter attempts to chart. The "requirements of the occasion" are the pragmatic needs faced by workers in identifying and sorting out phenomena. As Dewey (1920) put it, this is the basis for all classification systems:

> A classification is not a bare transcript or duplicate or some finished and done-for arrangement pre-existing in nature. It is rather a repertory of weapons for attack upon the future and the unknown. For success, the details of past knowledge must be reduced from bare facts to meanings, the fewer, simpler and more extensive the better. They must be broad enough in scope to prepare inquiry to cope with any phenomenon however unexpected. They must be arranged so as not to overlap, for otherwise when they are applied to new events they interfere and produce confusion. In order that there may be ease and economy of movement in dealing with the enormous diversity of occurrences that present themselves, we must be able to move promptly and definitely from one tool of attack to another. In other words, our various classes and kinds must be themselves classified in graded series from the large to the more specific (p. 154-55).

Dewey here implicitly points to the basis in work of classification systems--the need for tools that are simple enough to be practical, but which can produce a fine enough sorting process to deal with a wide variety of exceptional circumstances. We see this development in localizationists' classification systems.

1.1 Conventions and Biases

I will now discuss the conventions developed in each of the four lines of work, and their resulting biases. These are:

1. Clinical neurology. The conventions developed in this line of work are a) standard diagnostic taxonomies based on configurations of symptoms and administration of neurological tests, and b) routines of care and observation based on temporal sequences of events. The biases which result from these conventions are instantial (material can only be dealt with on a case-by-case basis) and black boxing (internal mechanisms and interrelationships of phenomena are deleted or glossed over).

2. Physiology. The conventions developed in physiological work related to localization are a) creation of detailed atlases of the brain and nervous system, which can be decomposed into tiny parts and regions, and b) a "blanketing" approach to the brain and nervous system. That is, a major strategy was to "cover" the nervous system by eking out and describing parts of it--sooner or later the "whole thing" would get covered. The bias which resulted from this was spatial. The phenomena tended to get described in terms of space occupied, not time or sequences of events. Even mechanisms were often described spatially.

3. Pathology. Conventions in pathology were heavily in-

fluenced by its reliance on the microscope and by the highly unstable and difficult organizational circumstances within which pathologists were forced to operate. Pathologists developed a) detailed _tumor taxonomies_ (type of cancer) and b) _cross-sectional_ (slices of tissue) methods. The bias resulting from these conventions was _black-worlding_ (Wimsatt, 1980a)--concentrating only on the fine-grained characteristics of an internal phenomenon, and ignoring environmental (in this case, organismic) effects or developmental effects (either the development of the organism or of the tumor).

4. _Neurosurgery_. Surgeons developed two conventional responses to uncertainty: a) a _focus on technical improvements_ in tumor removal without regard to clinical outcome (what Bucher and Stelling call the development of "vocabularies of realism," 1973), and b) a view of _disease_ as _localized_ and _removable_. The biases in these conventions were _anatomical_ (the world can be explained in terms of structure), and like those in physiology, _spatial_.

When these biases were combined to argue for _localization_, the following _triangulation biases_ evolved:

1. _Physiological investigations were described anatomically_. Instantial, anatomical/surgical evidence was added to the results of physiological investigation, and used to transform physiological problems into problems of location instead of systemic mechanisms.[3]

2. _Diagnostic taxonomies became basic research taxonomies_. An instantial approach to problems was used. Physiological researchers adapted the strategy of trying to match or produce disease locations in their subjects.

3. _Instantial arguments_ (cases) were presented as _analytic arguments_ (rules) and _vice versa_. One serious result was that individual differences got effectively screened out. _Anomalies_ from the analytic realm and _complications_ from the clinical realm were given the same analytic significance despite contextual differences. Thus anomalies could be declared to be of smaller scope than if they were only analytically analyzed. Complications could be declared anomalous exceptions but did not disprove theories (thus a mechanism for downshift or upshift at the convenience of the debater became available).

4. A _modular theory_ was created whose components could be "plugged in" or worked on in interchangeable pieces. This was facilitated by being able to black box or black world according to the needs of the situation (Volberg, 1982). Also, messy aspects of delineating areas (e.g., individual differences, or tumors which grow outside of main areas studied) were deleted.

The implications of these biases can be seen in many modern neuroscience models--the way most Western science has come to think about the brain and the mind. In order to understand the roots of these biases in daily work (as well as the institutional contexts described in Chapter 2), I present below a detailed description of the daily uncertainties in the four lines of work, and localizationists' methods of resolving them.

2. Clinical Work

Neurology and surgery are both forms of clinical work. The general organization of this work is on an individual-doctor, case-by-case basis. Patients are presented one at a time to doctors to be cured, and each case forms the boundaries of the problem to be solved. Within the confines of this work organization, doctors also tried to do clinical research: classify types of diseases, develop new technologies that would be applicable across cases, and try to understand the underlying causes of diseases in general. But it is important to remember that the case is the basic unit of analysis in medical work and commonly in medical research as well. Cures are evaluated on a case basis, not on an epidemiological basis. As we will see in Chapter 4, this led to the use of certain debating tactics (e.g., instantial, or relying on instances) where debates about localization included clinical cases.

This organization is important in understanding clin-

ical uncertainty and the response of clinicians to their daily work uncertainties. Where basic researchers see anomalies or threats to the validity and reliability of their general theories, clinicians see complications, or threats to the cure of their patients. This difference becomes a tricky source of bias in the triangulation situation, where evidence from cases is used to support localization theory, but complications from cases are considered individual and irrelevant differences. Similarly, artifacts from physiological research are unimportant in the individual case diagnosis or management. As Heelan suggests above, clinicians draw on what is relevant to their particular cases from the "space" of physiological results that is useful to them at any given time.

2.1 Neurology

Clinical neurology consisted of diagnosing patients with diseases of the nervous system, classifying types of diseases, making admissions and discharge decisions, writing up cases, publishing cases, and exchanging information with support staff, including house (general) physicians.

Much of the daily work of being a neurologist at Queen Square consisted of administering and refining various tests. These tests usually involved prodding, poking, and physically examining patients, trying to elicit certain classes of reaction (such as a knee jerk or ankle movement)

from them. After neurologists administered the tests, they had to interpret them.

These procedures offered many sources of uncertainty. I have divided the discussion here into four types: nosological, diagnostic, organizational and technical.

Nosological uncertainties are those which occur when doctors try to classify diseases. This was a particularly difficult problem with types of epilepsy--symptom configurations overlap and are highly variable between individuals. Similarly, brain tumors had highly variable symptoms, and in addition were fairly rare. Arriving at a "typical picture" of any neurological disease is always complicated for these reasons.

Diagnostic uncertainties arise in trying to identify the type of disease a given patient has. Nosological and diagnostic uncertainties are interactive: if you're not sure what a given disease should typically look like, how do you tell if someone has it? If someone's symptoms are highly variable, or identifying them is problematic, how do you build a clear picture of a typical disease?

Organizational uncertainties arise from unclear communications within the workplace and divisions of labor which interrupt the clear conduct of work, such as unclear communications between caretaking staff and physicians. Organizational uncertainties also include those about sponsor relationships, funding, and professional status.

Technical uncertainty develops as a result of inadequate tools, malfunctioning tools, tools whose information is difficult to interpret, or whose products are subject to multiple interpretations. (There is an organizational interaction here, as workers from one site use tools differently from those in another.)

The general situation in clinical neurology at Queen Square was one of great uncertainty in all of these areas. As time went on, and physicians gained expertise with classes of patients, developed new tools and taxonomies, and developed a smoothly-working organization, methods for handling these uncertainties were conventionalized and packaged. These conventions allowed uncertainties to be dealt with in a standard way, if not eliminated altogether.

2.1.1 Nosological Uncertainty

The hospital at Queen Square admitted patients with certain symptoms: fits, paralysis, and/or inability to speak were the most common. The doctors who worked there were faced with the problem of constructing disease categories from the array of patients who appeared before them with these symptoms. Recall that the institutional arrangements in medicine up until that time had not permitted large numbers of patients with the same types of symptoms to be grouped together for purposes of study (see

Chapter 2). Nosological categories often followed strictly logical classification schemes (Newman, 1957), like the eighteenth century botanical systems. The first big changes in nosological work had come with the transfer of medical education into the large general hospitals, where medical students could be instructed with large numbers of comparative cases (Abel-Smith, 1964). With the rise of specialty hospitals like Queen Square, an even finer "sieve" was constructed, and patients presumed to have nervous diseases were grouped at Queen Square.

But there was a lag between the mechanisms of admission and the mechanisms of classification. Patients were admitted with fits or with paralysis, and some connection to the nervous system was usually presumed by doctors. However, beyond that initial screening process, there was little to help doctors construct classification systems. Epilepsy was a mystery; it had many forms, perhaps constituted many different diseases, certainly had many different causes (Temkin, 1945; Waller, 1882). No clear models existed which clearly linked cause and effect, or which clearly demarcated types of epilepsy. Brain tumors were equally vague and multiform. Brain tumor symptoms included some of the most common symptoms found in many other diseases: nausea, headache, dizziness, vision difficulties. These could be configured in many different ways and did not

appear in any one "typical" form. In addition, brain tumors were quite rare, so each case with its individual quirks loomed large in constructing the nosological picture. No comparative samples were possible.

Individual differences were another source of nosological uncertainty. Individuals produced widely varying "pictures" of different diseases. This was especially true with brain tumors. Buzzard (1881:132), for example, notes that one patient with a huge brain tumor appeared to be unaffected by the tumor until he died from it: "_It is remarkable_ that a person suffering from so extensive and grave an intracranial lesion should have been able to enjoy a long day's hunting within a week of his death."

Doctors tried to explain individual differences by classifying some groups of patients as more "susceptible" to nervous diseases, for example, those incarcerated in the lunatic asylums. Ferrier (1882) notes that asylum patients were first grouped according to mental disease, then autopsied after death to determine the physiological correlates of those diseases.

But this classification approach to nosological uncertainty was not successful for classifying types of epilepsy or brain tumors. Populations were too internally diverse, and symptoms could also mercurially change within a patient. Patients at Queen Square were ultimately diagnosed as having tuberculosis, syphilis, stroke, tumors, or

lesions from injury. This mixture of patients with overlapping symptoms made it difficult to sort out the defining characteristics of brain tumors or diseases of the nervous system.

The episodic nature of epileptic fits, the problem of recovery of function (the ability of a patient to relearn or reproduce lost functions after injury or tumor), and the widely overlapping symptoms between disease categories made the whole picture one of extreme confusion, both diagnostically and nosologically.

> It must be realized that a lesion identical with that which evokes a characteristic Jacksonian attack when in proximity to the motor cortex, may, when situated elsewhere, elicit what appears to be a general convulsion without warning (Cushing, 1910: 333).

Paget, discussing a speech of Horsley's, says:

> He spoke of the confusion of theories of epilepsy, and of the vague phrases in which the disease was described. He would have nothing to do with Nothnagel's theory of the existence of "special convulsive centre." He discussed the value of the theory that each hemisphere "represented both sides of the brain"; and said that it would not be proved either from clinical evidence or from experimental evidence....He was of the opinion that convulsions due to cortical discharge are evoked in various groups of muscles by nerve energy proceeding from that centre in each hemisphere which is in relation to each group of muscles, and that in generalized epileptic convulsions both cerebral hemispheres are involved (1919: 122-23).

Temkin says that:

> Of course it was realized that organic changes in the brain could lead to epilepsy. But even in these cases it was emphasized that such organic lesions had to be considered remote causes producing the disposi-

> tion to the disease, rather than directly responsible agents (p. 274).

Doctors at Queen Square took several measures to cope with this nosological uncertainty. These strategies reflected both their available resources, and the sequence of events as patients presented their problems. First, they proceeded to sort epilepsies by <u>order of appearance</u> of symptoms. One of the only clear bases for classification was to sort symptoms on the basis of <u>when</u>, and <u>in what order</u>, the symptoms appeared to affect <u>different parts of the body</u>. This "march" of symptoms became the criterion for distinguishing types of epilepsy (Jackson, 1876; 1878; 1879; Temkin, 1945).

Its primary author was Hughlings Jackson, who became affiliated with Queen Square in 1862. Another response to nosological uncertainty was soon added to the repetoire. Jackson (and later, Ferrier) began to link the <u>order of appearance</u> of symptoms to localization theory. Did epileptic fits sort into disease categories by <u>region</u> of origin? Doctors eagerly seized on this idea. When Fritsch and Hitzig's research was published in 1870, it showed that convulsions might <u>begin</u> locally and spread. Temkin (1945) notes that:

> Here indeed was experimental proof for Jackson's clinical observations and pathological inferences. Then Ferrier's experimental work followed and anatomical investigations of the conductive fibres, all of which gave a sound basis for Jackson's contention that localized convulsions indicated localized in-

juries of the convolutions and that the convulsions might spread, if the discharge involved more and more nervous tissue. Ferrier insisted that as a result of his and Jackson's work the dualistic pathology of epilepsy should be abandoned (p.307). [4]

The sequential classification system was used by doctors, attendants and patients. Most neurologists developed pre-printed "fits sheets." Patients, nurses, attendants or kin would record the sequence, location of spasms, and other particulars about the fits. These forms were then studied by doctors like Jackson. The forms themselves were an attempt to deal with nosological uncertainty, and to gather data about the sequence of fits in a systematic fashion. (They were also thus an important part of patient, kin, and attendant work. See Strauss et al., forthcoming, for a discussion of this work.) Horsley's casebook (1904) contains the following directions to patients about filling out the forms: "State very clearly on which side the movement commences, and whether it is confined to that side or spreads to the opposite side."

But despite these instructions, the forms became another source of uncertainty. Many of the forms which I saw were partially or vaguely filled out. There is much uncertainty in the responses recorded from patients, and many blanks left in the forms with phrases like : "Yes, no,?...," "Feels lethargic," "Twitches all over," "I don't know" or "felt poorly," etc. What <u>was</u> usually obvious and certain for both doctors and patients was what side the fit

started on. Spatial location of spasms was evident. As such it provided an obvious and later important link to localization theory.

Doctors thus turned to sequential classification and to links with physiological research on localization to deal with nosological uncertainty. They also tried sporadically to link pathological evidence of tumors or degeneration with conditions existing in patients before their death in order to find an anatomical correlation with disease states. They also tried to find exemplary cases of certain diseases to illustrate what a disease would "look like" in ideal form; these exemplary cases were often the ones written up in the medical journals or presented in classrooms (see e.g., notes from E. Jones, University College Hospital, 1899; Jones, 1959). Doctors thus filtered cases and screened out a lot of the uncertainty in the clinical setting.

The problem with turning to postmortem or physiological evidence in a clinical setting is that it was not immediately helpful with daily problems of either nosology or diagnosis. Classifications thus developed that were only provable after patients died or by inference from experimental or pathological data. The uncertainty itself, then, became temporally segmented within the workplace.

Patients would appear with fits or aphasia or

paralysis, and be processed into various diagnostic categories. These categories were then refined according to surgical, pathological, or perhaps experimental data. The refining process always took place with regard to some anatomical evidence. The immediate uncertainties about how to classify patients were thus jettisoned in the service of gaining anatomical information and certainty at a later time about something which was not visible in the work site.

An important bias arose from this resolution of uncertainty on the nosological level. Categories of localized function were developed which had invisible and presently unprovable anatomical referents, but which were legitimated by clinical practice. Thus, the case-by-case and symptom-by-symptom proximate handling of patients became integrated with ultimate physiological laws of localization of function. A sequence of events which had spatial correlates became linked with an invisible substratum, and remaining uncertainties were jettisoned into physiological or pathological lines of work.

Meanwhile, what did neurologists do with patients they couldn't classify? Let's turn to a discussion of diagnostic uncertainty and the responses to it.

2.1.2 Diagnostic Uncertainty

As noted above, diagnostic uncertainty was interactive with nosological uncertainty. Having unclear pictures of disease configurations made it difficult to classify patients. In addition, many of the symptoms of e.g., brain tumors, were those commonly found in any number of other diseases (headache, vomiting, dizziness). Thus, the sources of diagnostic uncertainty were the same as those which produced nosological uncertainty. In addition, there were technical and organizational interactions with diagnostic uncertainty. Tests to place patients in certain disease categories had not yet been developed at the beginning of this period (Hunter and Hurwitz, 1961). The familiar reflex test battery of modern neurology was in fact developed in canonical form at Queen Square in the last quarter of the nineteenth century. In part, it was an attempt to minimize diagnostic uncertainty.

But the major response from neurologists to diagnostic uncertainty was to look for what King (1982) calls a "pathognomic sign." This is the diagnostic equivalent of a magic bullet--an indisputable sign in a patient that will indicate one disease, only that disease and no other.

Neurologists thought that they had such a sign in <u>optic neuritis</u>. This is a condition where the optic nerve is inflamed. It is the <u>only</u> condition occuring within the brain that is visible on physical examination. Neurologists tried to use this as a sure sign of brain tumor. Other signs provide information that must then go through at least another transformation to be meaningful; a missing knee reflex, for example, can indicate several different conditions.

In 1871 Jackson said that optic neuritis was the commonest ophthalmological condition in cerebral disease (Spillane, 1981:354-58). But he had trouble distinguishing between types of swellings. By 1877, he was convinced that without an extensive knowledge of ophthalmology, a methodical investigation of diseases of the nervous system was not merely difficult but impossible. But by 1906, Horsley was writing on the complexity of optic neuritis as a symptom, and how optic discs change over the course of a disease (Spillane, 1981:412). Spillane describes Gowers' reaction to the problem:

> Optic neuritis was <u>the</u> ocular lesion in intracranial tumour; it was present in about four-fifths of cases. But it did not seem to be related to the site, size, or nature of the tumour, or even to its rate of growth. Optic neuritis was not a constantly associated condition in the history of a cerebral tumour; it was a transient event. A tumour might exist and cause symptoms for years before optic neuritis was produced....The atrophy left by optic neuritis...could not always be diagnosed with certainty (1981:360).

So optic neuritis did not work out as the simple pathognomic sign it was hoped to be, although this was frequently ignored by desperate clinicians looking for a <u>sine qua non</u> (see Hutchinson, 1878). The very search for these signs also points to confusion and the need for clear indicators. Nor were there other clear-cut diagnostic signs that would exclusively signal particular diseases. Neurologists were stuck with having to look at configurations of symptoms to diagnose patients--what was each individual patient's symptom configuration?

Another source of uncertainty was the spread of diseases of the nervous system. Nervous diseases of all kinds were rarely contained in one place in the nervous system. Nearly all diagnostic manuals of the time referred to this problem. Nor were the effects of such diseases contained in their effects (see e.g. Gowers, 1885; Bramwell, 1888). Horsley discussed this problem, and noted the response:

> When we are labouring with the difficulties of endeavoring to establish a differential diagnosis, we are likely to forget that the whole machine is in active operation while our attention may be drawn to one point only (Paget, 1919:180-81).

As with nosological uncertainty, one common response to diagnostic uncertainty was to postpone the diagnosis until the patient died or responded to some treatment that was linked solely with one kind of disease, such as mercury treatments for syphilis (Paget, 1919). Again, the uncertainty in the work site was segmented into present and future time arcs in order to be managed. This segmentation was obscured by triangulation of evidence. Clinical diagnoses would be supported by pathological or physiological evidence, but the mistakes or artifacts in the latter situation would not impinge on the clinical situation because the patient would already be dead.

Another response to diagnostic uncertainty was to treat each patient with a wide variety of therapies, hoping that something would succeed. Patients were massaged, electrified, given steam baths and mud plaster, potassium bromide and "metallotherapy" (Rawlings,1913:189 ;Urquhart, 1878; Bennett, 1878b). Buzzard records of a patient that: "Headaches have been extremely severe at times but are relieved at once by the application of two leeches behind the thorax" (National Hospital Records, Buzzard casebook, 1901).

The administration of treatments was organizationally divided-- separate doctors gave the electrical treatments, nurses and attendants gave massages, and so forth. Thus, neurologists became almost entirely concerned with <u>diagnostic categorization</u> and with finding exemplars for their teaching lectures and examples (Holmes, 1954), and with <u>localizing</u> tumors in order to prepare patients for surgery. This separation of care and diagnosis, and the mingling of diagnosis with localization, proved critical to the triangulation situation and to the increasing legitimacy of localization theory.

2.1.3 <u>Organizational Uncertainty</u>

Organizational uncertainty arose when information was lost or trapped due to the division of labor at the hospital, between doctors and patient or between doctors and surgeons.

While the specialization at Queen Square created opportunity for research, and reduced some kinds of uncertainty, it created others, especially in the relationships between specialists and nonspecialists. Nonspecialists often did not know how to diagnose nervous diseases--remember that even the specialists were developing their canon of tests during this period--and patients would often be referred to Queen Square only in the advanced stages of their diseases, after more subtle symptoms had been missed, or after they had undergone ineffective treatments from nonspecialists. One casebook of Ferrier's from 1894 records that a patient with a cerebral tumor had been discharged from the army several months before his admission to Queen Square with a diagnosis of "debility from climate and military service" (National Hospital Records, 1894).

Relationships between doctors and surgeons were also a source of uncertainty, as well as some antagonism. During this period, surgeons were fighting for equality with doctors. This movement proceeded throughout the turn of the century. While surgeons had achieved nominal equality with doctors in professional associations by the 1880s, there were still large organizational imbalances of power.

This was graphically reflected in the records from the hospital at Queen Square. I had some trouble locating surgeon Victor Horsley's casebooks, and was told that surgical cases were maintained separately from medical cases. After searching through the entire hospital, I finally found that Horsley's case notes (like nurses', attendants', and house physicians' notes) had been appended to those of the attending physician in cases requiring brain or nervous system operations. The names of the head physicians were stamped in gold on the leather-bound volumes, and Horsley's name was only to be found in his signature at the bottom of the surgical reports.

A series of letters between Gowers (a physician at Queen Square) and Horsley further illustrates this division. Gowers had trouble even contacting Horsley to get him to operate on a patient, indicating communication difficulties between surgeons and doctors. One patient was getting much worse because Gowers couldn't find Horsley to consult him about operating. The trajectory management problems are evident in this letter from Gowers to Horsley in 1894 (Lyons, 1965:263-64):

> possible syphilis and urgency to act <u>as soon</u> as <u>ever the absence</u> of result from treatment is <u>just</u> definite enough. I hear you think it is very likely a large tumour. I suppose you will do the second part, here, as soon as is proper. Can you tell me when?

In this same series of letters, optic neuritis is alluded to by Gowers, who also mentions the unacceptability of brain surgery to most of the medical and much of the patient community (Lyons, 1965:263):

> No headache no optic neuritis. What more wd you want before operating? Of course op.n. would suffice but one can't propose it yet to friends though I have to a cousin doctor (no date given; 1894?, spelling as in original).

Gowers later also mentions that he's having trouble deciding how to present "the potentiality of surgery" in a lecture.

This organizational uncertainty was exacerbated by poor relationships between doctors and administration at Queen Square. Rawlings (1913), former administrator at Queen Square during the period when Jackson, Ferrier, Gowers and Horsley were associated with it, and Holmes (1954) former physician at Queen Square in a later period, both state that there was continual friction between Rawlings and the board of governors, and the physicians at the hospital.

One bone of contention was control over admissions. Physicians wanted exclusive rights over admission, partly in order to screen patients and admit only acute and interesting ones. Hospital administrators and governors wanted to be able to admit patients (presumably their servants or tradespersons known to them) at will.

This was a source of uncertainty for physicians.

They never knew when there would be an empty bed, or with what illnesses they would be confronted. They resolved it partly by going on strike (in 1901, after many disputes, see Holmes, 1954 and Rawlings, 1913), and partly by using an emergency admission provision to admit their own interesting cases. Eventually, the physicians did win control over the hospital administration and get representation on the board.

Organizational uncertainty between physicians and surgeons was handled in part by separating patients into those with clearly operable tumors and those without. Physicians had to submit their diagnoses to surgeons for review for possible brain surgery (Lyons, 1965). The physicians then helped surgeons localize the tumor. Other patients were cared for exclusively by the physicians and support staff.

Thus, localizationist diagnoses were applied to surgical patients, strengthening localization theory and allying surgeons with neurological theorists. Since there were relatively few operations during the early years of neurosurgery, and the number only gradually built up (until at the turn of the century they were doing 100 per year), the organizational machinery got set up fairly early on.

2.1.4 Technical Uncertainty

Technical uncertainty in neurology arose from inadequate (by scientific standards of the time) measurement instruments. In part neurologists attempted to solve these problems intersectionally--that is, by bringing in technical experts who could properly apply instruments and get reliable diagnostic and nosological information. Semon, an ear, nose, and throat doctor, was occasionally brought in to Queen Square to examine aphasic patients to see if they had organic diseases of the larynx. Various ophthalmologists were consulted by Queen Square physicians on a regular basis, and during the latter part of the period under examination, Queen Square physicians began hiring ophthalmologists to draw them diagrams of the optic discs that they could use in diagnosis.

Neurologists at Queen Square did not develop the focus on technical skill improvement that surgeons did. Where surgeons focussed almost exclusively on technical improvement of brain surgery, neurologists chose instead to hire others to improve their own technical work. Technical uncertainty in neurology was rarely addressed by the line of work as a whole in terms of doing better neurological exams or developing better test equipment. Rather, getting better configurations and sorting out responses into better categories was the focus.

This overall <u>taxonomical</u> focus left the technical work to those concerned with anatomy or a search for structure: physiology, surgery, or pathology. This segmentation framed some of the later development in localization theory: neurology was equated with locating the "mind" and classifying its disturbances in a localized taxonomy. The taxonomy was then verified by other lines of work, with anatomical bases. Later philosophical commitments to parallelism were built into the organization of work and the management of uncertainties in neurology and other lines of work. This will be discussed in more detail in Chapter 5. For now, it is important to remember that a division between methods and lines of work associated with detecting problems in the "mind" and those employing manual/anatomical skills was institutionalized in a set of heuristics and organizational practices.

2.2 Surgery

Surgeons dealt with three kinds of uncertainty: is there a tumor?; if there is a tumor, where is it located?; and how can I get to it and remove it without killing the patient in the process? The situation is graphically summed up by Cushing (1905):

> the neurologist spends days or weeks in working out the presumable location and nature of, let us say, a cerebral tumor. An operator is called in; he has little knowledge of maladies of this nature and less interest in them, but is willing to undertake the exploration. The supposed site of the growth is marked out for him on the scalp by the neurologist;

> and he proceeds to trephine. The dura is opened hesitatingly; the cortex is exposed, and too often no tumor is found. The operator's interest ceases with the exploration, and for the patient the common sequel is a hernia, a fungus cerebri, meningitis and death (p. 78).

The first two sets of questions--is there a tumor and where is it located-- were shared by surgeons and neurologists. The existence of a tumor involved both nosology and diagnosis. But as the quote from Cushing indicates, surgical concerns were only partly clinical. Locating tumors was also an anatomical research issue, as the discussion below illustrates.

One major complication faced by both surgeons and neurologists was that syphilis and brain tumors exhibited many of the same symptoms--paralysis, epileptic-like fits, blindness.

Conservative doctors wanted <u>all</u> patients exhibiting these symptoms to undergo a prophylactic course of treatment for syphilis <u>before</u> the diagnosis of brain tumor would even be considered. This meant a wait of at least six weeks, sometimes longer, for patients who could be getting much worse with a brain tumor. (Bear in mind that the Wasserman test for syphilis was not invented until 1908, so there was really no way to know if patients had syphilis unless they reacted well to the course of treatments.)

This very often meant that patients with fast-growing brain tumors would not see surgeons until the tumors had progressed very far indeed, drastically reducing the chances for surgical success, and adding to the uncertainty of the clinical course. Both surgeons and localizationist neurologists bitterly fought this restriction (Cushing, 1905; Horsley, 1906; de Watteville, 1881) but it continued to interfere with surgical success until the Wasserman reaction was discovered (Cushing, 1910).

Once it was decided that a patient had a brain tumor, neurologists tried to localize it. Uncertainties arose as localizations failed, as apparently they often did. Von Bergmann, in 1889, "was able to collect seven cases only in which a brain tumor had been diagnosed by neurological evidence, localized, and removed by operation" (Horrax, 1952: 56). Of these patients, only three had lived.

In my own examination of the casebooks at Queen Square, I came across a tracing that Horsley had made of his incision through the skull of a tumor patient. The drawing shows four separate openings, made successively in the operation as the tumor was repeatedly not found (see Figure 1). Ultimately, the hole encompassed nearly 25% of the entire skull area (Ferrier casebook, 1886; see also Beevor casebook, 1894 for a similar case where no tumor was removed but a large amount of bone was removed in the search for it).

Gowers and Horsley collaborated on the first opera-

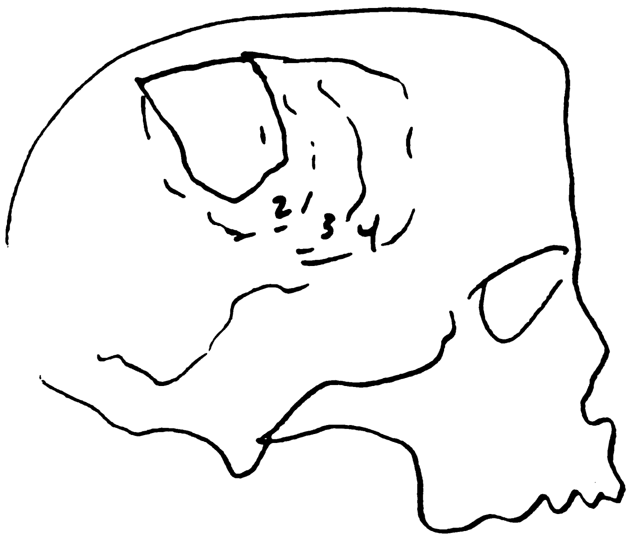


Figure 1: Progressive holes cut for tumor. From Horsley's sketch, in Ferrier's casebook (National Hospital Records, 1886).

tion for removal of a spinal tumor. Gowers performed the neurological localization tests. Yet when Horsley opened the spine at the lamina which Gowers had indicated, he found no tumor. He had to remove several more sections of the man's spine before he found the tumor. Yet this was never defined as a "mistake"--because the patient was cured (and after many years of suffering during which he had seen many doctors all over Europe). The clinical cure here was used to evaluate the success of the operation. Knapp and Bradford, similarly, report the death of a patient after 45 minutes (in 1889), but again, the operation was a "success," because the tumor was located--a different criterion for success.

A contemporary of Horsley's similarly reports that:

> It would be quite wrong to convey the impression that all Horsley's operations were successes, many were only partial successes, and some considerably less than that. But this was a stage, doubtless, through which the surgery of the nervous system had to pass. The problems were to localise a tumour accurately by clinical means, to verify that diagnosis by operation, remove the tumour if possible. Nobody else could have done any better than Horsley...and when one had seen a man carried into Queen Square comatose, had seen Horsley remove a tumour, and one had afterwards met the man travelling on the underground railway in perfectly good health, in spite of a large lacuna in the vault of his skull, one had to admit that a very remarkable thing had been done (Jones, 1946:153).

We recognize in this description the development of vocabularies of realism in a profession, a sociological phenomenon described by Bucher and Stelling (1973). Vocabularies of realism are developed by members of a profession to account for such phenomena as "the patient died but the operation was a success." They are ways of evaluating results of work on the basis of the process, not the product. There is a focus on "doing one's best" and "recognition of limitation," sometimes with a deliberate ignoring of the outcome of a given procedure. For the psychiatrists that Bucher and Stelling studied, uncertainty about outcome or proper treatment was sometimes managed under the rubric of "untreatable patients," but not untreatable disease.

The development of this kind of vocabulary in a profession points not only to the attempt of the profession to keep its evaluation internal, but also to a lack of alternative solutions to the problems it is addressing. Even if the professionals in question are filling the needs "badly" from the point of view of the patient, the key phrase here, as in the quote above, is that "nobody else could have done any better." The comatose patient had nowhere else to turn, and Horsley's failure was seen not as a failure but as an attempt.

But the needs for brain surgery were not immediately apparent to the medical profession as a whole. Surgeons and localizationist neurologists first had to convince the medical profession that brain surgery was possible, and second, that once possible, it was a wise idea.

The profession was convinced partly because there were no alternatives for tumor patients. Neurologists had not developed effective treatments for such diseases. They had concentrated so on diagnostic classification systems, and on supporting the development of physiological theories, that they really had little to offer brain tumor patients (Cushing, 1913). As Horsley notes in 1906:

> As in all special branches of medicine and surgery which are in a process of evolution, it is not easy to assign credit or blame when the course of treatment pursued is respectively successful or unsuccessful; but so long as our powers of diagnosis remain as imperfect as they are so long will the vulgar error of regarding surgical treatment as a dernier ressort be committed. This question, namely, When should medicinal treatment be given up and operative treatment substituted? has been raised again and again and hotly discussed in connextion with many diseases...(p. 411).

And Cushing (1905) asks:

> For what eager student of medicine can face without dismay the "poverty of therapy" that characterize the present day, and which is emphasized more especially in the neurological clinic, which stands largely on the therapeutic tripod of iodine, bromine and electricity (p. 78).

The intersection of neurology and surgery into neurosurgery gave neurology claim to a unique treatment for desperate, hitherto-untreatable cases, and gave surgeons a theoretical base for localizationist therapies. The rising class of surgeons thus provided an important commodity: vital anatomical information for localizationist theorists. They used localization theory to gain professional prominence (see Cushing, 1910; 1920; 1935 for a review of these histories).

The development of "vocabularies of realism" reflected the growing commitment of surgeons as part of the localization enterprise (as basic research concerns began to buttress clinical ones). It also reflected their upward professional mobilization.

Brain surgery was used to validate localization theory, initially not because the localizations were successful, but because the possibility of success was so important. After the first brain operation, Crichton-Browne wrote a letter to the Times expressing the following sentiment:

> Sir, - While the Bishop of Oxford and Professor Ruskin were, on somewhat intangible grounds, denouncing vivisection at Oxford last Tuesday afternoon, there sat at one of the windows of the Hospital for Epilepsy and Paralysis, in Regent's Park, in an invalid chair, propped up with pillows, pale and careworn, but with a hopeful smile on his face, a man who could have spoken a really pertinent word upon the subject, and told the right rev. prelate and great art critic that he owed his life, and his wife and children their rescue from bereavement and penury, to some of these experiments on living animals which they so roundly condemned. The case of this man has been watched with intense interest by the medical profession, for it is of a unique description, and inaugurated a new era in cerebral surgery (Spillane, 1981:398).

Brain surgery was used thus to fight against antivivisectionists, although the patient in this case only lived for 28 days and then died of meningitis (Spillane, 1981:398).

The uncertainty and difficulties of the actual operative procedures are evident from the descriptions of operations and their sequelae. Some important points to remember here are:

--Tumors didn't fit maps of the brain; they didn't grow in neat patterns. They often expanded beyond the "localized' boundaries they were "supposed" to occupy according to neurological theories;

--There were many unpredictable side effects of tumors, including pressure, scar tissue, veins which fed the tumor affected the rest of the brain;

--Infections of all kinds were very common, including some which were unique to brain operations and which surgeons did not know how to control (e.g., brain fungus and meningitis);

--There were very few surgeons doing this kind of work, far fewer even than neurologists doing similar work (Jefferson, 1960). Horsley was for years the only surgeon at Queen Square (from 1886 to 1891).

--Surgeons also had difficulty obtaining animals to practice operations on because of antivivisection restrictions.

--Physicians had control of patient care, and surgeons had to bargain with them to obtain cases (Lyons, 1965).

One major class of responses from surgeons to these multifarious uncertainties was to concentrate on <u>technical</u>

improvements and ignore physiological or neurological failures to localize functions.

What neurologists needed from surgeons was to be able to take out tumors; what physiologists needed from surgeons was validation of their anatomical predictions, supplied in the form of data; what pathologists needed from them was simply raw material in the form of tumors or cadavers. The major uncertainties threatening surgeons thus became converted into technical problems, especially those problems which might interfere with their being able to supply their various coworkers.

By concentrating on technical improvements (see e.g., Rogers, 1930, Ballance, 1922 or MacCormac, 1880 for a summary of the work done in this line), surgeons made a very important tacit bargain with other localizationists. They agreed to accept both neurological and physiological results and black box those research processes. Evidence from the surgical realm was reported only procedurally and within a case-by-case framework. It was also reported anatomically. Thus, surgical work became a key proof for neurology, which lacked anatomical referents of its own.

Surgery's reliability was vested in its technology, not in its clinical or anatomical successes. Yet its data was picked up and used by physiologists, pathologists and neurologists to validate both clinical and anatomical work in those realms. Here the mechanisms of professional autonomy in surgery and the other lines of work operated to prevent outside evaluation. Through political organization, surgeons fought for and gained control of the evaluation of their work--both clinical and anatomical. As I noted in a previous chapter, this occurred within a context of simultaneous specialization and professionalization. Localization theory became the research tool which surgeons and neurologists used to legitimate their professional aspirations. At the same time, the dynamics of professionalization helped legitimate the rise of the theory.

With the success of the perspective, late in the 19th century, localized functional atlases of the brain came to be standard tools for surgeons and pathologists. This is in contrast with earlier models of the brain. During the time of Gall, a major source of objection to localization had been that the cerebral hemispheres exhibited no outward differences in shape or region (Walker, 1957; Jefferson, 1955), and it was hard to imagine functional differentiation with no outward differences in appearance. Surgeons essentially went into the brain without good maps to guide them according to the localizationist principles they espoused. By 1885, standard functional representations of the brain in the form of localizationist anatomical atlases had been developed. The lack of outward differences had ceased to be important.

As neurologists responded to uncertainty by screening patients and developing localized diagnostic taxonomies; and as surgeons responded by focussing on technical improvement, a _de facto_ agreement was instituted about _where_ dysfunctions were located and what they meant. The skull itself became an important boundary when discussing behavioral problems or dysfunctions. The categories of localization were seen as physically bounded by the skull.

This boundary became increasingly robust as surgeons became critical contributors to localization theory. The implications of this boundary are discussed in detail in Chapter 5. Its location was important for the development of psychology and neuropsychiatry, because it determined the heuristics chosen to investigate the "mind." Physical/anatomical heuristics such as tumor location and boundaries were widely adopted to discuss psychological results.

3. Basic Research

Pathology [5] and physiology are both basic research specialties in that they seek to answer general questions about nature and not to solve specific clinical problems. As such, they are distinguished from clinical work along several dimensions:

- Unit of Analysis. Clinical work is primarily geared toward individual problems and their solution; basic research, toward aggregates or statistical units (Freidson, 1970b: 164).

- Level of abstraction. "The practicing physician may use general principles to deal with concrete problems; the scientist typically investigates concrete phenomena in order to test, elaborate, or arrive at general principles." (Freidson, 1970b:163) The use and direction of abstraction are different, then, in the two enterprises.

- Temporal Orientation. Clinical work is oriented toward immediate results--the patient must be cured now, before the results of the basic research may be fully "in." Basic research, on the other hand, is geared toward posing and solving problems which will be a thorough description of a general situation. The time horizon is remote for any individual case. [6]

- Orientation toward anomalies. Anomalies are interruptions to the routine performance of work, and include accidents, mistakes, artifacts and discoveries (Star and Gerson, forthcoming a and b). Clinical anomalies are those which inhibit or prohibit the administration of therapies, diagnostic procedures, or patient responses to therapies. Their management thus consists primarily in insulating and eradicating the individual-instance mistake or accident. The hallmark of anomaly management in clinical work is found under the rubric "complication."

Basic research anomalies are interruptions in a chain of inference or experimental procedure. They thus inhibit or prohibit smooth inference from a set of procedures to a set of generalized results. Their management consists primarily in determining the impact of the anomaly on the chain of inference: is it an artifact, or a discovery, and what is its significance? Where clinical science sees complications, basic research sees artifacts which may impeach the validity of findings if not handled correctly.

Because clinical and basic lines of work have different orientations, they have developed different conventions to manage uncertainties. Clinical lines of work, concentrate on finding what works to cure, including diagnostic work while basic scientists concentrate on understanding mechanisms.[7]

3.1 Pathology

Pathological work consisted mostly of dissecting deceased patients to find and describe tumors or lesions, then classifying those tumors or lesions after viewing them under the microscope. The primary uncertainties in pathology were topological and organizational.

By "topological" I mean the appearance of the surface of the brain. Like surgeons, pathologists faced a very delicate structure whose divisions were totally unapparent in a naive sense. For example, Lewis (1880), in an article in Brain, called for histologists (those who study cells, an overlapping category at the time with pathology) to study not just cells but the gross appearance of the brain. They should look, he said, at "the manifold appearances which constitute an unbroken whole" (p. 315). Medical students, he said, were often vague and indefinite about the changes that they saw. He gave step-by-step directions in this article, and tried to instruct medical students and doctors in understanding the topographical outlines of the brain.

For the most part Lewis' call went unheard by pathologists who contributed data to the localizationists. Rather, they responded to topographical uncertainty by ignoring it or leaving it to surgeons. They focussed instead on the microscopical qualities of individual brain tumors.

The organizational uncertainties which pathologists faced also pushed them to focus on the minute details of tumors rather than on the gross characteristics of brains or comparative/epidemiological approaches. Pathologists had to work quickly and often even furtively; they had a difficult time obtaining materials.

It was very difficult at first for pathologists to get permission to do postmortems, and once permission was given, to do careful, leisurely work.[8] Horsley, for example, had to make an alliance with the porter at the mortuary of the hospital where he had his first job, in the early 1880s. He got--even stole--organs from dead bodies, with the compliance of this porter (Lyons, 1966). There was no other way to get a steady supply of bodies.

Yeo, a physician at Queen Square, remarks (1878) that the postmortem examination of the brain of a tumor patient "had to be performed hastily, and under difficulties which precluded the possibility of a detailed examination of that organ being made." (p. 275) In this same case report, he says that he had difficulty convincing the family to give permission for the patient to have a brain operation because the patient kept going into remission.

Here we see clinical and basic uncertainties and difficulties overlapping. After the patient died from the operation for which the family was reluctant to give permission in the first place, they were then reluctant to give permission for a post mortem.

Even where permission was received for a post mortem, there were difficulties. Atkins (1878) notes the following:

> The spinal cord presented nothing abnormal to the naked eye. Portions of the brain and spinal cord were placed aside for future microscopical examination, but unfortunately got spoiled in preparing, during my absence from home, therefore I regret that, so far as the microscopical examination could have thrown further light on the case, the report is imperfect.

Bennett, in the same year, notes the great difficulty of getting the brain of a dead patient who had had a cerebral tumor, which was then accidentally thrown away (1878a). Ferrier, in an 1879 article in Brain, also describes difficulties with getting an adequate postmortem supply:

> I freely admit that in the absence of postmortem confirmation these various facts do not altogether satisfactorily establish a correspondence between the locality of the percussion pain and the cerebral lesion, I have not, however, had an opportunity of post mortem examination in cases which I have carefully examined this way (pp. 482-83).

The organizational and topographical uncertainties presented by pathological work were resolved by pathologists in just about the only way possible. They took brains and tumors when they could, preserved them as well as they could, and focussed on minute details of individual cases, especially on minute descriptions of tumors and lesions.

As noted above, this approach established a set of conventions which relied on microscopic examination of the brain, within the individual body. The rest of the body, or environmental conditions, were black-boxed with respect to the focus on the tumor.

While one ostensive purpose of postmortem examination was to verify the neurological localization of cerebral and other nervous system diseases, this proof became in fact an incidental part of pathological work. Each line of work in effect made the assumption that postmortem findings were consistently correlated with neurological localization. In fact the lack of topographical emphasis and the frequent inability to correlate tumor location with location predicted by neurologists made validation moot. Instead, there was a declaration that such correlations were being made. Failures to correlate were ignored.

3.2 Physiology

The physiological work which I am discussing here consisted primarily of trying to precisely recreate in animals the destruction or malfunctions created by disease in humans, or applying electricity to the animal's exposed cortex to try to chart pathways and reactions. While there were many complicated intersections and cross-currents in physiology in the nineteenth century which impinged upon this work, a full discussion of the field is beyond the scope of this essay. Therefore I am limiting the discussion to those aspects of physiology which were directly used to support localization theory. This work was often done by the same localizationist neurologists who did clinical work, or by neurosurgeons like Horsley.

It is important to remember that the clinical work came first in the localizationist enterprise, and that the physiological work was used to try to give it a basic science foundation or justification. The central task for physiologists, was to <u>match</u> the clinical symptoms and diagnostic taxonomies with their own observations; to <u>recreate</u> the conditions of disease in the nervous system in order to more precisely observe their genesis and effects.

The epistemological foundations of physiological research, as noted above, are very different from those of clinical science. Physiologists were looking for <u>universals</u>, not particulars. Furthermore, they were looking for universals which could be converted into clinical handbooks and anatomical atlases. These would be static, enduring, practically applicable representations of the nervous system and brain that would provide an objective referent for diagnostic categorization and treatment as well as medical training.

The problems of performing these tasks originally seemed pretty straightforward to researchers. Ferrier scrawled a quote from Magendie in one of his notebooks from 1874 which reads:

> Thanks to the progress of good sense, we prefer the experiment to the most ingenious of "systems"; the most simple truth appears more beautiful than all the glorious fantasies of the imagination (MS246/4, n.p.). [9]

Finding these simple truths appeared to be a matter of recreating the one-to-one relationship between area and function apparently found in the human brain: delete area, observe misfunction, validate existence of area. Of course both physiologists and clinicians very quickly found that such a simple approach did not hold either in applying techniques or observing results. This section is concerned with how they managed the uncertainties and anomalies involved in this work.

As with the other lines of work discussed here, there were several sources of uncertainty in physiological work. They included technical or manual skills management, resolution or matching difficulties, and organizational uncertainties.

3.2.1 Technical Sources of Uncertainty

The experimental subjects employed by Ferrier, Horsley, and other physiologists included dogs, rabbits, monkeys, birds, even jackals (Ferrier notebooks, MS/246 1-19; Beevor and Horsley, 1890; 1894). Technical uncertainties arose both from characteristics of the subjects themselves and from difficulties with equipment or procedures, including lack of standardized measures.

The problems and uncertainties of working with "reacting subjects," particularly with monkeys, are vividly conveyed in Ferrier's laboratory notebooks. The notes themselves were often written in an obviously hasty, shaky hand, trying to record minute-by-minute events in the laboratory; the pages are spattered with blood stains. Ferrier noted that the monkeys were often "mischievious" or affectionate, constantly trying to run away from him, climb up his pants legs, bite or scratch. In Ferrier's words:

> Apparently monkey disinclined to move. Could see somewhat as he when making a push away from being pursued did not knock except occasionally...Difficult to say whether right extended or not as being disinclined to move it--at any rate we had few methods of testing...After this we tried hard to get the bandage off the left eye. Was very unwilling to move at all. When kicked would run against anything.
>
> Taken into the other room. Sat still with head down. Would not respond to when called. Gave him a piece of cracker and he put it in his mouth. Took him back into laboratory. Got quite still and grunted or made a rush anywhere when distracted (MS246/5, Jan. 5, 1875).

In addition to being reactive in a behavioral sense, the monkeys and other laboratory animals were also reactive physiologically. Damage to subjects, including operative complications, was a major problem for Ferrier. Again and again the notebooks record frequent accidental deaths from hemorrhage or chloroform overdose. This entry is typical:

> Monkey (macaque)--operation commenced 2:30 pm. Animal died 7 pm before any experiments could be made.

One set of procedures often used by Ferrier--which in fact comprise his exemplary experiments--was to surgically open the skull of an animal, then to systematically apply electrical current to minute areas of the cortex. He then recorded muscle movements which followed the application of the current. The application points were carefully numbered and charted. Ferrier would start at point 1, then proceed to point 32, applying electrical current to each small area.

There were many problems and uncertainties involved with this part of the procedure, if indeed the animal lived through the surgical opening of the skull. Identifying the various points accurately; accounting for individual anatomical variations in the animals' cortices, and, most important, controlling the amount and kind of electrical current applied were major sources of artifacts and confusions for experimenters (Duret, 1878; Dodds, 1877-78; Burdon-Sanderson, 1873-74).

The areas that Ferrier tried to identify were pre-numbered on charts printed up for his purposes, or traced and retraced from old schematic drawings of animal brains. The application of this system had some problems, as Ferrier's notes reveal:

(5) elevation of left--upper eyelid (?). This was going on before spasmodically but seemed intensified by electrodes. This point may be (4) (MS/246/1, June 1873-July 1873).

The masseter could not be got to contract again. (ibid, June 27)

No results could be obtained, owing to some bleeding having taken place. (July 4)

No application could be made nor could the electrolisation be made to be localised. (July 12)

This movement was very difficult to analyse as the brain very speedily lost its excitability. (July 22)

Hard to distinguish where the electrodes were due to the bleeding. (MS 246/2, August 8)

Position of the electrodes..not capable of being quite accurately made out. (MS 246/4, August, 1873)

Ferrier was not the only researcher facing these uncertainties. Rabagliati (1879), in describing the work of Munk, says that Munk "has not been able definitely to localize the representations, however, because all his animals died of most acute meningitis in his efforts to remove the surrounding centres" (pp. 537-38).

The researchers themselves were both acutely aware of and concerned with the uncertainties that they faced. At the same time, they were committed to the localizationist mandate and confident that once the technical difficulties were solved the centers they sought would appear in robust form. [10] A criticism of Ferrier by two Italian localizationists, Luciani and Tamburini, says that,

> it is impossible to have exactly similar conditions present, no account being taken of the amount of the haemorrhage, of the amount of injury suffered, of the exhaustion of the two animals compared, of the narcosis, and of the precise amount of the electrisation (Rabagliati, 1878b: 532).

Yet repeatedly one runs across the phrase "failure to localize," or descriptions like the following:

> Had a different system of partitioning the brain to that which I adopted been pursued, perhaps even more striking results would have been obtained, and certainly more trustworthy data would have been collected bearing upon the researches of Hitzig and Ferrier (Crichton-Browne, 1879:65).

> In spite of innumerable attempts to degrade the grey matter of the brain and to exclude it from all share in the results, it may be regarded as established that its definite groups of cells yield definite effects always constant under definite stimulation of whatever nature (Ferrier, 1878b: 131).

Despite technical problems and uncertainties, localizationists vested certainty in the ultimate validity of localization. In Ferrier's remarks we also see a claim to consistent results that are contradicted by the data in his notebooks.

It was possible for localizationist physiologists to continue to ignore failures of standardization or anomalies in their data for two reasons. First, they had the backing of sponsors who also agreed to overlook the anomalies and even assisted in the process. Second, clinicians attributed a great deal of certainty to physiology.

One of Ferrier's sponsors was the Royal Society, which gave him a small grant to conduct his localization experiments. When he submitted his original report in 1873-74 to the Society, referees had taken care to check the numbered regions Ferrier claimed for given functions, and disagreed with his placement of some regions. Rolleston's report on the experiments contains the following passage:

> I have however to say, with reference to certain statements made on pages 30-32 of the paper, that I do not think Dr. Ferrier is quite right in saying that the parts of the brain indicated by him with the circles 9 and 10 as stated to have called forth movements of the tongue and mouth are really "the homologues of this region in man which is the seat of lesion in the disease known as aphasia." The seat of this lesion I believe is seated some distance in front of this locality indicated in Dr. Ferrier's figure ii by the circles 9 and 10 . If Dr. Ferrier will refer to the figure given by Dr. Sanders in the Edinburgh Medical found for March 1866 of the brain of an aphasic patient I think he will see that the facts of the case are against the view he has taken of the homologies of the two sets of Brains, the human, to wit, and the simious. I have not before me any figures of the brains of the aphasic patients upon which Broca forward his pathological theory with which his name and that of the condition are now connected; but in the French nomenclature of the convolution called now Broca's convolution as formerly the "third stage of the frontal lobe" is never described as reaching further back than the first ascending convolution. It cannot therefore be homologous at any rate with the commencement of the second upon which however Dr. Ferrier's circle 10 is placed (Report of the Royal Society, May 12, 1874).

The referee goes on to suggest that Ferrier change the location of the number in question on the diagram. He also notes that his earlier paper reporting work done at West Riding had circles and numbers placed differently, and suggests that he standardize the two sets of drawings. Note that the suggestion is to standardize the diagrams, not to redo the experiments in what might have been seen as an anomalous or inconsistent finding. Ferrier's sponsors backed his procedures and localizationist outlook to such an extent that they were willing to overlook data which did not fit.

Another important source of technical uncertainty was the fact that electrical current spreads and cannot be contained in one area. Many of the muscle-movement results which Ferrier obtained were attributed at first to diffusion of current over the surface of the brain or into deeper structures. Critics said that the movement which he obtained had nothing to do with functional attributes of the cortex, but rather were artifacts of the procedures that he used (see. e.g., Burdon-Sanderson, 1873-74; Dodds, 1877-78; Rabagliati, 1879). A similar "spreading" effect was observed in the surgical procedures where infection and pus or hemmorhage increased the size of the areas under investigation in ways that could not be controlled or accounted for.

Some technical difficulties were related to observing the animals properly, or "getting at" the effects of the lesions or electrical stimulation. Ferrier's notebooks record his attempts to observe a monkey with one hemisphere removed. The animal falls down, occasionally to the right and occasionally to the left. "Some also down. The exact points not defined..." (MS246/4, Dec. 1874).

He speaks of animals exhausted, in states of stupor, unable to respond at all to any stimulus. He pokes and prods the animals, gives them smelling salts and electrical shocks, bangs on the water pipes to see if they react. An experiment in 1879 records an operation done with Yeo (MS 246/7, Oct. 1879) where Ferrier removed a portion of the left occipital lobe. Later the monkey is noticed limping with the right side, but Ferrier is not "sure if existed before."

Yeo, a collaborator of Ferrier's, expressed a similar problem in a dog he observed. Yeo, speaking at the 1881 Medical Congress, says that the dog had much of its cortex removed. Yeo disagreed with this claim, and in the process of debate revealed much about the difficulties of ablation experiments (for a discussion of the 1881 conference, see Chapter 4):

> The general behaviour of the dog, together with the difficulties I have experienced in exposing and completely removing certain parts of the cerebral cortex of other animals, makes me rather sceptical as to the exact extent of the lesion. One often thinks one has destroyed much more than really turns out to be the case at the autopsy. It may seem extremely bold on my part, but I can not refrain from expressing my conviction that much of this dog's cerebral cortex will be found intact (MacCormac, 1881: 239).

Some of the technical difficulties came from the lack of standardized procedures for recording experiments, the lack of standard equipment, and the early and experimental stage of surgical skills. In 1873, when these initial experiments were done, antisepsis was not at all standardized even for humans. In addition, there was a prevalent belief that animals were more resistant to infection than human, and that precautions need not be taken for animal operations. Where to place electrodes, how to make incisions, how to create lesions and then control their boundaries, were all new problems for Ferrier and other physiologists at the time.

3.2.2 Uncertainties of Resolution or Matching

Another set of problems interlaced with technical difficulties were uncertainties arising from uncertain observations or problems of resolution. Like the problems of nosology in clinical work, these are problems of classification. The mandate of this line of work was to discover the physiological correlates of certain diseases, and to reproduce human disease conditions in healthy animals. Problems arose from uncertainty about what a "typical" disease looked like; to what extent ablations or lesions were like tumors or disease lesions in humans; how individual differences between animals of the same species or between species affected the validity of results. The autopsy report on the brain of the same dog observed by

Yeo, above, mentions this as a major problem:

> a great deal remains to be done before the brains of different dogs can be at all accurately divided into corresponding areas; for it is clear that some variation in the real position of the fissures might still take place...In any case the division of the cortex into areas is only approximate for it is impossible to say which of the boundary convolutions the cortex at the bottom of the fissure belongs, if indeed it does belong to one more than to the other; this can only be done when we find a difference in histological structure. But what I wish to point out is that if the apparently corresponding fissures do not run along corresponding lines of the cortex, experiments made on the functions of the parts of the cortex in one dog afford very inadequate data for mapping out the cortex of the brains of other dogs, and _a fortiori_ of mapping out the cortex of the brains of other animals (Klein, Langley, and Schafer, 1883-84: 250).

The resolution to this class of uncertainty was to ignore it. In 1876 Ferrier published _The Functions of the Brain_ and directly transposed localized maps of dog brains onto human brains, transferring the numbered regions from the one to the other. There was little or no objection to this; I found one comment by Gowers (1888:170) which stated that the results could not be generalized [11], but little criticism elsewhere. Similarly, individual differences between animals were simply ignored.

I have no satisfactory explanation as to why the problems of transposing animal results to humans, and human and animal interspecies differences were so easily ignored. Clearly, paying attention to those differences would have required changing the very nature of the physiological and medical work underway. The benefits of universal invariant principles in a system so filled with uncertainty were large enough to prevent effective objections to the practices.

3.2.3 Organizational Sources of Uncertainty

Physiological work also had organizational sources of uncertainty. The major uncertainties here were lack of independent funding or institutional security; and the opposition of the antivivisectionists, discussed in the previous chapter.

Physiologists responded to the lack of institutional security or independent funding by sticking to research that could be justified in clinical terms. Geison (1978) notes that, due to this dependence, the utilitarian emphasis in medical training also became a "yoke" for physiology:

> When taught at all in the London hospital schools, physiology was taught in conjunction with anatomy or from an anatomical point of view, and almost without exception by a medical practitioner recruited from the hospital staff....In their aims and organization, then, the London hospital schools fostered a utilitarian and correspondingly anatomical conception of physiology. In no way did they provide an institutional setting conducive to the liberation of physiology from anatomy (1978:26-27).

Nor did the London hospital schools provide a basis for the liberation of physiology from medicine. The triangulation of results from medicine and physiology was important to physiology's continued survival through the turn of the century. Within this situation, physiologists had to develop saleable "commodities." Their client was the medical community.

Physiologists did struggle to gain a separate institutional base from medicine. This is another important but poorly-researched area in the history of physiology. What kinds of questions were supported and which squelched by this dependence on medicine? At least in the case of localization theory, we can see that institutional precariousness was met with a conservative, anatomically-oriented approach geared toward clinical validation.

The second source of organizational uncertainty came from antivivisection. Michael Foster vividly describes the situation in his 1881 inaugural address to the Physiology Section of the International Medical Congress in London:

> Our science has been made the subject of what the highest legal authority stated in the House of Lords to be a <u>penal act</u>. We are liable at any moment in our inquiries to be arrested by legal prohibitions, we are hampered by licenses and certificates. When we enter upon any research we do not know how far we may go before we have to crave permission to proceed, laying bare our immature ideas before those who are, in our humble opinion, unfit to judge them; and we often find our suit refused...For surely we are all agreed that experiment is the chief weapon with which we can fight against the powers of darkness of the mysteries of life (MacCormac, 1881: 218).

The response of physiologists, already noted, was to use clinical findings and successes to validate localization theory, and to then use localization theory as an example of why vivisection should be allowed. In banding together to form the physiological society, primarily for the purposes of combatting antivivisection, physiologists (who were often medical doctors) became allied with each other, and with evolutionary biologists and botantists.

One result of this was to link vivisection techniques with physiological research (Sharpey-Schafer, 1927). Thus, anatomically-oriented vivisection techniques were further legitimated on an organizational level by this response to uncertainty. And this occurred within the context of the clinical-basic triangulation situation.

4. Triangulation: Processes and Results

Each of the lines of work described above is enormously complex, with many internally overlapping uncertainties. But, as the next chapter will discuss in detail, localizationists also faced external uncertainties in the form of intellectual attacks from diffusionists. Evidence from the four lines of work discussed here was combined for two purposes:

1. For internal triangulation and validation of results--to cross check and validate results and;

2. To defend against external enemies (principally antivivisectionists and diffusionists). Triangulation as an

activity consisted in writing articles or books, or giving lectures, drawing on multiple realms of evidence.

As localizationists created arguments and formed alliances across lines of work, the uncertainty from the several lines of work was permuted. Sometimes grey areas from one line of work were clarified by results from another; sometimes uncertainties from two lines of work magnified each other. Sometimes anomalies from one line of work were jettisoned into black boxes created by another.

Ultimately, a "seamless" and apparently inevitably true theory emerged from the situation outlined above. This section of the chapter analyzes the role and mechanisms of triangulation in this process. Here I briefly review the responses to uncertainty taken by each of the lines of work discussed above.

Neurology: Uncertainty is segmented into present and future, the future is vested with certainty; anatomical lines of work are vested with certainty and the power to validate diagnoses; sequential, instantial modes of investigation were used; pathognomic signs were traded for configurations; there is a separation of cure and diagnosis; reports and patient populations were filtered for exemplars; there was a taxonomic focus within the line of work, and a technical focus and development outside the line of work.

Surgery: Criteria for success shifted--sometimes clinical cures and sometimes successful location of tumor; development of "vocabularies of realism" are developed; there is a focus on improving techniques; results of neurology and physiology are accepted as black boxes.

Pathology: The focus was microscopic; black boxes were developed; organisms and environments were black-boxed; comparative cases were not developed; uncertainty was segmented to the past; clinical results were vested with certainty.

Physiology: The focus was spatial; there was also a focus on minute areas; precision was substituted for validity and validity for reliability; filter for exemplars; a "blanketing" approach to the nervous system was used; clinical results were used for validation of work.

In each of these lines of work, there was both a focus for daily work and a vesting of certainty outside that focus. In neurology, the work focus was on building taxonomies; certainty was vested in the future and in anatomy. In surgery, the focus was on technique, and certainty was vested in neurology and physiology. In pathology, the focus was on individual tumors, and certainty was vested in clinical description. Finally, in physiology, the focus was on standardization, while certainty was vested in the ultimate validity of localization theory and in minute precision.

A complicated interlock between foci and vested certainties developed as localizationists triangulated evidence. The context of this interlock included: the constraints of medical work in general, professionalization processes, the distribution of resources to patients and among doctors, and the need for standard operating procedures in chaotic and often desperate situations. There were three major processes in triangulation:

1. Need for and development of atlases that would be both applied and theoretical.
2. Combination of expertise and shuffling anomalies across realms.
3. Standardization producing emphasis on precision and modular theories.

4.1 <u>Need for and Development of Atlases</u>

Neurology and pathology, through their daily work organization, held the individual as a robust unit of analysis. Problems were analyzed in terms of individuals or cases. Physiology held universal structure as the unit of analysis. Surgeons had two units of analysis, depending on whether they were searching for structures or acting as doctors with respect to patients.

For surgeons, this was based in the very practical need to operate quickly and accurately, to make an incision and attempt to find the structures described by neurologists. There were neither time, resources nor methods to deal with the individuality of brain structures. In physiology, the need to find universal structures was based on the attempt to replicate, in animals, disease conditions existing in humans. The match was not between Mrs. Jones' left temporal brain tumor and Edna the monkey's laboratory-produced lesion. Rather, physiologists tried to produce a situation which would mirror <u>any</u> brain tumor so located. The reproduction of the disease was general, not specific. Physiologists' mandate was to validate diagnostic taxonomies, not individual cases.

Thus, at the same time that the individual became the robust unit of analysis in two disciplines, individual differences were simultaneously black-boxed by another two disciplines seeking to validate their work. Individual <u>boundaries</u> were combined with an absence of <u>individual differences</u>. This created the need for universal maps or atlases which would be at least marginally applicable in each individual case. These atlases had to incorporate function (the problem of neurologists and physiologists) with structure (the problem of surgeons and pathologists).

Localization theory responded to and was developed in response to this dual set of needs, by applying both structure and function simultaneously. And, as a simultaneously diagnostic and basic research approach, it accounted for the needs of both clinicians and basic researchers.

4.2 Combination of Lines of Expertise and Shuffling Anomalies

Localizationists not only combined evidence from different lines of work, but different qualities of evidence from different kinds of activity. One example was given above with respect to vivisection; localizationists combined validation of the theory with the political question of whether the research should be allowed. Any opponent of vivisection also thus became an opponent of localization theory--not a "logically" necessary connection (Gerson, in prep.), but a powerful one in contributing to the legitimacy of the theory. This quote from a review of Ferrier's work illustrates the combination of political and technical robustness in the triangulation situation:

> The obscurity and confusion which still embarrass the study of cerebral diseases are to be attributed, in great measure, to the facts--1) that many functional nervous disorders leave no discoverable trace behind them; 2) that interference with the brain at any one point tends to general functional disturbance; 3) that the discoveries of experimental physiology are not altogether consistent with clinical and pathological observations. The principles which must guide research, and determine conclusions, in the localization of cerebral disease are--1) that one clear case in which destruction of a cerebral region causes no interference with the function assigned to it, must overturn a large amount of positive evidence in favor of this particular localization; 2) that it is not always necessary, when certain functions are deranged, to show organic lesions in the parts in which these functions are led; 3) that the symptoms

> observed with a cerebral lesion are not necessarily the result of derangement of functions in the parts immediately affected; 4) that any statement which contradicts our uniform experience must be regarded with suspicion, and received only when it has satisfied all the requirements of scientific evidence (editorial in Brain, 1878:130).

Ferrier's statement indicates both the complex interchange between clinical and basic realms of evidence, and the introduction of a separate class of political evaluation. Statements which contradict accumulated evidence have more stringent criteria of evaluation applied to them than those which go along with it.

Another kind of political alliance enters the triangulation situation as workers in different lines of work claim and attribute certainty to themselves and each other. Surgeons believed that it was their work which validated neurologists' localizations; physiologists believed that they had provided validation. Compare these two statements from MacEwan, a cerebral surgeon and from Foster, a physiologist:

> Cerebral surgery has been the means of adding to and confirming the knowledge of brain functions in man, especially of the regions of the cerebral cortex other than motor...In a sense some of these lesions produced in the brain of man may be regarded as experiments carried out by nature with a delicacy, accuracy and refinement which no human experimenter could equal (MacEwan,1922: 164-65).

> The results of experiments are in many ways conflicting, but still more conflicting and still less trustworthy are the results of pathological observations. In this and in so many other parts of physiology the so-called "experiments of nature" as seen at

> the bed-side, are extremely useful in suggesting and correcting experimental inquiry; but they prove broken reeds when reliance is placed on them alone. There is hardly a thesis in cerebral physiology, in respect of which a long array of "cases" may not be quoted strikingly supporting the views enunciated, and a long array as flatly contradicting them (Foster, 1877: 439).

While these two statements are some years apart, they represent prevalent ideological claims from the lines of work represented here. Furthermore, localizationist theorists, in making their arguments for the validity of localization theory, also presented such claims at face value. Physiology would correct the uncertainties in clinical work; clinical work would provide the validation for physiology. The phrase "experiments made by nature," originally coined by Jackson (1873a), was a common one throughout localizationist descriptions of clinical results. It represents the attempt to view patients as subjects and subjects as patients. The dual role of surgery in this process is vividly demonstrated by this description from one of Horsley's cases:

> Operation by Mr. Horsley. Patient under morphia and chloroform, parts of the right ascending frontal and parietal convolutions corresponding to the facial centre were removed.
> (During the operation the surface of the brain was explored with faradic battery and movement of lower jaw, downwards to left obtained and angle of mouth.) (National Hospital Records, Jackson casebook, 1887).

Similarly, from the physiologist's side of things, great emphasis was placed on situations in which physiological symptoms mirrored disease conditions (see e.g., MacCormac, 1881, and the famous description of Charcot's reaction to the hemiplegic monkey as a patient, in Thorwald, 1959).

4.3 Standardization and Precision

Uncontrollable circumstances in physiology and neurology forced workers to shift their focus from reliability (will these findings hold up under similar circumstances if the experiment is repeated; can I make it happen again?) to validity (am I describing the phenomenon accurately and completely within this context?) and from validity to precision (am I applying the measurements exactly?).

In practice this meant that taxonomies and regional descriptions of brain activity became focussed on smaller and smaller areas of the brain, with very detailed descriptions of each measurement application. Little attention was paid to contextual variation, comparative cases, or questions about the information which different measures revealed.

This substitution and downshift in focus allowed the work to proceed in physiology in two ways. First, the problems of localization became modular in form. This meant that anyone could take a small problem and make a contribution to localization theory. Access to the problem thus became quite easy. Second, the scope of each individual piece of work became increasingly circumscribed. Small, well-bounded problems whose solution were valuable to the line of work were plentiful. These results were then added together. Localizationists made maps of the nervous system by "blanketing" it. That is, they gathered pieces of information from a broad spread of research and made a complete picture of the brain and nervous system by overlapping them.

4.4 Perspectives and Triangulation

The combination of evidence from the various lines of work described here produced a theory that was both obdurate and complex. Its characteristics were modularity, a spatial bias, decontextualization, and a deletion of individual differences and temporal development. Its obduracy grew in part from an interlock between the foci and vested certainties of the four lines of work contributing to it. That is, each line of work had certain foci; and the validation of results and certainty of results was vested elsewhere.

This separation between focus and certainty, combined with using the results of other lines' foci as validation, produced an interlock between triangulated lines of evidence. As the previous chapter demonstrated, this interlock of evidence drew on and was formed by larger institutional contexts as well as the daily work contexts discussed in this chapter.

5. Summary: Triangulation and Perspectives

This chapter has discussed the uncertainties faced by four lines of work: neurology, surgery, pathology and physiology. As each line of work resolved its uncertainties, certain biases were incorporated into the conventional solutions collectively adopted by workers. As evidence from each line of work was used to legitimate localization theory, these biases combined. The result was a theory which was extremely obdurate. It was obdurate partly because it resolved uncertainties for each line of work and partly because it could not be challenged or disassembled by any one of them alone. It was thus useful and important for workers within each of these spheres. At the same time, it became hegemonic (see also discussion in Chapter 6).

In Chapter 1, I discussed five dimensions of a successful perspective. These were inertia, momentum, cumulative reification, progressive inseparability of parts, and incompleteness at any point. The contributions of triangulation biases to each of these in the localizationist perspective were considerable. Briefly, these were:

1. Inertia. The combination and substitution of do-

mains of evidence meant that legitimations and daily problem structures for localizationists were entangled. Once established, these legitimations and approaches to problems were difficult to dismantle, due to their bases in multiple and shifting work domains. Also, triangulation of evidence made single problem solutions applicable to multiple areas; once adopted in multiple sites, such solutions were difficult to change.

2. Momentum. The black-boxing of results across lines of work meant that results could be quickly picked up and used by multiple lines of work. In addition, the development of universal, standard atlases made localization models portable [12] and thus easily diffused across sites.

3. Incompleteness. Because artifacts were shuffled between lines of work, the analysis of any individual line of evidence would make that line seem robust. Artifacts were jettisoned from each line of work according to the exigencies of daily work uncertainties. Thus, getting a complete picture from any one point was impossible. Also, because conventions were developed in each work site and biases thus obscured, the purview from any single site could not be comprehensive.

4. Reification. The process of presenting biases (and conventionally derived) results from one line of work to another obscured the developmental aspects of conventions, and helped reify results. Also, as evidence from different

lines of work was combined, uncertainty from within each line was obscured, making final results appear inevitable and reified.

5. Inseparability. Overlapping of evidence from different realms made apparently logical connections from originally unconnected points. The hierarchical organization of the nervous system, for example, originally derived from evolutionary biology and imported through neurology, became linked to the idea of a nervous substrate, strengthened by pathological evidence and surgical approaches.

Localizationist theory was not immediately accepted throughout the medical world, however, as the following chapter illustrates. In analyzing the tactics and outcome of the debate about cerebral localization, we again see the central role of institutional context and daily work, as well as the gradual evolution of theoretical hegemony.

FOOTNOTES

1. Again, the mechanisms of professional autonomy and the tendency of professionals to reserve evaluation of results to members only, plays an important part here. Also, the

little-explored phenomenon of ascription of expertise across occupational and professional boundaries is also important (Freidson, 1970a and b; 1968; J. Musick, personal communication).

2. For that matter, one would be hard pressed for examples of _any_ fields where formal interdisciplinary triangulation rules have been developed. It is a singularly blank area of methods. The reasons for this are probably that most methodologists do not use historical or work-based standards of evaluation for arguments, but rather simply decompose them analytically/philosophically. The conventions which became embedded in problem solving are thus not accounted for, and _validity_ across lines of work is assumed to be equal and invariant. I am indebted to Rachel Volberg, Elihu Gerson and William Wimsatt for helpful discussions of these matters.

3. A countervailing influence in physiology was presented in the school associated with Claude Bernard in Paris, during roughly the same period. This was a systemic, homeostatic school of thought, and greatly influenced both physiology and medicine. Nevertheless, the physiological arguments by localizationists did not often use evidence from Bernard's school. Or, where they did use it, they took only portions and translated them into localizationist language. The relationship between systemic physiology and localizationism is one of the many virtually unexplored

areas in the sociology of science and in the history of physiology. Todes (1981) provides an interesting historical account of some of these issues in Russian physiology.

4. That is, the idea that some forms of epilepsy come from brain tumors or injuries and others from internal, perhaps hereditary, causes.

5. In the sense in which I am discussing it here; obviously modern pathological labs have their direct clinical applications.

6. For a discussion of this constraint in a modern neurophysiology laboratory, see Star, 1983.

7. With what Freidson calls "professional" authority as opposed to "scientific authority" (1970a and b; Freidson and Rhea, 1963).

8. Blackwood (1961) notes that there were significant advances in microscopy between 1860-1888, and that most clinicians at Queen Square did their own pathological and postmortem work. In 1889 the first pathologist was appointed to Queen Square, by which time this tradition had been well-instituted.

9. Grace aux progres du bon sens, on prefere l'experience au plus ingenieux systeme; la plus simple verite parait plus belle que tous les prestiges de l'imagination.

10. As Henry Head, a later localizationist, records in his volume on aphasia (1926:57):

> Moreover, all this school of observers believed that they could interpret the clinical manifestations directly in terms of anatomical paths and centres; each one added one or more cases to those that had already become classical...It was an era of robust faith and nobody suggested that the clinical data might be insufficient for each precise localization; still less could they believe that the conclusions reported by men of eminent good faith might be grossly inaccurate. In reading these admirably written papers, we are astonished at the serene dogmatism with which the writers assume a knowledge of the working of the mind and its dependence on hypothetical groups of cells and fibres.

11. "...no symptoms have been observed in man corresponding to the functional centres that you often see marked on these convolutions in diagrams of the human brain, to which they have been transferred from the brains of monkeys" (p. 170).

12. A recent paper by Wimsatt (in preparation) discusses the importance of portability and modularity for "generative entrenchment," a term which is similar to the concepts of inertia and momentum presented here.

CHAPTER FOUR:

TACTICS IN THE DEBATE ABOUT CEREBRAL LOCALIZATION

Localizationist research was not universally accepted from the beginning of the endeavor. In fact, one major impetus for triangulation of evidence was that localizationists were called upon to defend their theories to unfriendly audiences. By culling evidence from different spheres, they could respond to criticisms with more substantial counter-points.

This thesis has analyzed several facets of the formation of the localizationist perspective: institutional and professional developments, daily work uncertainties, conventions and combinations of localizationist arguments with resulting <u>triangulation biases</u>. This chapter analyses another important factor: the role of debate with anti-localizationists in shaping the perspective's development.

1. <u>Critics of Localizationism</u>

Critics of localizationism were not an organized group, and information on their work was often difficult to find. Their position was also not monolithic. Diffusionists criticized localization theory on several different grounds, developing and drawing on alternate theories of nervous functioning. This chapter, however, focusses primarily on their interactions with the group at Queen

Square, and on the dynamics of the debate between the Queen Square localizationists and diffusionists in various locales.

Who were the diffusionists? The most prominent ones were:

1. C.E. Brown-Sequard, a Mauritian doctor (Olmstead, 1946) who did physiological experiments in France, England, Mauritius and the United States. He ultimately (in 1878) succeeded Claude Bernard to the chair of medicine at the College de France. Brown-Sequard developed a theory of "action at a distance" and "remote effects" to explain the relationship between lesions and dysfunctions. He believed that lesions could exert an influence anywhere in the nervous system, and that influence could be either inhibitory or excitatory. He saw two counterbalanced forces in the body: inhibition and dynamogenesis. It was the influence of these forces, and not of discrete areas with individual functions, that caused symptoms thought to be localized, by Ferrier, Jackson, Horsley and others who were attacked by Brown-Sequard (see e.g., Brown-Sequard, 1873; 1879; Role, 1977).

2. F. Goltz, a German physiologist, investigated nervous inhibition. He removed animal cortices and observed the effects. Goltz attempted to refute localizationism by showing that animals without cortices were still functio-

nal. He removed the areas localizationists supposed responsible for motor movements, but his animals still walked, ate, and responded to contact. Goltz held that this indicated the absence of discrete sensory-motor areas as posited by localizationists (see e.g., Goltz,1881; see also Appendix A).

3. Mario Panizza was an Italian physiologist who criticized the idea of "centers" and what he saw as methodological problems in localizationist theory. He maintained that localizationism was in a sense a "category error"--it could not be directly proved or disproved by direct evidence, because there was no basis for causality in the way localizationists were approaching the issue. [1] He, like Brown-Sequard, saw the nervous system as composed of complex, labile, sometimes duplicate functions which could also be interactive.

Like the Pragmatist philosophers who criticized reflex psychology, Panizza noted that sensations could not be localized, because such a model would require that sensation enter at one place and come out at another, meanwhile stopping somewhere to be processed by a nonexistent "overseeing" function (see e.g. Dewey, 1981 [1894]; Bentley, 1975b). [2] Panizza saw the nervous system as more temporally fluid than did localizationists (see Panizza, 1887; Appendix B). Panizza's work and criticisms were virtually ignored by English physiologists. I found only one review of his work (Rabagliati, 1882).

2. General Form of Debate

The general form of the analytic argument between localizationists and diffusionists was this: localizationists would publish the results of brain-mapping experiments or case studies. They would claim this as evidence for localized functions in the brain. The evidence would then be attacked by diffusionists (e.g. Brown-Sequard, 1879) or by other localizationists with slightly different approaches or hypotheses about the location of specific functions (Knapp and Bradford, 1889).

The issues raised by diffusionists were often incorporated (and I do not mean to suggest a direct back-and-forth here; there is no evidence for such a path of influence) into localizationist models, but they were given a different meaning because of the different analytic framework of the localizationists. [3]

As noted above, the participants in the cerebral localization debate represented several lines of work: neurology, surgery, pathology, physiology, hospital administration, philosophy, journalism, and patient work. Based on their work, these groups adopted different and overlapping debating positions.

During the period considered here, these multiple positions strengthened each other in complex ways. Like the strands of a braid, they became stronger in conjunction than any one argument would be alone. As I have demonstrated in the triangulation situation, such conjunctions become quite complex and obdurate. Although the logic of conjunction of the arguments was almost never addressed by participants, nor the connection between their work and their debating positions, they drew from each others' positions and from the circumstances of their daily work to weave a pattern of argument and proof that is extraordinarily dense. The situation was described by Paget (1919), biographer of Horsley:

> After 1875, the output of work, in this and other countries, became so great that no man can describe it. The main lines of it are clear enough. One was the application of the facts of cerebral localization to the study of injuries and diseases of the brain; with special reference to cases of "Jacksonian epilepsy." Another was the advancement of the surgery of the brain. Another was the incessant criticising and interpreting and adjusting of all new facts and theories as they came to hand. Another was the modern study of the deeper parts of the brain, and of the spinal cord. Along these and other lines, all of them crossing and recrossing, legions of men were at work (p. 94).

"Winning" the localization debate was thus a complicated progression of events. Its logic and progress are not found in the rules of a game, but rather in an analysis of the kinds of work which are entangled in its momentum (Star, 1982b). Over the thirty-six year span analyzed here, concepts of localization of brain function and the resulting picture of the brain-mind relationship became increasingly tied to a number of going concerns in medicine, physiology and psychology (Walker, 1957).

The picture of debate presented here, then is like the analysis of conflict presented by Simmel (1903-04). Conflict is itself socializing, in that participation in conflict develops commitments to certain paths of action. Scientific debates, in dialectical fashion, arise from commitments to work, and conflict helps shape those commitments.

3. Stylistic Conflicts

I have discussed some of the conventional ways of approaching questions developed by the various lines of work involved in investigating localizationism. In this chapter, I will to discuss more broadly the kinds of tactics used when scientists from work situations with sharply differing approaches, or styles, disagree about results.

Gerson (in prep.) defines style as a set of commitments to approach problems in certain ways. These approaches cannot be reduced to logical components or defined strictly along logical lines. Rather, they are overall approaches to the world that are often rooted in "ultimate questions" about nature. Styles may vary along several dimensions, including particulate (the world is composed of discrete particles, added together to form a whole; also called atomistic) and systemic (the world is an inviolable whole, and things operate together to form a unity; also called wholistic or gestalt).

The localizationists were particulate in style; diffusionists were systemic. Thus, the debate about localization of function in the brain is an example of a debate between incompatible stylistic approaches. There were also arguments within the localizationist camp about the extent or location of specific areas, but I do not discuss those here. Rather, in this chapter, I seek to elucidate the clash of styles as an important element in conflicting perspectives.

Conflict between stylistic orientations is a frequent and important phenomenon in the history of biology: in the battle between Ramon y Cajal and Golgi about whether nerves are continuous or discrete; in reproductive biology, about whether reproductive cells reproduce in particulate or systemic fashion (Clarke, in prep.); in population biology, between those advocating an organismic approach and those advocating a population approach (Gerson, in prep.).

The sociology and history of science have paid little attention to the formal properties of scientific debates although case studies do exist (see e.g., Cravens, 1978; Farley and Geison, 1974). Recent work by Gerson (in prep.; 1983a) and Strauss (1978b) has begun to analyze debates from the perspective of social worlds and scientific work.

A scientific problem becomes a debate when workers disagree about what needs to be done next, but negotiations continue as participants refuse to change bargaining positions (Gerson, in prep.). The scope of these debates can be large or small; they can be prolonged or short-lived.

Debates which occur across several lines of work involve intersections as well as arguments. That is, multiple traditions and ways of working are at stake in the debate about what necessary next steps will solve the problem. In the social world analysis, debates become arenas when they accrue multiple lines of work, public appeal, and, often, policy interests, and thus become a relatively stable focus for work and conflict. Stylistic conflicts, because they are irreconcilable, may actually help stabilize the arena. Goltz, for example, complained of being made into a symbolic "loyal opposition" in the localizationist-diffusionist debate, despite certain agreements with the localizationists:

> In the meantime I went along my own way. Even if I was convinced that the assumption of small circumscribed centers did not correspond with the facts, that did not mean that the idea of Flourens, that the substance of the brain was equivalent everywhere, was correct. I tried to find out what consequences the amputation of single lobes of the brain had, and found that the destruction of the anterior lobes led to entirely different disturbances from the destruction of the occipital lobes. In this direction, too, I could rectify Flourens' ideas. Instead of thanking me, the proponents of cerebral centers have accused me of fleeing from the colors. It should be my business to deny any localization. If I now admit a

certain localization, I denounce former sins, so I was told (Goltz, 1888: 129-30).

In his discussion of the evolution of debates, Gerson (in prep.) states that debates may become gradually packaged over time. That is, as portions of argument, or ways of arguing, become well worked out by participants, and other problems involved in the debate get solved, the debate becomes increasingly treated as a seamless whole. It becomes a package whose contents are not subject to question. For example, there was a great deal of argument in the nineteenth century about the relative merits of induction current vs. alternating current in testing electrical responses in the brain (e.g., Dodds, 1877-78; Duret, 1878; Ferrier, 1876). That part of the debate became fully packaged with the advent of commercial electroencephalographic firms, which provided the standard machinery for measuring brain electricity and giving standard amounts of current in experiments.

Philosophical questions get packaged as well. For example, the argument that "correlation is not causation" is now a familiar one to most scientists, and its points generally communicated by the phrase itself [4]. In the nineteenth century, it was still being argued as a fairly fresh issue whose points were not always accepted in the scientific community.

4. General Background of Debate

Several kinds of argument were entangled in the cerebral localization debate in the nineteenth century. These were:

1. Existence. Does cerebral localization exist? Can the brain be divided into areas with different functions?

2. Extent. If cerebral localization exists, to what extent? In animals and humans? If only in animals, which ones? In the cerebral hemispheres as well as the lower brain centers? If not, where does it stop, and why? Is it so localized as one place, one function? Or could it be two places, one function? Several places, one function? Are functions then duplicated or distributed?

3. Causes and consequences. What causes localization? Is it an evolutionary, anatomical, or socially-derived phenomenon? Is it caused by the distribution of different kinds of cells throughout the brain (Campbell, 1905)? Or at the endpoint of various nervous pathways? Do events and abilities become located in the brain via chemicals, associations, or electrical storage? Is there a necessary connection between localization of function and impairment of function in certain regions of the brain? Does localization of function in the brain imply a similar localized organization throughout the nervous system?

4. Use. Can localization of function be used to diagnose diseases of the nervous system? Can it be used as

a model to explain the nature of the mind? Should clinical principles be derived from it?

5. Ethics. Is cerebral localization a good way to conceive of human nature? Does it have good or bad consequences for the way human nature is perceived? Is doing the research itself a good or bad thing? (I include here vivisection arguments.)

These several types of debate were often mixed in the larger cerebral localization debate, in addition to the triangulation of evidence from different spheres. Simultaneous arguments were going on, for example, about whether localization existed, if it existed only in animals, whether duplication of functions occurred, and if this latter meant that localization did not exist after all. And while all these arguments proceeded--in the medical and scientific press, in medical congresses and demonstrations, in professional associations--localization-based practices were instituted in hospitals and physiological laboratories, and patients were educated (through the popular press and through doctors) about the merits of localization.

This meant several things. First, the sub-debates were conflated. Participants in the sub-debates often raised an issue from one set of problems and were answered with one from another; would give an example from one domain of evidence and receive a counterexample from another. Second, as _anomalies_ arose in the course of research and daily work, workers would often find them being argued about in the scientific journals. Thus, their daily work concerns were addressed in a larger sphere of debate, and against a common enemy. Third, within a given line of work, arguments were often _confounded_. That is, participants talked past one another, answering questions that not only did not address the original question, but changed the terms of the argument by _substituting_ concerns.

Debates are often thought of as simply verbal or written interchanges. The debate presented here is a battle whose weapons are only partly linguistic. It was fought through many kinds of action, not simply writing or speaking. Demonstrations, solutions to problems at work, successful operations, lobbying, voting, and funding all became debating strategies. As I have demonstrated in the previous two chapters, the successful construction of the localizationist perspective arose both from daily work concerns, and from an institutional context which supported its development. Here, I examine those two kinds of concerns in a slightly different light, but one which is also dialectical. Localizationist debaters were simultaneously _defending_ their perspective from outside attack by diffusionists, and _using_ information published in the defense to solve problems arising in the work.

The conflict about localizationism was thus socializing in two senses. First, a set of arguments was developed and packaged by participants which, because of the institutional strength which it developed, became a standard approach to questions about the nature of the brain. These arguments were used to resolve ambiguities in work. Thus participants were socialized in debating to handle anomalies in certain ways. Secondly, the questions raised by diffusionists were either ignored or partially assimilated into the localizationist perspective. Thus, as debaters faced common enemies and their criticisms, they collectively developed a hierarchy of significance for questions about the brain and its functions.

Analytically, this adds a third layer of context to the analysis presented thus far. Chapter 2 presented the institutional context of localization work, and the processes of segmentation and professionalization which supported and helped form the perspective's development. Chapter 3 presented the daily work uncertainties faced by localizationists and the conventions they developed in resolving them. Triangulation of results between different conventions was discussed, with attendant biases.

The institutional, daily work, and triangulation contexts described thus far have alternately been figure and ground for one another depending on the analytic emphasis. The debating context pushes this analysis into a 3-space form. That is, debating with diffusionists affected both the daily work situation (in the sense of developing packaged stances toward certain classes of problems) and the institutional context (in the sense of united against a common enemy and creating a homogeneous pool of researchers by defining an "in" and "out" group). There are interactions between all three processes as well.

5. <u>Issues in the Debate</u>

In simplest form, the idea of localization of function in the brain is based on a constant relation between a region and a function: the "speech area" as located in the third frontal convolution, for example. However, it quickly became apparent both to localizationists and diffusionists that such a simple correspondence did not fit the available data. As Ferrier put it:

> It is evident from the discrepancy of views thus enumerated that the facts of disease on which they are based are neither uniform nor altogether simple. I will not here attempt an analysis of the individual cases, adduced in favour of this or that hypothesis, but merely apply certain rules which should guide us in forming a decision on these points. Mere frequency, as the records of cerebral disease amply illustrate, is not sufficient to establish direct causal relationship between the obvious lesion and the symptoms exhibited (1889:39).

Goltz made a similar point:

> The fact that the result of the stimulation is so extremely different according to the location of the stimulated point, suggested that the different sections of the cerebrum could be equally different in

their functional meaning. But Fritsch and Hitzig also realized that the stimulation method alone would be insufficient to successfully pursue this idea any further. If we see a group of muscles, for example the muscles of the foot, twitch as a result of a stimulation, the nervous system stimulated can be of very different importance. Whether we stimulate the motor nerves, the spinal cord, the brain, or certain sense nerves, in all cases twitches of the foot muscles can be causes. Therefore, I cannot deduce with certainty from the twitching foot muscle caused by the stimulation of a section of the nervous system, what the functional importance of the stimulated organ is (In MacCormac, 1881: 219).

And Panizza made an even more radical statement about the possibilities of inferring function from injury, and the limits of correlation:

> What we deduce from the facts is contrary to the theory of localization.
>
> Given these considerations, it is clear that the fact of partial sensory and motor paralysis following an injury to the nerve centers can never be brought up as proof that either sensitivity or the instigating principle of movement is located in the injured spot. In order to be used as proof, it would be necessary for no other area to be injured and give rise to the same effects, and for the same effects always to follow the injury of the same area with absolute constancy. But such constancy is universally contradicted by the facts. Not only can the irritation of <u>any</u> point on the nervous system give rise to the same morbid phenomena identical to those obtained by irritating areas that are far apart and totally different, but the most varied phenomena can be obtained by irritating the same point: "Very numerous experiences," writes Brown-Sequard, "which confirm the teachings of human pathology have proved to me that producing the same injury in the same area of animals of the same species can give rise, as in men, to an immense variety of effects (1887:149).

The reason that "mere frequency is not enough" was that many of the data are contradictory or unclear. In their struggle to explain contradictions diffusionists raised to attack them, localizationists attempted to develop <u>principles</u> which could withstand the exceptions and contradictions of data that did not fit their principles.

The contradictions raised by diffusionists about inconstant relations were of two kinds: symptoms with no lesion (found in postmortem examination), or lesions/tumors with no symptoms. Brown-Sequard had this to say about inconstancy as disproving localization theory:

> A propagation of organic alterations from the seat of an original lesion in the brain can, of course, give rise to symptoms. But we cannot explain the appearance of symptoms in that way in most cases of hemorrhage, of wound, or of softening of the brain, as it requires more time for the propagation of a morbid state to distant parts of the brain than there is between the moment the lesion takes place and of that apparition of symptoms. Even in cases of tumors enlarging slowly, we cannot look upon such a propagation as the essential cause of appearance of symptoms, as there are many cases of tumors in which there is no propagation of a morbid state (1873b:253).

Brown-Sequard proposed alternate explanations for the appearance of symptoms in the absence of lesions: change in "quantity or quality of blood"; irritation at a distance; changes in pressure. He went on to say:

> <u>There is no relation whatever between the extent of a lesion in the brain and the symptoms that may be caused by it</u>. If symptoms were due, as is admitted, to the loss of function of the part altered or destroyed in the brain, or to <u>immediate</u> effects of the irritation of such a part, there would be a constant relation between them and the diseases, so that the intensity and extent of the symptoms would be in proportion with the intensity and extent of the alteration (1873b:257, emphasis in original).

A second, related class of points raised here by Brown-Sequard is that an "immense variety of symptoms in different individuals may be caused by a lesion in one and the same part of the brain" and/or that "the same symptom may originate from the most various lesions" (1873b:259).

The issue of multiple lesions/similar symptoms or similar symptoms/multiple lesions was a vexing one to the localizationists, as were all forms of the "inconstant relation" argument.

As the debate progressed, and as anomalies of this nature piled up, localizationists relied more and more heavily on the idea of redundancy of functions to deal with the anomalies. This was the theory that functions were localized in multiple areas; that there were multiple "centers of control" for various functions. Horsley had this to say: "The little centres, therefore, which preside over this function are dotted all down the spinal cord in each segmental division of it..."(1892:187). Some years earlier, he had put forth this idea under the title "On Substitution as a Means of Restoring Nerve Function Considered with Reference to Cerebral Localisation." (1884).

Here the localizationists and the diffusionists approached the same anomaly in vastly different ways, reflecting their different stylistic commitments. The idea of redundancy allowed localizationists to meet and assimi-

late many of the criticisms of the diffusionists, yet remain within a particulate view of the nervous system. The idea of many centers was different than the idea of holistic functioning; yet both localizationists and diffusionists agreed upon the existence of the anomaly.

Brown-Sequard also brought up the indeterminacy of the structure-function relationship posited by localizationists. One can favor a right hand or a stronger muscle, he suggested, but this does not mean that the function performed by the hand or muscle is "hardwired." He thus implicitly raised the idea of localization as indeterminate as well as inconstant (1873a:120). As far as I know, this criticism was never answered by the localizationists in this period, and where issues of "favoring" were dealt with at all, it was under the rubric of habituation or "impressions" on the nervous system from "accumulated experiences" (Ferrier, 1876).

Localizationists and diffusionists, then, did not disagree about the existence of symptoms, or even on discrepancies in the ideal one-to-one correlation between function and region which a strict localization theory posited. Rather, it was the negotiated <u>significance</u> of discrepancies and symptoms that differed between approaches. Nor were there large differences in the methods used by diffusionists or localizationists to examine these questions. Ferrier, Yeo, Horsley, Brown-

Sequard, and Goltz, for example, all used vivisectionist techniques and created lesions in their subjects to produce results for analysis. All debaters relied on patient case histories for data.

The use of debating tactics was similarly comparable across camps. The primary difference in outcome of the debate is found not in the superior arguing styles of localizationists, but rather, as Chapter 2 demonstrated, in the fact that they were able to link each tactic with a successful organizational enterprise.

Both sides, for example, tried to establish credibility and pile up credentials to support their arguments. Yet the localizationists' use of this tactic was more effective, because it was picked up and implemented broadly within allied organizations and training sites, and because the credentialing process used in one localizationist site could legitimately be used in another. Similar asymmetries existed for all of the tactics discussed below.

6. <u>Tactics Used in the Debate</u>

Several kinds of tactic were used in the debate. I define them briefly here, then elaborate them with examples. It is important to note that these tactics may be used in a number of different <u>modes</u>. That is, they can be used to demonstrate a fact, to analyze inconsistencies, to examine or criticize procedures, or to identify what skills should be brought to bear to solve the problems (see Gerson, in prep., for an analogous discussion of problem decomposition strategies in scientific research).

The tactics may also be used to upshift or downshift arguments (again, see Gerson's discussion of these processes, in prep.). Upshifting means to raise the level of evaluation of the problem involved: "It's not as simple as that, it's a question of basic approach." Downshifting, similarly, means to lower the evaluation level: "It's not all that complex or important, it's simply a matter of re-doing the figures."

The tactics can be used at any debating stage: making the argument, challenging the argument, responding to the argument. As positions acquire inertia and momentum, and become cumulatively reified, the stage at which tactics are applied is significant.

The following tactics were used in the localizationist-diffusionist debate:

1. DIPLOMACY. These tactics smoothed over conflict between negotiators and, at least temporarily, achieved peace.

2. ESTABLISHMENT OF CREDIBILITY. These tactics were used to establish that an argument is sound, solid, and reliable.

3. USING AND MANIPULATING HIERARCHY OF CREDIBILITY.

This was the use and abuse of claims that one's argument was superior by virtue of one's status; and, conversely, that one's opponents' arguments were no good because of their lower status or inferior qualities as persons.

4. ORGANIZATIONAL TACTICS. These were tactics which used organizational (either interorganizational or intra-organizational) politics to support or attack an argument.

5. MANIPULATING FOCUS. These were attempts to change the configuration of what was significant or insignificant in an argument; to switch some things from foreground to background, and vice versa.

6. ARGUMENTS FOR STATUS QUO. These were tactics which appealed in a positive way to the status quo of a position.

6.1. Diplomacy

Diplomacy was used to defuse a challenge, or in the attempt to establish a competing perspective without antagonizing the "powers that be." Diplomatic tactics included assimilating parts of challenges; creating truisms; appeals to tradition; and asking an audience to "bear with us."

6.1.1 Assimilating parts of challenges.

This refers to the practice of appearing to accept a criticism as true, but in fact putting only a small part of it into practice. It can also mean giving a pro forma acknowledgement to a challenge (often the accusation that data contain artifacts) in the beginning of an article, but in fact not incorporating the criticism at all. I call this latter phenomenon an "artifact bow," and it is still quite common in science today.

A generalized idea develops in a sphere of argument that certain phenomena exist and should, in principle, be accounted for. But no mechanism exists in the line of work for actually accounting for them. Thus, in psychological experiments now, there is acknowledgement that "experimenter expectations" influence research results, or that psychological labs create certain compliance demands for subjects (Orne,1962; Rosenthal, 1963; 1966; see also discussion in Star and Gerson, forthcoming a and b for an analysis of the negotiation of these artifacts in psychology). Yet, institutional method for accomodating these are scarce. We find a similar situation in the claim for ceteris parabis (Lilienfeld, 1982) in modern-day clinical trials.

Certain artifacts achieved this status in the localizationist-diffusionist dispute. While localizationists transformed, ignored or assimilated many diffusionist criticisms, other criticisms were taken seriously enough to be openly acknowledged in localizationist writings. Often these acknowledgements had no direct impact on the work itself, but were included as disclaimers. Similarly, no mechanisms were instituted in the lines of work to deal with them.

Two types of disclaimers developed. First, there was a general acknowledgement that the brain is an <u>interrelated whole</u>. Second, there was an acknowledgement of <u>uncertainty and imprecision</u> of measurement techniques, often attributed to a different line of work. Let's consider these two disclaimers:

The disclaimer about the interrelatedness of the brain took the form that, "Of course we understand that everything's related, but..." The ellipses implied that such interrelationship was impossible to translate into practical action; that it was ineffable; that it had no practical implications. Horsley expresses the concept in this way:

> Conceivably every part of the body is represented in every nerve-centre, just as it of necessity is in the single primordial ovum: and that when the nervous system, considered functionally, is regarded, in the view of Flourens, as working as a whole, the determination of action by any given part is never more than relative...When we are labouring with the difficulties of endeavoring to establish a differential diagnosis, we are likely to forget that the whole machine is in active operation while our attention may be drawn to one point only (Paget, 1919:180-81).

Here, it is the exigencies of clinical diagnosis which prevented localizationists from dealing with the interrelatedness of the brain. Ferrier, by contrast, lodged his disclaimer in the ascent up the evolutionary scale:

> as we ascend the animal scale, the centres of which the cerebro-spinal system is composed become more and more intimately bound up and associated with each other in action, so that to separate the one from the

other involves such functional perturbation of the whole, that only in rare instances is it possible to obtain indication of independent activity in the part of those which are not directly injured (1876:38).

And later in the same volume:

> The difficulties of localisation necessarily increase when the regions of the brain under exploration are incapable of being clearly freed from surrounding sensitive structures; and where such is the case the phenomena must be regarded as of doubtful significance unless their nature can be resolved by other and complementary methods of investigation (p. 148).

This last statement of Ferrier's shows the course of action taken in the face of the diffusionist "truisms." While it was acknowledged that the brain acted as a whole, and that cerebral structures could not in fact be separated from one another, the responsibility for incorporating this approach was not located in a single line of work nor did it bring investigation to a halt.

6.1.2 Creation of truisms.

Another tactic was asserting that of course the basic premises of the opposing position are quite true, but there is nothing practical to be done with them. Even to consider implementing the premises was portrayed as somehow absurd. This tactic sidestepped direct confrontation at the basic philosophical assumption level, and substituted instead the evaluation criterion of practicality.

Ferrier's The Localisation of Cerebral Disease (1878b), had several examples of the use of truisms as a debating tactic. On page 2, he states that one clear case

of destruction of an area with no resulting functional disorder "would be sufficient to overturn our conclusion" (about localization), and two paragraphs later he says:

> The doctrine of cerebral localization does not assume, as Brown-Sequard would seem to imply, that the symptoms observed in connection with a cerebral lesion are necessarily the result of derangement of function in the part immediately affected. Every one admits direct and indirect results in cerebral disease. We have no right even to assume any causal relation at all, direct or indirect, between the phenomenon, unless the lesion in question is constantly, or more frequently than chance would account for, associated with the same symptoms.

Yet, some pages later in the book, he attacks Brown-Sequard for positing an intermediate link between lesions and dysfunctions, and for challenging the direct link in fact posited by localizationists between lesions and dysfunctions (pp. 41-42).

The phrase "everyone admits direct and indirect results in cerebral disease" is a good example of use of truism as a debating tactic. While everyone may <u>admit</u> that such a phenomenon could in principle exist, only the diffusionists took serious practical account of it at the time that this article was written. Yet making the statement covers the bases against that criticism.

Goltz made a similar disclaimer in his discussion on the localizationist position:

> When these and other considerations forced the conclusion upon me that the hypotheses of Hitzig, Ferrier and their successors could not possibly be

> correct, then this did not exclude, of course the possibility that some totally different kind of spatial distribution of the functions of the cerebrum does actually exist. In fact, I have been said (blamed) to deny any localization of the functions of the cerebrum, but that is wrong. I absolutely believe it possible that the individual corteces of such a powerful organ as the cerebrum have varying functions. But whether and how far this possibility proves true, that will still have to be investigated, dispassionately investigated (in MacCormac, 1881:223).

Both the localizationists and diffusionists, then, here use truisms or partial acceptance of the other's opinions (although Goltz' acceptance was more highly qualified than Ferrier's, with an emphasis on what is possible). Such truisms have the advantage of making the debater appear reasonable, understanding of the position of the other side. It is politically more strategic to offer an alternative explanation than to argue basic premises from scratch, and here we see both sides trying to portray themselves as arguing for alternatives rather than whole new frameworks.

6.1.3 "Bear with us"

Some diplomatic arguments take the form of a plea to an audience (sometimes, perhaps usually unspecified) to wait for the future payoff of a set of experiments or the application of a perspective, and meanwhile accept the basic premises. The implications of such tactics are that while results can't be demonstrated now, they can be in the future. In one sense, the tactic is an attempt to inveigle an audience into a side bet with the experimenters. Such tactics involve using the <u>unknown</u> to argue for a position.

Horsley, for example, makes the following statement:

> As yet this hypothesis has to be tested by experiments directly bearing on the point, such, for instance, as first extirpating a motor centre completely (vide <u>infra</u>), and then some weeks or months later exploring the portion of brain in the immediate neighborhood with very weak induced currents....But even in the absence of direct experimentation, it is obvious that there are certain centres in the cortex destruction of which, or the fibres leading from them, is followed by permanent paralysis (1884:7).

Both sides used the tactic of "bear with us." Some of the most acrimonious parts of the debate occurred when this tactic was challenged.

Atkins (1878) provides an example of "bear with us," including "don't bear with them":

> These views of M. Brown-Sequard are, comparatively speaking, but recently promulgated; and should they have any foundation, not only the more modern doctrines of localisation, but also the older and hitherto firmly believed in theory of the crossed action of the cerebral hemispheres must be at once and for ever abandoned. With all respect, however, for their great originator, I hold that we cannot accept these views with the degree of proof at present forthcoming...However, they are now at the bar of science, and time and further research will show sooner or later whether they shall stand or fall....These hypotheses [of localizationists now], though they may satisfy the mind for a time, are after all mere speculations which do not throw any real light on the subject. We must be content at present with recording such cases; and, although we may be led to speculate thereon, we can only hope that extended research into now hidden domains may in time afford th real explanation which is at present concealed from our view (pp. 415-17).

The diplomatic tactics outlined here in general have

the characteristic of sidestepping direct confrontation between proponents with different assumptions. One important outcome of diplomatic tactics is that apparent agreement may be reached on a temporary basis. This temporary agreement holds for a period of time, during which different sides of a debate are competing for resources, establishing programs, and continuing to argue on other grounds. It can thus be a "holding" tactic which forestalls confrontation while validity is being constructed by other types of commitment.

6.2. Establishment of Credibility

What we think of as routine scientific work can also be a debating strategy. The persistent compilation of a "track record" is an important way to establish credibility in a long debate. Some aspects of this process have received little attention, and I focus on them here.

6.2.1 Filtering for exemplars.

This tactic means finding the example that most clearly fits the theory and emphasizing it in speech or writing. I discussed this tactic in Chapter 3, where it was part of constructing clinical/instantial arguments and managing uncertainties encountered in clinical work. Such exemplars demonstrate a smooth match between theory and examples, ignoring contradictory or vague cases which do not fit either nosologically or diagnostically.

Localizationists presented such exemplars as "celebrated cases." There are several cases in the medical literature which were especially dramatic: the first brain tumor operation (Bennett and Godlee, 1884); the first operation for a tumor of the spinal cord by Gowers and Horsley, in 1879 (Paget, 1919). These cases were used to underscore the validity of localization theory, while unsuccessful or vague brain or spinal cord tumor cases were often not discussed in the literature (see National Hospital Records, 1870-1901).

6.2.2 Plodding through.

Another effective strategy for establishment of credibility was simply persisting despite criticism or contradictory evidence. This can mean simply continuing to publish results, and repeat analyses, with small incremental changes, or with just slight variability in application of a formula. Stubbornness is an important part of winning an argument. Many of the localizationists simply outlived their opponents. Brown-Sequard's challenges, for example, spanned two decades. Ferrier, and others to follow him, kept publishing books and articles which simply classified regions of the brain in various animals and humans (see e.g., Ferrier, 1878b; 1910) during Brown-Sequard's life and after his death (in 1894). Despite use of diplomatic tactics and partial assimilation of Brown-Sequard's criticisms, perhaps the most effective tactic was the patient accumulation of publications and repetition of the argument by localizationists.

6.2.3 Accrual of Cases.

This tactic involves collecting a pile of cases or examples which fit a particular taxonomy or theory. The logical basis of this proof is additive: simply, the more one has, the more convincing the argument. Gerson (in prep.) calls this "piling up examples." This type of work is clearly defined and easily divided.

Accrual of cases was particularly common in the clinical sphere and was used to argue for localization. Cases of brain damage, stroke or tumors were presented in the contemporary medical journals as evidence for localization of function. These cases, which took up a substantial percentage of the journals, were often simply presented with no explicit theoretical accompaniment, or simply with a statement of the form, "x symptom indicates functional disturbance in z areas" (see e.g., Atkins, 1878; MccKenzie, 1878). The cases were sometimes accompanied by postmortem information showing softening or malformation of the brain in the correctly localized area.

The cases were strikingly detailed, often with hour-by-hour descriptions of patient behavior, symptoms, medication prescribed, as well as minute detail about tumors, discharges, and other clinical phenomena. The cumulation of cases here acted as a data base for theoretical arguments, as well as a body of evidence composed of numerous examples.

6.2.4 Simplification.

Credibility is established through simplification (Star, 1983) by jettisoning complications or contradictory evidence. Anomalies and exceptions brought to the attention of localizationists were often either jettisoned or transformed. Consider the following example from a localizationist anatomist who prepared a cytological atlas of "Ferrier's sites":

> histologically the area of Broca pertains to the intermediate precentral field; its type of cortex, as displayed by the methods I have employed, does not differ from that situated immediately above, nor from that extending in continuation with it forwards and round to the orbital operculum...This is of course negative evidence, but it gains in significance when all the data of clinical experience, both the positive and the negative, are considered together with it. Thus in cases of tumour, meningitis, and allied conditions in which the lesion though accurately localised is irritative and superficial in kind, it is a matter of common knowledge that if aphasia results, it is of a transient nature. On the contrary, in cases of a lesion extending to the underlying parts the disability is permanent (Campbell, 1905:222).

This negative evidence (of a lack of difference between the speech area and the surrounding tissue) was explained by reference to a deeper causal site and to the transient nature of the lesion, which is said to be, however, "accurately localised." This explanation for the anomaly occurred in a volume whose purpose was to match cytoarchitectural data with the findings of localizationists in other lines of work. Here we see several debating tactics fused together.

In the quote above, which was late in the period under consideration, Campbell was able to appeal to "all the data of clinical experience..." and "it is a matter of common knowledge." Here truisms, cross-legitimation, and increasing inertia were intertwined. The negative findings are simplified in the sense that contradictions are jettisoned and transformed.

The general thrust of establishing credibility was to emphasize positive findings and accumulate them slowly over a long period of time.

6.3. Manipulating Hierarchies of Credibility.

A hierarchy of credibility refers to the differential weight given to the word of people or organizations with different status. A person or institution at the top of a hierarchy is more "believable" than someone at the bottom (Becker, 1967).

Scientific arguments which manipulate hierarchies of credibility are not officially sanctioned by scientific method. Yet they are common. Their source and means of enforcement are found in the institutional power of those with more resources. The localization debate was not won because localizationists were more sarcastic or _ad hominem_ than diffusionists. Rather, localizationists and diffusionists both used the following sets of tactics, made more effective in the localizationists' case by their control of resources. Localizationists rose to the top of the hierarchy of credibility in physiological research.

6.3.1 "More scientific than thou"

These are claims that one procedure or approach is more scientific than another (Gerson, in prep.). Such claims are often opposed to designations like "metaphysical," "poetic," "impressionistic" or "soft science." Claims to scientificness are often used in conjunction with new technologies whose drawbacks may not be well understood. To use the new technology is to be more "scientific" than to employ older, often non-technological methods.

Such claims were often tacitly made by localizationists, as medical science sought to become more scientific through physiological research. Localizationists claimed to be more scientific than diffusionists as they made alliances with doctors using electrical therapies (Schiller, 1982); with evolutionary biologists (Sharpey-Schafer, 1927); and used new technologies in histology and microscopy.

6.3.2 _Ad Hominem_ tactics.

These tactics criticize an argument on the basis of personal characteristics of the author. _Ad hominem_ arguments discredit opponents, attempting to place them lower in the hierarchy of credibility. Ad hominem arguments can be subtle or explicit. Dodds (1877-78), a localizationist, for example, said that "Goltz' results have not been confirmed" while Ferrier's results are "far more accurately described than Brown-Sequard's." Ferrier went further and questioned Brown-Sequard's data or his ability to interpret it:

> every physiologist who has seen paralysis produced by cerebral lesion, has seen it on the opposite side, with the single exception, I believe, of Brown-Sequard (1878b:10).

Ad hominem discrediting also appeared in a choice of words which added doubt to the actions of opponents. Ferrier used the following phrases in his criticism of Brown-Sequard (1876:233-35):

> Certain remarkable views advanced by Dr. Brown-Sequard...
>
> He professes to have collected...
>
> the paralysis from brain disease, he attributes, not as is usually held...

Ferrier's summary comment about the argument of Brown-Sequard's that Ferrier tried to discredit here was "I have little to add in the way of comment on these views, beyond the various facts and arguments adduced in the above chapter" (1876:233-35). Here he combined ad hominem argument with a truism or appeal to the obvious--"I rest my case," or an appeal to "what should have been obvious" by then to a presumed audience.

Panizza used _ad hominem_ argument to impugn the judgement of localizationists and to argue that their observations were biased by their hypotheses:

> In other words, the physiologist sees in these facts which always stay the same, what is convenient to him or what he chooses to see (1887:153).

But when referring to diffusionists, Panizza explained anomalous data as due to technical error or carelessness:

> since Goltz himself admits that it made defensive movements analogous to those of the frog to whose skin acetic acid was applied. We have repeated numerous times this experiment and have reached totally opposite results, which, in order not to negate Goltz's statements, forces us to think that he made the experiment only one time, and that in this time, as an exception, the shock of decapitation had immobilized the animal, or had greatly handicapped its sensori-motor reactions (1887: 153).

Another example of _ad hominem_ argument was given by Althaus (1880), who gave a psychological explanation for disagreements with localizationists:

> As at the present day any opposition to the main features of Broca's theory proceeds only from those who are wilfully blind or disingenuous, the least that the adherents of that doctrine have a right to ask for from their opponents is a thoroughly complete description of the cases brought forward, with the view of disproving the same (p. 63).

Here again there is a combination of tactics: _ad hominem_ (must be wilfully blind or disingenuous); truism (any opposition to the main features); and what Gerson (in prep.) calls "upping the ante": asking for a complete and detailed description and accounting from critics of localization.

6.3.3 Arguments from authority.

Another use of the hierarchy of credibility is to argue from authority--to use the ad hominem position to one's own advantage. These are attempts to use one's position (or claimed position) at the top of a hierarchy of credibility to give legitimacy to an argument apart from its technical merits. These claims can be made through a subtle use of language to describe one's own actions:

> Hughlings Jackson pointed out that certain convulsions were due to disease causing localized irritation on opposite sides...From such facts he came to the conclusion that... (Ferrier, 1876:126-27).

> Fritsch and Hitzig established that direct current on hemispheres caused movements (p. 128).

The phrase "pointed out that" casually conveyed that Jackson simply noticed a phenomena and was bringing it to the attention of the scientific public. Once he noticed these facts, he then inevitably came to a further conclusion about localization. Similarly, the term "established" in conjunction with the observations of Fritsch and Hitzig put their theory beyond a doubt.

6.3.4 Ignoring and censorship.

These are tactics which pass over arguments or criticisms or opponents, or ignore opponents themselves in terms of invitations to professional meetings, citations, etc.

Similarly, censorship invokes moral criteria to filter out opponents' results from publication or discussion. There is an implicit manipulation of the heirarchy of credibility here, because the censors identify themselves with that which has the highest moral status and is therefore true.

6.3.5 Sarcasm.

Sarcasm is a tactic which takes the results of a piece of work, argument, or challenge, and implies to an audience that this work is inferior by virtue of missing context; it's "out of place." Sarcastic tactics are often coupled with ad hominem arguments i.e., not only is the work "out of place" but the author is "out of it" and misbegotten.

Although in general the debate was imbued with an elaborate courtesy ("with all due respect to my extremely eminent colleague"), sometimes debaters became sarcastic. Brown-Sequard referred to his opponents as "these clever physiologists" (1890a), and Ferrier portrayed his opponents as making an obvious, rather foolish mistake:

> What, it is triumphantly asked, could more conclusively dispose of the view that the cortex is concerned in the results, seeing it may be removed without prejudice to them? Apparently, those who argue in this manner forget that there is a plurality of causes and conditions (1878b:18).

Here, the words "triumphantly" and "apparently" were used sarcastically to undermine the diffusionists' credibility, and the appeal to the obvious further tried to make those ideas seem ridiculous. Ferrier attempted to convey that his ideas are simply what everyone admits; Brown-Sequard's, by contrast, are beyond the pale.

Brown-Sequard used similar tactics. For example:

> _It is evident that_ hemiplegia as considerable as exists in the case, could not have been caused by the _insignificant_ expression that existed in the _pretended_ motor centers, and _one must admit_ that trephining was followed by recovery because it effected a cessation of the irritation (1890b:768; my translation, emphasis added).

He attempted to establish certainty and to diminish the importance of the influence of the areas localized by Ferrier et al., using words like "insignificant" and "pretended." "One must admit" is also a truism--an attempt to rely on common knowledge, an appeal to the evident, and a tacit attempt to _establish_ the evident.

Much of the stylistic conflict between localizationists and diffusionists was tacit. For localizationists, there was something fundamentally "right" in an aesethetic or stylistic sense about allocating each function to a discrete place in the brain. One result of different stylistic commitments was an exaggerated perception of the tenets of opponents' theories. This resulted in sarcastic or vastly simplified portrayals of the opponents' position.

Diffusionists tended to characterize localizationist work as "neo-phrenology," and localizationists as engaged in a absurd search for separate "organs" in the brain.

Similarly, localizationists depicted diffusionism in cartoon fashion. First, they confounded diffusionism with total collapse. Phenomena diffusionists saw as distributed through the body or interactive were characterized by localizationists as one big chaotic and confused mess. Second, localizationists characterized diffusionism as tautological. If everything is connected, they argued, then how can anything be said to cause anything else?

6.3.6 Advertising.

Advertising puts one's argument/results before an audience for the purpose of getting them to buy it, in a way that disparages opposition and claims to be the best deal. These two statements from localizationists and diffusionists are good examples of advertising:

> The earnest study of brain function (localization) can hardly fail also to lead to the elucidation of some of the most difficult and interesting physical, educational and social questions. From whatever side it may be viewed--with regard to the treatment of insanity, the diagnosis and treatment of nervous diseases, the solution of the questions of psychology, sociology, and metaphysics--an exact knowledge of the functions of the brain is indispensable (British Medical Journal editorial, 1881:823).

> We must admit that either half of the brain can will and regulate the movements of the limbs on the two sides, and that when disease exists in any part of the brain...the lesion can produce paralysis either in the corresponding side, and also that if paralysis is produced (I say if--because paralysis is not constant), it is not by the destruction of either the organ of the will or that of conduction between it and the muscles, but by an inhibitory or arresting (or suspensory influence exerted on distant parts by

> the irritation of the diseased fibres or cells). If we accept these views...then everything becomes clear and easy to explain (Brown-Sequard, 1873c:142).

In both cases, the advertisement for the position claims to solve a multitude of problems, to be exact or "clearly" true.

6.4. Organizational Tactics.

Organizational tactics are those which rely simply on the strengthening of organizational relationships (or conversely, weakening other relations) in order to put an argument across. Tactics here include:

6.4.1 Uniting against a common enemy.

Here the union of two groups of scientists against a common enemy means that attacking the argument of one group becomes an attack on its ally. This union disregards the intellectual connections between the two arguments. Similarly, the grounds for enmity become generalized to all work done by "the other side."

What came to be seen as logically necessary connections, as discussed in Chapter 1, are thus formed through intersections or professional alliances. As we saw in Chapter 2, these connections influence the acceptance of arguments, and thus become debating tactics.

6.4.2 Selective enforcement of standards.

The standards of good scientific practice can be selectively enforced by those in power (referees, hiring committees, journal editors, reviewers). This tactic is important in legitimating or delegitimating arguments.

6.5 Manipulating Focus.

These are tactics which push favorable evidence up front and deemphasize unfavorable evidence. The foreground/background relationship of data, or the significance of certain findings may also be manipulated. [5] The tactics here include:

6.5.1 Shuffling domains of evidence.

This means answering challenges or dealing with anomalies by substituting or alternating between domains of evidence (see also Chapter 3). Ferrier, for example, makes the following statement in the introduction to his influential 1876 exposition of localization, The Functions of the Brain:

> experiments on animals, under conditions selected and varied at the will of the experimenter, are alone capable of furnishing precise data for sound inductions as to the functions of the brain and its various parts; the experiments performed for us by nature, in the form of diseased conditions, being rarely limited, or free from such complications as render analysis and the discovery of cause and effect extremely difficult, and in many cases practically impossible (p.xiv).

Horsley contrasted surgical data with postmortem data on similar grounds: "Post-mortem records can never teach what the careful study of the living tumours exposed in an operation can demonstrate, since in almost every case the former condition is practically what we may term inoperable" (1906:411). Campbell makes a similar argument for the use of histological data, where physiological and clinical data are seen as imprecise (1905:xi). Yet all three used clinical, surgical, postmortem pathology and physiological experiments to argue for localization.

In all of these examples, overlapping domains of evidence are tapped in order to _circumvent_ the theoretical problems posed by the complex, interactive nature of the brain. In the context of the debate between localizationists and antilocalizationists, this may appear as "passing the buck" or _pro forma_ nods to artifacts recognized by both sides. Yet it is vital that we remember the context of work forming these debates, as described above in Chapter 3. In order to proceed with work, such shuffling and simplification was necessary.

6.5.2 Substituting reliability for validity.

The substitution of reliability for validity was a response by localizationists to problems of uncertainty faced on the daily level. This substitution also appeared as a response to technical criticisms of localizationists.

Throughout the debate, there were arguments about the technical adequacy of localizationist or diffusionist experiments and techniques. At times these arguments were used to attempt to discredit one stance or another; at other times they simply appear as arguments within a position about the correct way to go about business.

One common technical artifact raised by both diffusionists and localizationists concerned the use of electrical current (Ferrier, 1878a; 1878c). Burdon-Sanderson, for example, attacked the conclusions of Fritsch and Hitzig "demonstrating to his satisfaction that the results were due to spread of current to the striatum then considered a motor ganglion". (Walker, 1957; Burdon-Sanderson, 1873-74). Hitzig similarly criticized Ferrier's results as being "vitiated by diffusion currents" (Dodds, 1877-78), as did Carville and Duret, two French physiologists (Duret, 1878).

This criticism was later tested by two Italian physiologists, Luciani and Tamburini, who, according to a report in Brain (Rabagliati, 1879:535), determined that, "It is absolutely impossible that movements produced by electrisation of the cerebral cortex can be due to diffusion of the current to the dura mater." They put glass between the cortex and the dura mater (brain covering), electrified both, and measured the difference. But other criticisms, such as that raised by Foster (1877) and Burdon-Sanderson, stated that perhaps the currents were electrifying structures deeper in the brain, not the dura mater (Dodds, 1877-78).

Two dimensions of this technical debate are important. First, the object of inquiry is itself labile, reactive to electrical current in some unknown degree, and <u>not uniform in shape</u> between subjects. Thus, the debate about how far the current extends, what kind of current the brain reacts to, and what is a "response" as opposed to what is an "artifact" involves managing these various uncertainties.

But while diffusionists struggled to make the artifact significant and thus to impeach localizationism (on grounds of <u>validity</u>), localizationists successfully manipulated the focus of the argument on issues of precision and <u>reliability</u>. The technical arguments raised by diffusionists were converted to internal procedural arguments.

Arguments within a position can serve to strengthen that position when basic assumptions remain unquestioned. In Gerson's terms, focus on a number of second-order problems and on technical details can force a <u>de facto</u> resolution of nonresolvable stylistic or political commitments (in prep.). Such is the case with several of the technical artifacts raised in the localization dispute, especially those, like electricity, whose uses were an unquestioned part of the larger context of medical practice. The management and manipulation of focus here was important in accomplishing this substitution.

6.5.3 Uses of the unknown.

Another focus management tactic was using the unknown to favorably support one's side. For example, Ferrier, discussing the problem of indeterminacy of lesion-function relationships, attacked Brown-Sequard for using the unknown to establish his viewpoint:

> Owing, however, to the fact, as Hughlings-Jackson has remarked, that "the damage by disease is often coarse, ill-defined, and widespread," the determination of the functions of the brain by the clinico-pathological method had made comparatively little progress, there being apparently no constant uniformity between the seat of the disease and the symptoms manifested. The difficulty of discriminating between the direct and indirect effects of cerebral lesions had furnished Brown-Sequard with arguments in favour of his peculiar views, that all the symptoms of cerebral disease are due to some dynamic influence exercised by the lesion on parts situated at a distance (and always apparently out of reach), which are credited with the functions lost or otherwise disturbed (1890:15-16).

"Always apparently out of reach" is Ferrier's sarcastic objection to Brown-Sequard's uses of the unknown. Brown-Sequard posited an influence "at a distance" whose nature or location is unknown; localizationists referred to unseen but probable tumors. Both used these unknown or unverifiable entities to prove a point. This was not simply speculation, in the sense of propositions that might have been true. Rather, it extended a conceptual framework to unprovable realms. Brown-Sequard gives an example of this in localizationist work:

> <u>Lesions of a very small part of the brain often produce very marked symptoms</u>...It is evident that the old theory of production of symptoms of brain disease (localization) is absolutely unable to explain such facts. The only way to get out of the difficulty is to suppose that the autopsy has not been well made, and that a lesion, not discovered, existed somewhere else than the one found, and that this supposed lesion was the cause of the symptoms. It

is, of course, quite possible, if not certain, that real organic lesions have frequently escaped notice; [6] but it would be a purely gratuitous supposition to admit that it always has been so in the very large number of cases in which only a small lesion has been found in the cerebral lobes in the bodies of persons who had had decided symptoms of brain disease (1873b:254-55).

And Ferrier attacks Goltz for the same type of logic:

> The results of ablation of the cerebral hemispheres indicate nothing for or against the doctrine of functional localisation, nor do the experiments of Goltz in the least degree militate against the existence of specific centres; for if, even after complete bilateral extirpation of these centres, the functions which survive do not transcend those capable of being manifested in the entire absence of the cerebral hemispheres, there still remains the question whether the lesions have not caused a loss or paralysis of something of a higher order. That this is so is capable of ample demonstration, of which not the least part has been contributed by the very facts which Goltz himself has ascertained through the numerous and varied devices which he has so ingeniously contrived. It is no explanation of the defects which admittedly result from removal of the cerebral hemispheres to say that they are caused by a loss of intelligence. This is merely restating the facts in a more metaphysical but less intelligible form. We are not, however, dealing with metaphysical terms when we are studying the effects of lesions of the cerebral cortex. We are dealing with material entities connected with sensory and motor tract, and it is our object to determine, if possible, what are the anatomical and physiological factors which are correlated with the functions which we generalise under the head of intelligence....(1890:13).

Both sides here accused each other of the same sin: using the unknown to resolve anomalies or extend frameworks. Such uses were labelled "metaphysics" by Ferrier and "gratuitous supposition" by Brown-Sequard.

Another form of manipulating focus included emphasizing inconsistencies or uncertainties in opponents' data or procedures, while ignoring those in one's own. These may have been inconsistencies or uncertainties to which the opponent admitted, but the tactic emphasized them as more significant than the positive results obtained.

This was common when the diffusionist-localizationist debate included demonstrations. In nineteenth-century medicine and physiology, arguments were often supplemented with physical demonstrations. Such demonstrations may use all of the tactics of a dramatic setting. An example from clinical work vividly illustrates the focus management aspects of demonstrations:

> His [Gowers'] teaching clinics soon became thronged with physicians from all over the world standing in the gangways and straining the capacity of the accomodation. Gowers was able to combine teaching with out-patient consultations, and he set the practice which has been followed ever since. He was not aware beforehand of what cases were attending and he saw the patient for the first time in the presence of a critical and admiring audience. He took the history, examined the patient, and afterwards discussed the nature of the case, the differential diagnosis, prognosis and treatment. There was no set talk; no questions or answers...If clinical cases were to be shown, careful arrangements were made beforehand with his house physician. Gowers was well aware of his skill as an instructor, and was even guilty of a certain justifiable vanity in respect of his powers of attracting large audiences (Critchley, 1949:81-82).

Here, the patient's disease was used to instantiate a taxonomy of disease based on localization theory (see e.g., Gowers, 1885). As with most forms of instantial argument (example-giving), there was little time or space for rebuttal or counter-argument on an analytic level. The focus in this case was managed so that all diagnoses appear successful and supported the taxonomic framework the Queen Square physicians were developing.

Another kind of demonstration occurred at medical congresses and other professional meetings. A famous confrontation and demonstration occurred between Ferrier and Goltz at the 1881 International Medical Congress held in London (MacCormac, 1881). It included an exhibition of the animals experimented on by each. Each was literally given the stage for a set period of time, and each had to make their point offering physical evidence.

Goltz brought dogs whose "motor areas" had been ablated. These decorticate dogs retained the ability to move, bark, and interact with humans. Goltz' contention was that if the motor areas were where Ferrier said they were, then the dogs without them shouldn't be able to move around. Ferrier, on the other hand, brought several members of the audience to his laboratories across town to view his monkeys who had <u>their</u> motor areas removed, and who were now hemiplegic or otherwise disabled (see Thorwald, 1959, for a vivid description of this scene; also MacCormac, 1881 for full proceedings of the Congress; and Spillane 1981:393-397; see also Appendix B for a translation of Goltz' remarks).

The animals were later killed, and a blue-ribbon committee of anatomists and physiologists (including Gowers and Schafer) determined if the dogs were indeed decorticate, or if the monkey's injuries were in fact limited to the areas Ferrier claimed as damaged or Goltz claimed as removed (Klein, Langley, and Schafer, 1883-84).

A report of this investigation was published two years after the conference in the Journal of Physiology. This delay probably reflected the slowness of the dissection process at the time, and the fact that it took nearly eighteen months to fully examine a brain histologically.

But a critical aspect of convincing an audience is exactly the length of time it can take to refute a demonstration or instantial argument of this form. In this case, Ferrier had the upper hand in several ways. When the proceedings were published a few months after the conference (MacCormac, 1881), Goltz' remarks were not translated from the German, either in the proceedings, or, presumably, at the conference itself. Ferrier's animals were demonstrated last, after Goltz had presented his dogs. Thus, the "final word" was Ferrier's.

6.6 Arguments for Status Quo

Another important debating tactic is arguing for the status quo. These include claims that changing procedures, or disconfirming results would simply affect too many, and/or too precious resources. This tactic often shades into arguments from absurdity or appeals to tradition, or appears in conjunction with them. For example, this review from _Brain_ characterizes Brown-Sequard's work as denying "all scientific progress":

> The hypothesis of Brown-Sequard, who denies the possibility of localising in the cortex or any other part of the brain functions of any kind, besides being a denial of all scientific progress, is contradicted by experimental and clinical results, and the facts brought to sustain his thesis are not of a kind to destroy the doctrine of localisation. (Rabagliati, 1878c:145).

Another example of this tactic appears in the following criticism of Brown-Sequard made by Ferrier:

> Brown-Sequard, however, holds that, in all cases of paralysis from cortical lesion, there is some intermediate link or _tertium quid_ intervening between the antecedent and the consequent: a kind of inhibitory influence exerted by the lesion on some centre or centres which are credited with those functions which are lost. It would, I think be easy by parity of reasoning--and I say it with all due respect to the distinguished author of this theory--to make a complete _reductio ad absurdum_ of the whole of experimental psychology. We should never be entitled to infer direct relationship between organ and function, but be condemned to a perpetual search after some _tertium quid_, which, like an _ignis fatuus_ [7], would for ever elude our grasp (1878b:41-42).

Ferrier invoked this argument selectively. Foster had made a similar point in his famous 1877 textbook, where he cautioned against too-simple causal interpretations of function loss-area damage, because of inhibition, which he saw as easily confounded and not well understood (p. 446).

Yet Foster agreed with localization, and _his tertium quid_ was in accord with localization theory and the logic of deletion. So Ferrier is invoking this argument selectively.

This debating tactic, like many others, had an elliptical ending. It may have been "wrong" to bring in a _tertium quid_ without specifying the nature of the intervening relationship. However, that did not in itself prove that the _tertium quid_ did not exert an influence; only that certain rules of _argument_ had not been followed. Ferrier here confounded the rules of the arguments with the validity of the data, a common and interesting phenomenon in scientific arguments. The variability of invoking this debating tactic is illustrated by the fact that he inveighs against Brown-Sequard and not Foster.

7. _Modes of Debate and Tacit Debates_

This chapter has presented several debating tactics used by localizationists and diffusionists to argue for their positions. The tactics were employed in several different modes. Each of these types of arguments was tied to the types of work done by localizationists, and defended from that work. _Instantial_ arguments have been discussed here and in Chapter 3. Another type of argument was by _comparative taxonomies_.

Instantial arguments proceed by filling in slots in a framework. For physiologist/physicians trying to prove localization theory and map out the regions of the brain where various functions were located, this meant filling in regions on maps of the brain. The instantial maps were argued for by matching movements or disturbances of function with areas in the brain (e.g., Ferrier, 1873-74; 1876).

The "filling in" activity itself became the argument. For instance, Beevor and Horsley (1890; 1894) did a series of experiments on the monkey in which extremely small areas of the monkey brain were exposed to electrical current. Movements by the monkey were then recorded and areas correspondingly labelled. Accompanying the text of the article were plates with representations of the monkey brain, shaded in for various localized regions of the cortex. The explicit part of this writing gave information about the movements of these monkeys after being exposed to electrical current. The implicit argument is that the map of their brains, already extant, was simply <u>filled</u> <u>in</u>.

The development of these instantial maps was a major enterprise for localizationist physiologists (Young, 1970:234). The need for the maps and their clinical uses were discussed in the last chapter.

Beevor and Horsley's first experiment was attacked by antilocalizationist Brown-Sequard, in an article entitled "Proofs of the Insignificance of a Celebrated Experiment by

Victor Horsley and Beevor on the So-Called Motor Centers" (1890a). [8] He offered an alternative explanation for the movements made after the electrical current is applied. Just because an electrical current in x area made the thumb move, he argued, we can't conclude that that part of the brain determines thumb movements; similarly, you can tickle the face muscles and always get a smile--if you used the same logic as Beevor and Horsley, then the "smile center" would be in the facial skin. He said:

> All reflex movements in the organism would furnish a similar argument against the value of the fact presented by the English physiologists (1890a:199, my translation).

Here, Brown-Sequard uses sarcasm to make his point. Later in his article he states:

> All the value of the facts of irritation in the so-called motor region of the cerebral cortex, after the experiments of Fritsch and Hitzig, Ferrier, Horsley and Beevor and of others, are certainly annulled by the experiments demonstrating that the same part of this region on the other side of the brain is capable of producing, under the same conditions, identical movements (1890a:201, my translation). [9]

Brown-Sequard's argument here was the same as the argument used by localizationists to explain the anomaly of why sometimes functions are retained after destruction of an areas thought to be responsible for the function. He postulated two areas (or multiple areas) capable of producing movement. If this was so, then why did destruction of both these regions sometimes not cause paralysis? The answer given by Brown-Sequard was that effects are not localized in the motor cortex, but rather can be caused at a distance in the nervous system, and be either inhibiting or action-producing.

Brown-Sequard was trying to prohibit the localizationists from moving down an instantial hierarchy and simply fitting in their examples into a presumed map. He did this by questioning _the nature_ of the hierarchy. He provided an alternative explanatory framework for the phenomenon, as well as employing many of the tactics described above (sarcasm, use of truisms, _ad hominem_). The tacit framework is thus challenged and made explicit.

Brown-Sequard's challenges, as well as those of Panizza and Goltz (see Appendices), were not successful in the sense of getting the localizationists to change direction or abandon their enterprises. Yet they had an important effect on the content of the localizationist perspective--an effect which has been virtually ignored by historians of neurophysiology.

The criticisms raised by diffusionists _were_ met by localizationists. Sometimes they were assimilated, sometimes directly rebutted, sometimes diplomatically "insulated." But in the process of meeting the criticisms, localizationists developed a powerful technology for arguing.

Arguments are not usually thought of as technologies, yet they perform many of the same information-handling functions as scientific instruments. As localizationists argued with diffusionists, they confronted anomalies, irregularities, and had to argue about significance of certain phenomena. As this became routine, pools of argument developed with which localizationists could handle classes of anomaly. Inconstant relations between area and function, for example, became a well-argued point. Instead of being a persistent uncertainty in daily work, it was transformed into a point of doctrine for localizationists. They had ready and packaged formulas to deal with inconstant relations, created as a result of arguments with diffusionists.

Thus, in addition to daily work and institutional concerns, the shape of localization theory was developed through conflict with antilocalizationists. In simplest form, this thesis argues that localizationists "won" the debate because they controlled more resources and linked their arguments to successful enterprises. But winning the debate is only one part of the analysis of the development of the perspective.. The actual content of the final theory was shaped in a far more dialectical fashion.

8. Summary: Debate and Perspectives

The dynamics and strategies of debating described in this chapter contributed to the evolution of the successful perspective (with inertia, momentum, cumulative reification, progressive inseparability of parts of the perspective, and incompleteness at any one point) in the following ways:

1. Inertia. The tacit basis of instantial arguments, and the strong link between clinical taxonomies and clinical programs heavily contributed to the inertial properties of the perspective. In addition, the slow accumulation of cases to support localizationist arguments were well entrenched in canonical medical forms and training programs, and difficult to refute on the case level. By gradually piling up examples, localizationists created an inert pile of evidence that was difficult to move.

2. Momentum. The orchestrations and public demonstrations, with no chance for rejoinders or counter-evidence, contributed to the momentum of the perspective. Public relations and "advertising," as noted above, helped it spread quickly as well.

3. Incompleteness. The partial assimilation and transformation of diffusionist arguments, and the confounding of different aspects of the debate contributed to the "incompleteness at any one point" aspect of the localizationist perspective. The arguments were slippery and complex, with unclear boundaries around positions. Therefore a comprehensive characterization of it from within was impossible.

Artifact bows, and shuffling artifacts from one realm to another also contributed to the incompleteness at any point of the perspective. Seeming agreement with points raised by diffusionists, and the fact that artifacts were not the responsibility of any particular line of work (but they were recognized) made it difficult to fully characterize the perspective from any one viewpoint.

4. Cumulative Reification. *Ad hominem* arguments and the development of standardized "pools" of argument helped cumulatively reify the perspective by obscuring contradictions, procedural and methodological subtleties, and the valid parts of conflicting opinions. The oversimplified "cartooning" of each side by the other also reified the localizationist approach.

5. Progressive Inseparability of Parts. The conflation of different aspects of the debate (such as vivisection with the validity of localization, see Chapter 2) helped make the parts of the localizationist perspective progressively more difficult to separate. In addition, using the unknown to extend the perspective into future or untestable realms often resulted in creating necessary "logical" connections between parts of the perspective, which were then legitimated at a later time.

FOOTNOTES

1. For contemporary versions of this criticism see Glassman (1978); Laurence (1977), Wimsatt (1976), and Klein (1978).

2. William James was an American philosopher and psychologist whose Principles of Psychology provides a lively critique of this aspect of the work of the localizationists. James, like Panizza, saw problems with the conceptual foundations of strict localizationism, especially as it conceived of behavior. James saw all behavior as contextual, and as acquiring meaning in light of goals and interactions (1890). If functional control of behavior was rooted in anatomy or in a static location, one would encounter serious epistemological problems with determinism, idealism, and non-empirical systems of understanding which could not be verified in experience. I do not include James as a diffusionist, here, however, because he also agreed (in what seems to me a contradictory way) with many of the localizationists' positions, especially about sensory and motor localizations. A study of the development of the physiological positions of James and other Pragmatists, and their relationship to other developments in medicine and physiology at the end of the nineteenth century would be fascinating.

3. There is not space here to discuss this interesting

facet of the debate in detail, but perhaps future research will trace this development. The work of Sherrington, for example, was strongly affected by the work of Goltz, although Sherrington transformed Goltz' ideas into a localizationist model. The themes of inhibition and dynamogenesis are familiar to generations of researchers on the nervous system--since Bernard, at least--and the history of these concepts would contain rich information on assimilation and transformation of concepts between schools or researchers with different stylistic commitments.

4. Gerson, personal communication.

5. For a good discussion of a contemporary controversy about significance, see Morrison and Henkel, 1970.

6. I would almost certainly agree with Brown-Sequard here. As discussed in Chapter 3, autopsies and postmortem examinations were often hurried and inexpert. Relatives were reluctant to allow postmortems, and although dissection had been legalized in 1838, it was still quite stigmatized. Horsley, for example, had to make arrangements with a porter at the hospital where he worked to use <u>sub rosa</u> methods to obtain cadavers (Lyons, 1966).

7."Will-o'-the-wisp".

8. "Preuves de l'insignifiance d'une expérience célebre de MM. Victor Horsley and Beevor sur les centres appelés moteurs."

9. Toute la valeur des faîts d'irritation de la zone dite

motrice de l'écorce cérébrale, d'après les expériences de Fritsch et Hitzig, de David Ferrier, de Horsley et Beevor et tant d'autres, est certainement annulée par des expériences demontrant que la même partie d'une cóté de cette zone est capable de produire, suivant les circonstances, des mouvements identiques.

CHAPTER FIVE:

COINCIDENT BOUNDARIES, DIVISIONS OF LABOR AND THE MIND-BODY PROBLEM: PARALLELISM AND LOCALIZATIONISM

1. Introduction

This chapter describes the development of the philosophical position of parallelism in localizationist neurophysiology. Parallelism is the doctrine that the mind and the body operate as two separate but parallel realms. This idea predated the localizationists--the work of Descartes is commonly cited as exemplary of its beginning, and there are earlier examples as well. But parallelism was integrated into modern neurophysiology and technology by the localizationists.

There have been perhaps thousands of articles and books on the subject of the relationship between the brain and the mind, or the mind and the body, since the beginning of the period studied here (see for example Ryle, 1949; James, 1890; Sperry, 1980; Smart, 1955; Eccles, 1982; Smith, 1959; MacKay, 1978). The area is enormously complicated, at least in part because of overlapping terminologies and confoundings between philosophers and interpreters of neurophysiology. It is not within the scope or intent of this thesis to recapitulate those arguments, nor even to try to give a summary of the range of positions. [1]

Rather, my concern here is with the <u>origin</u> of the ideology of parallelism in the work arrangements of neurophysiologists, and with the subsequent _persistence_ of the debate about the nature of the relationship between the mind and the brain. The point of this chapter is to show that localizationists chose parallelism as a philosophical ideology because of constraints imposed upon them by their work, especially collaborative work.

Once committed to parallelism, they had to face certain criticisms from philosophers of science and other researchers (A. James, 1881). In facing these criticisms, they constructed temporary solutions which allowed them to continue with their work, but which never met stringent philosophy of science standards. These temporary solutions, or _plausible bridges_ between the mind and the brain, were "makeshift epistemologies." That is, the theories about knowledge developed by the localizationists worked long enough and well enough to allow them to plausibly continue writing about the mind and the brain, but they were makeshift in the sense of not standing up to philosophical scrutiny.

2. _Origins of Parallelism in Work Situation_

In Chapter 3 I discussed the development of _triangulation biases_ which arise when two or more ways of measuring the same phenomenon are combined. Another result of triangulation is that the boundaries of the phenomenon as established by the several lines of work are presumed to coincide. In this case, that means that surgeons, neurologists, pathologists and physiologists were all addressing the problem of localization of function in the nervous system. As their results were used to legitimate one anothers' findings, a common boundary for the functions they addressed was tacitly established.

They worked on various phenomena. Neurologists dealt with paralysis, epilepsies, and various other disordered behaviors. Surgeons tried to find physical locations of tumors, and to understand enough brain anatomy not to do serious damage inside the skull. Physiologists dealt with movements, and minute correlations between application of electrical current or deletion of an area and a movement. Pathologists, like surgeons, dealt with the physical characteristics of the brain, and with the differential distribution of various kinds of cells, or the composition of tumor cells.

As localizationists worked together from these different fields to make their arguments for localization of function, they created a tacitly identified common boundary for the phenomena addressed by their several lines of work.

This boundary was the skull. The skull was the physical boundary faced by surgeons and physiologists, who had to cut through it; the informational boundary for neurologists, who had to guess what was inside of it and where; and a boundary which demarcated data for pathologists, who were supplying information about the interior of the brain to neurologists and physiologists.

For localizationists, then, the skull became the edge of the mind, and bounded its functions. While localizationists said that there was a distinct difference between mental and physical realms, the triangulation of their work results forced them to act as if these realms had a common boundary. [2]

Yet there was no doubt that physical realms were preferable to mental realms for work purposes. Mental phenomena were messy, imprecise, and not easily discovered or replicated. As Hughlings Jackson said:

> Our concern as medical men is with the body. If there be such a thing as disease of the mind, we can do nothing for it (1932b:41).

Wherever possible, localizationists opted for an explanatory primacy for the physical realm. The brain caused the mind, and not the other way around. Yet mental events could not be dismissed altogether, for they often formed the immediate clues to nervous disease found by the neurologists (see e.g., Ross, 1882a; Crichton-Browne, 1872).

Particularly interesting here is the study of aphasia, often a symptom of brain disease. The inability to speak may be simply physical, as with impaired vocal cords. But when the vocal cords were not damaged, localizationists saw aphasia as a type case of a mental phenomenon caused by physical impairment in the brain (e.g., Seguin, 1881). Furthermore, there were many kinds of aphasia (Riese, 1977; Head, 1926). Some kinds interfered with ability to understand words, others with ability to produce words. Still others were classified as impairments of the ability to talk to oneself, but not the inability to talk to others.

Localizationists sought to correlate these subtly different kinds of mental impairment with physical damage or brain tumors. As evidence from different realms was slowly accumulated, localizationists developed a doctrine of strict psycho-physical parallelism, or the idea that the brain and the mind operate in tandem but are completely separate realms. As Engelhardt (1972) states:

> The choice between these options was important, for it would define what factors and indeed what "facts" would be of immediate significance to neurology. For example, a materialism would dismiss the importance of psychological events, while an interactionism would require such events to be acknowledged as causal factors. But a theory of the concomitance of mind and body would allow one to attend to purely physiological explanation without denying the significance of psychological reality. In developing his notion of neurology, Jackson considered that he was making a pragmatic choice between these options (p. 23).

Jackson's claims for the separation of the two spheres are quite strong, and are based on the reliability of the physiological parameters: "...The trustworthy symptoms in the diagnosis of actual and primary disease of the organs of the mind are physical, and the untrustworthy symptoms for that diagnosis are mental" (1875:492). Most explicitly, he says that:

> The doctrine I hold is: first, that states of consciousness (or, synonymously, states of mind) are utterly different from nervous states; second, that the two things occur together--that for every mental state there is a correlative nervous state; third, that, although the two things occur in parallelism, there is no interference of one with the other. This may be called the doctrine of concomitance (1932c:72).

In opting for parallelism or concomitance as a philosophical position, localizationists found a doctrine which supported localizationism and most importantly, explained the data in a way that allowed them to carry on their clinical and physiological work.

Another important consequence of the adoption of this doctrine, however, and one that has had a lasting impact on neurology in terms of philosophical quarrels, was that localizationists found themselves in an old and tangled arena in the philosophy of science about the relationship between the mind and the brain. And not only were they _in_ the arena, but suddenly found themselves a focal point for it (Calderwood, 1879). There is not space here to review the history of the entire arena, but localizationist neurosurgery and neurology provided new information to debate and a powerful empirical base for explanations of parallelism.

Still, localizationists for the most part were not philosophers, but rather, doctors. They adopted parallelism from the contingencies of their everyday work, but found themselves facing rigorous questioning, and, sometimes, opposition from philosophers of science (Ireland, 1879). On the other hand, philosophers of science lacked empirical data and strong institutional bases, most especially a clinical program.

The adoption of parallelism, the scrutiny of philosophers, the lack of empirical alternatives from philosophers, triangulation biases and the complex debates about localization created a philosophical deadlock which has persisted to the present day in the neurosciences (see e.g., Wimsatt, 1976; Laurence, 1977; Sperry, 1980; Eccles, 1982; Schmitt and Worden, 1979, and Schmitt, 1979 for reviews).

3. Explaining the Contradictions

Localizationists recognized that material and immaterial realms could not be posited as causing action in one another without philosophical difficulties. They also recognized that "correlation is not causation," although they sometimes used correlation as proof.

The major class of loopholes which needed to be explained, conceptually caused by parallelism, was how the two realms (mind and brain, or mind and body) were brought together, and by what mechanisms they were made to operate in tandem. Again, keeping in mind the philosophical and evolutionary biology audiences localizationists were addressing, and the great uncertainty and technical difficulties they faced in everyday work, it is not surprising to find that their responses to these problems were neither unified nor consistent within writers.

Localizationists adopted three kinds of strategies to address the philosophical problems of parallelism. These were:

1. _Refer philosophical problems to an expert within their ranks_ who understood the daily work concerns, but who would speak as a philosopher for them. The person elected to do this was John Hughlings Jackson--eventually a "symbolic leader" (Klapp, 1964) for the localizationists.

2. Develop _plausible bridges_ between the realms of the mind and the brain which would act as "good enough" theoretical explanations, or "bridges" between the two.

3. _Jettison problems which could not easily be addressed by some physical/medical aspect of localization into "mind"_ lines of work (psychiatry and other mental health areas), thus reinforcing the parallelism on an organizational level but jettisoning many of the epistemological problems through a division of labor.

3.1 _Hughlings Jackson as Symbolic Leader_

Symbolic leaders (Klapp, 1964) are those who are chosen by members of a social world to represent one aspect of a solution, or to embody or represent one kind of activity or position in a conflict. Such a figure's name or image is vested by members with leadership for a viewpoint or debating stance. As Strauss discusses in his work on images of American cities (1961), a composite image is formed from a social world which comes to have the quality of a stereotype.

Hughlings Jackson came to be seen as "the great unifier" by localizationists. So pervasive was this designation that it has persisted to the present day in the form of testimonial articles, references, and honorifics accorded to Jackson ["the father of neurology"; "the most brilliant neurologist the world has ever seen"; "a goldmine of theoretical astuteness" (Levin, 1965; Lassek, 1970; Smith, 1982; Angel, 1961; Critchley, 1960a; 1960b; Bishop, 1960; Brain, 1957)].

Jackson's work was dedicated to the eradication of the loopholes caused by the adoption of parallelism. His writing is thick, convoluted, full of metaphors and images (Mitchell, 1960). He was the oldest of the important localizationist theorists, and in many ways was the key bridge between the older diffusionist ways of thinking and the new localizationism (Greenblatt, 1970). He had been a student of Brown-Sequard's, and while he disagreed with

Brown-Sequard about localization, could still address issues raised by diffusionists. He said, wryly, of him-self, "I am neither a universaliser nor a localiser...In consequence I have been attacked as a universaliser and also as a localiser. But I do not remember that the view I really hold as to localisation has ever been referred to" (Jackson, 1932b:35).

Jackson is perhaps best known in medicine, then and now, for his work on epilepsy. He provided detailed descriptions of epileptic fits, and was important in collecting clinical details on the disease. His theory about epilepsy, as for the nervous system as a whole, was that epilepsy represented in perfect ordinal correspondence the process of Dissolution which was the opposite of evolution (see e.g., Jackson, 1932b and 1932c). The concept of Dissolution was directly borrowed from Herbert Spencer, and Jackson claimed to be a staunch Spencerian throughout his career.

Use of the language of Spencer proved important in cementing alliances and making plausible arguments to evolutionary biologists. Localizationist and evolutionary theorists had become political allies in the antivivisection debates and trials of the 1870s (French, 1975), and had also formed professional alliances in the Physiological Society, originally founded in 1876 to combat vivisection

(Sharpey-Schafer, 1927; see discussion in Chapter 2).

Historians of neurology have often referred to Jackson as exclusively influenced on a theoretical level by Spencer [(e.g., Head (1926:31), who states that "Jackson derived all his psychological knowledge from Herbert Spencer"]. Yet Jackson's medical methods and many other aspects of his theory derive from assumptions and training practices prevalent in British medicine. He was a student, while at York Medical School, of Thomas Laycock (Lassek, 1970).[3] Laycock's Lectures on medical methods (1857) provides clear methodological and theoretical precedents for Jackson's work. Throughout this volume, Laycock stresses the order of events as of primary importance in medical diagnosis, a concern later reflected in Jackson's observations of the sequence of events in an epileptic seizure. Laycock, too, stresses that disease is a deviation from a normal state, to be measured in extent of deviation. For Laycock,

> a theory is a means to an end only, namely, the progressive discovery of the order of events... the observation of the order of succession of phenomena is the foundation of etiology....(Laycock, 1857:50, 81).

An important difference between Laycock and Jackson, however, is that Laycock was concerned with environmental factors in disease. He discusses the importance of seasonal, meteorological, cyclic and temperature changes (1857:129-32). By Jackson's time, some fifteen to twenty years later, these concerns had pretty much dropped away. The organism-environment distinction had become more sharply drawn in medical practice.

Jackson claimed to be a strict parallelist. His "doctrine of concomitance" states that the brain or nervous system and the mind are two separate realms, which act at the same time but do not touch. Further, based on the contingencies of work (the proper sphere of medicine, as he says above), the only proper realm for doctors to investigate was the physiological side of things.

But Jackson's claims to strict parallelism, and his attempts to demarcate a sphere of investigation that was only physiological, were not successful (Engelhardt, 1975). Jackson was forced to address philosophical issues of links between the realms he distinguished. Because he did so so thoroughly, and in a way that touched on many other burning questions in neurophysiology, his work became the standard reference on the problem for localizationists. His status as a symbolic leader derived from this and from his central participation in events at Queen Square.

3.2 Plausible Bridges

There were three types of response to problems of parallelism from localizationists, all of which "bridged" mind/brain realms. These were:

1. Concomitance theories which were based on correl-

ating activity between the two realms and lodging the connection in temporal simultaneity. These theories included development of the idea of substrata, either anatomical or physiological, which underlie functions or behaviors. The major philosophical difficulty here was finding out the nature of mechanisms of connection, and of learning (Obersteiner, 1879).

2. Building block theories attempted to find "basic units" whose assembly would constitute either brain or mind. We find primarily here those reflex theories which saw actions as built up of sets of particulate reflexes. These theories also had to deal with problems of storage of nervous impulses and coordination of basic units.

3. Representation theories tried to link the realms of mind and brain by having one "stand for" and indicate or imply the other. These theories were the most abstruse, and Jackson's main works relied heavily on this concept. The gap between mind and brain was bridged in these theories by a kind of calling back and forth, or imaging, between realms. They included theories of ordinal representation, automaton theories, and some muscle sense theories. They also, via representation, often addressed problems of consciousness and unconsciousness. A major source of difficulty for these theories was how to account for mechanisms of perception between or across realms.

A practical concern about the usefulness of concomitance often appeared in the early volumes of the localizationist journal Brain (founded 1878), where writers stated that even though philosophically the connection between the realms of brain and mind was impossible to understand, concomitance allowed them to get on with their work. [4] This example from Cappie (1879:373) is typical:

> In studying the causation of mental phenomena we need not be deterred by the fact that our knowledge, at the best, is likely to be always remote. It may not be the less positive so far as it may pretend to go. We must ever be content to accept the fact of consciousness as ultimate. The physiologist will never be able to perceive why the brain's activity should be associated with its manifestations. Yet, if a correlation be assumed, he may be able to determine why one form of consciousness rather than another is present; why it is present with a felt amount of intensity; or why in certain circumstances it is obscured or suspended. The nexus between molecular activity of brain and activity of thought and volition may forever remain "unthinkable," but many of the modifying conditions of the molecular activity may certainly be determined, and the remote but necessary influence of these on the mode and intensity of mental action may thus to some extent be recognized.

A review of G.H. Lewes' The Study of Psychology in the same issue (Benham, 1879:392) makes the same point:

> It is necessary to adopt a new method of research--to translate the facts of consciousness into terms of another class...The new method follows from the assumption, for long tacitly, though fitfully, made by all, that organic and mental processes are strictly correlated; so that a complete analysis of the one would give an equally complete acquaintance with the other: and then, while some terms of the

> one series may be beyond our reach, the corresponding members of the other series may be accessible.

Another corollary to the doctrine of concomitance, and perhaps its most important outcome in terms of the subsequent history of neurophysiology, was the establishment of the idea of an anatomical substratum which somehow (and the mechanism or bridge proposed varied considerably) "gave rise to" behavior or mind. The practice of sliding over the subtler philosophical problems of the connection became institutionalized in neurophysiology at this time. Philosophical questions gave way to working assumptions about substrata (see e.g., Benham, 1880; Ferrier, 1880). For example, in Ferrier's Functions of the Brain (1876) [5], he refers to "the anatomical substrata of consciousness" (p. 225), and says that:

> In order that impressions made on the individual organs of sense shall excite the subjective modification called a sensation, it is necessary that they reach and induce certain molecular changes in the cells of their respective cortical centers (p. 257).

Mind, or "mental operations in the last analysis must be merely the subjective side of sensory and motor substrata" (p. 256) and the mechanism of connection between the mind and the brain Ferrier calls a "molecular thrill" (p. 258). In a later work (The Localisation of Cerebral Disease, 1878b), he similarly states that "the physiology and psychology are but different aspects of the same anatomical substrata, is the conclusion to which all modern research tends" (p. 5). But in this work, it is not a "molecular thrill," but rather gross pathological processes which are said to be the source for concomitance. This is also true of his lesion experiments, yet the switch in levels of analysis from molecules to lesions is never discussed.

Similarly, there is disagreement and ambivalence in localizationist writings about the composition of this anatomical substratum. Ferrier (and others at times) said that:

> the various regions of the cortex are linked together by systems of "associating fibres," which furnish an anatomical substratum for the associated action of different regions with each other (1876:11).

(See also Dodds, 1877-78 for a review of other similar opinions.) In some contrast, Jackson saw the substratum as electrical in nature:

> We have, as anatomists and physiologists, to study not ideas, but the material substrata of ideas (anatomy) and the modes and conditions of energising of these substrata (physiology). Where most would say that the speechless patient has lost the memory of words, I would say that he has lost the anatomical substrata of words.
>
> The anatomical substratum of a word is a nervous process for a highly special movement of the articulatory series. That we may have an "idea" of the word, it suffices that the nervous process for it energises; it is not necessary that it energises so strongly that currents reach the articulatory muscles. How it is that from any degree of energising any kind of arrangement of any sort of matter we have "ideas" of any kind is not a point we are here concerned with. Ours is not a psychological inquiry. It is a physiological investigation, and our methods

> must be physiological. We have no direct concern with "ideas," but with more or less complex processes for impressions and movements (1932a: 132-33).

Here again we have a disclaimer of involvement with "mental" processes, coupled with an untestable mechanism ("energizing") which joins two realms.

By contrast to concomitance theories, building block theories saw "mind" or "consciousness" as emerging from combinations of physical units. Maudsley (1890), for example, articulates this view as a philosopher, and sees mind as what is left over after the building blocks of reflexes and learned reflexes combine to form nervous action.

Building block theories, because they are particulate and modular, had the advantages of similar engineering models. Units could be distributed over the nervous system in different amounts or densities, thus providing a model for localization of function. It also encompassed many of the anomalies of noncorrelation (discussed in Chapter 4) faced by experimenters and postmortem analyzers.

Consciousness, or mind, was seen as emerging from the building blocks as alternately "sense datum plus memory" (Ferrier, 1876:44), or, as Gowers put it (1885:117), as a residue or discharge of memory:

> Memory, like other mental actions, has its psychical side. Every functional state of the nerve elements leaves behind it a change in their nutrition, a residual state, in consequence of which the same functional action occurs more readily than before; and this residual disposition is increased by repetition.

This quote from Dodds (1877-78:357) illustrates many of the problems faced by localizationists in using a building block model to solve the problems of parallelism:

> What explanation then is to be given of the evident difference between Ferrier's motor and non-motor regions? We have seen that movements called forth by the stimulation of the non-motor regions are of the nature of reflex actions, being produced by certain sensations, as of sight, hearing, etc., and we would look upon these as sensori-motor centres for the highest and most complicated reflex actions. Is there, then, likewise no peripheral stimulus which will have for its effect the calling into action of the "motor" region? We know of none. It may possibly be the centre for muscle-sense or muscle-consciousness. But this seems incapable of explaining the phenomenon. The only probable explanation appears to us to be that the cells of this motor region of Ferrier form, as it were, the motor alphabet of the will. In a way as yet not understood, the will is able to decide upon certain movements, and then, compositor-like, to pick out the area, by whose stimulation the desired result is accomplished.

The compositor image used by Dodds reflects a concern with the idea of coordination. What coordinated the parts? A key passage from Ferrier (1876) illustrates the major change in emphasis on this question brought about by the localizationists:

> Hence we are not entitled to say that mind, as a unity, has a local inhabitation in any one part of the encephalon, but rather that mental manifestations in their entirety depend on the conjoint action of several parts, the functions of which are capable, within certain limits, of being individually differentiated from each other (p. 42).

The problems of the origin and consequence of conjoint action were never fully addressed. Rather, the ideas of concomitance and building block models became themselves increasingly robust as they were useful in addressing clinical issues and in doing experimental work. The problems of storage, coordination, and the mechanism of connection between the two realms were addressed by representation theories.

Representation theories are the most philosophically sophisticated of the plausible bridges constructed by localizationists. They are also the most complete in the number and variety of phenomena addressed. They bridge the gap between the mind and body by postulating that activity from one realm is translated via some sort of algorithm, to the other. Indications are made across realms which represent either movements, ideas, words, or physiological processes. As Jackson said, "we understand a speaker because he arouses our words" (1932d:206).

Representation theories arose in part from the connection between treating aphasia and localization work. One of the core philosophical problems addressed by representation theories is that of "consciousness vs. unconsciousness" or automatic vs. reflexive, conscious activity. Early on, clinicians were confronted with patients who apparently could think but not speak. They could intelligently (by gesture) answer questions, showing that they heard and understood words, but were mute. Jackson and other writers, drew on this empirical base to try to distinguish word understanding and word articulation capacities. But this in turn raised the question of whether there was in fact "inner" and "outer" speech. Were the patients talking to themselves inside their heads? Was this a different "speech" than that which is articulated aloud? These questions would continue to fascinate neurologists for some years (Head, 1926; Riese, 1977).

Jackson used the concept of representation in two ways. First, he postulated the existence of inner and outer domains of experience, most notably inner speech and outer (articulated) speech, but also inner muscle sensations vs. outward movements:

> A word is a psychical thing, but of course there is a physical process correlative with it. I submit that this physical process is a discharge of cerebral nervous arrangements representing articulatory muscles in a particular movement, or, if there be several syllables, in a series of particular movements (1932d:205).
>
> In speaking aloud there is a strong discharge of the same nervous arrangements with a correlative vivid psychical state--in this case the discharge is so strong that lower motor centres are engaged, and finally there are movements of the articulatory muscles in a particular sequence (1932d:207).

Second, he mapped evolutionary changes onto the individual, by saying that processes of dissolution were ordinally represented from the larger sphere to the individual. These representations could be faint or vivid, mere echoes or actual imprints (Walshe, 1961).

Representation as a concept solved some of the problems or blanks left by the doctrine of concomitance. Specifically, it helped serve to transform physiology into static functions, and avoid some of the previous philosophical objections to the idea of substratum. Jackson's definition of anatomy here was unique:

> To give an account of the anatomy of any centre is to draw what <u>parts of the body it represents</u>, and the ways in which it represents them (1932f:96).

Engelhardt (1972) says that Jackson transformed psychological notions of cerebral localization (which he said were outside the province of medicine) into physiological and sensory-motor concepts via the use of the concept of substratum. He then <u>redefined</u> anatomy in terms of representation, and transformed the physiological concepts into anatomical ones. This was in turn tied back to parallelist, localized models of the brain. [6] In Jackson's words:

> It is an anatomical division. By anatomical I mean that it is after the different degree of complexity, cf., or representation of parts of the body by different centres. The nervous system is a sensori-motor mechanism, a coordinating system from top to bottom. The evolutionist can take a brutally materialistic view of disease of any part of the nervous system, for the reason that he does <u>not</u> take a materialistic view of mind--does not confound nervous states with psychical states. The highest centres are only exceedingly complex, and special sensori-motor nervous arrangements representing, or coordinating..the whole organism. The doctrine of evolution has nothing whatever to do with the nature of the relation of psychical to the physical states of these centres; it simply affirms concomitance of psychical states with states of these centres (1932b:40-41).

Again, Jackson is here addressing the important audience of evolutionary theorists. Evolutionary biology had long concerned itself with its own species of mind-body problem: How did "consciousness," or that which makes humans uniquely human, evolve? Huxley (1904) and others had postulated a progression from automatic to reflexive or voluntary activities based on the evolution of the brain. Human beings retained automaton-like movements in the lower part of the nervous system, the "animal" part. Higher functions were contained in, and localized within, the "higher" cortical centres. Jackson, drawing on Spencer's ideas of increasing heterogeneity with evolution, saw an increasingly complex, localized human brain as the inevitable outcome of evolutionary processes (1873c). His 1884 Croonian lectures to the Royal College of Physicians (1932e:45-75) concluded that the highest centres, which make up the "Physical basis of consciousness" are the most complex and the most voluntary, (although the least "organized" in the sense that they are naturally ordered). They draw, via representation, on lower centers. An 1873 article of Jackson's had stated that:

> Lesions...<u>discharge</u> <u>through</u> the corpus striatum. I suppose that these convolutions represent over again, but in new and more complex combinations, the very same movements which represented in the corpus striatum. They are, I believe, the corpus striatum "raised to a higher power" (1873b:200; see also 1932e:218-219).

In sum, then, representation theories are a kind of Cartesian sign language, where the body and mind mutely gesture across the epistemological gap of parallelism.

All of the bridges constructed by localizationists involved emphasis, although not exclusive, on the practical primacy of the physical sphere.

3.3 Jettisoning Unsolvable "Mind" Problems

It was in part the emphasis on the practical primacy of the physical sphere that made possible the third strategy used by localizationists to explain the contradictions of parallelism: jettisoning unsolvable "mind" related problems into other lines of work. This division of labor ensured that neurologists and surgeons could concentrate on the physically-based problems which has been their basis for success.

The creation of "garbage categories" is a familiar process to medical sociologists. When faced with phenomena that do not fit diagnostic or nosological classification schemes, doctors often assign an omnibus diagnosis to patients. One function of such a diagnosis is to shunt unmanageable, incurable or undiagnosable patients into other lines of care where they will not interfere with the ongoing work.

Localizationists created such categories for problems which did not have a localized referent and or the possibility of a physical treatment. These patients were diagnosed as hysteric or neurasthenic (National Hospital Records). Early in the period studied here patients so diagnosed were treated by the hospital at Queen Square, with much the same treatments as patients with brain tumors, stroke, or other diseases with known physical referents.

However, by the turn of the century, patients so diagnosed began to be referred to psychiatrists and to be jettisoned from the jurisdiction of localizationist neurologists and surgeons. This both reinforced the physical orientation of these workers, and mirrored the philosophy of parallelism on an organizational level. In an active sense, it helped solve the contradictions posed by parallelism by lodging "mind" and "body" in the ordinary division of labor and referral system in medicine.

4. Summary: Coincident Boundaries and Perspectives

In the first chapter I discussed five aspects of a successful perspective--inertia, momentum, cumulative reification, incompleteness at any point, and progressive inseparability of parts. I will briefly discuss here how the adoption of parallelism, and the strategies used to counter its complications, affected these dimensions.

1. Inertia. The coincidence of multiple boundaries at the skull, which formed a practical boundary for the several lines of work triangulated on the localization prob-

lem, helped make the localizationist perspective inert. Changing such a view would have required redesigning all of the lines of work involved, or separating one of them. The problem-solving value of parallelism and of the strategies used to make it work was enormous, as well.

2. Momentum. Support from evolutionary biology, the promise of genuinely new results for the old arena of debate about the mind and body, and the promise of a working algorithm to map the coincidence of mind and brain phenomena all contributed to the momentum of the perspective. While philosophers argued with the parallelism adopted by localizationists, they also picked up and discussed the results of localizationist experiments, thus contributing to the rapid spread of such results.

3. Incompleteness. The segmentation of mind and body into different realms of work, the use of unprovable categories, and the jettisoning of contradictions into the convoluted work of Jackson contributed to the incompleteness of the perspective at any one point.

4. Reification. The use of unprovable categories, and the transformation of physiological processes into anatomical distinctions made localization more reified. In a more subtle way, as parallelism was adopted, the processual and discovered aspects of behavior became transformed into correlates of either mind or body, and thus reified.

5. Inseparability. As phenomena which localizationists saw as material and as immaterial were presumed to have coincident boundaries, parts of the perspective became seen as inseparable. Mind as contained in the individual nervous system, and the idea of an anatomical substrate in particular came to inform the localizationist perspective.

In sum, the adoption of parallelism and the strategies outlined in this chapter had a significant impact on the success of the localizationist perspective.

FOOTNOTES

1. But an excellent summary of these positions may be found in James (1890:128-82). A strikingly similar review of these positions may also be found in Eccles (1982), who gives essentially a point-for-point recap of the positions laid out by James.

2. There was support from other sources for the idea that the individual mind was contained in the individual nervous system. Developments in medicine helped support this view. After the 1860s, germ theories of disease prevailed over epidemiological models. Disease began more and more to be thought of as located exclusively inside the individual.

The rise of surgery supported this development as antisepsis became more successful. A stronger organism-environment distinction than had been made previously was strongly supported by the end of the 19th century. Boring (1950) believes that the localizationists were heavily influenced by association psychology, and that this was an important source of the adoption of parallelism. He says that:

> the establishment of the principle of the conservation of energy in the 1840s favored the principle of psychophysical parallelism...Very able thinkers came to believe in "chains" of causes and effects, series in which any event is the effect of the preceding one and the cause of the succeeding one and in which all events are equal in energy...Bain...in the 1870s...did not see how a neural event could cause a psychic one without transfering energy to it, nor a psychic event, with no physical energy to give us, cause a neural one. Being a dualist and believing in the conservation of physical energy within the closed system of the physical world, he could not believe that physical energy could be lost into the psychical world or acquired from it. That was the argument which gave victory to parallelism in the late nineteenth century (pp. 666-67).

Young (1970) also believes that the localizationists were directly influenced by the associationists. I find the evidence less striking than he does. Ferrier was Bain's student, but this provides little explanation for linking Ferrier's theories with his actual work. Unlike Jackson's use of Spencer, which is clearly cited throughout his work, Ferrier's use of Bain and other associationists is much less clear.

Bentley (1975c) provides a brilliant analysis of the philosophical implications and limits of adopting the skin (and similarly, by extension, the skull) as a limit for philosophical analysis. He states that this boundary has provided an enduring basis for dualism and physiological reductionism.

3. Later, when Laycock was at Edinburgh, David Ferrier became his student. Critchley (1960a) says that Laycock influenced Ferrier to go into neurology.

4. William James attacked this view harshly, calling it sloppy thinking (1890:134-36).

5. Which was dedicated to Jackson.

6. A similar tacit interlevel switch, with accompanying reductionism, is discussed by Wimsatt, 1976. While he does not explicitly discuss it in detail, Wimsatt also states that the source for arriving at these levels of analysis, or for confounding them, is a pragmatic need of working scientists (see especially pp. 231-32).

CHAPTER SIX:

CONCLUSION

1. <u>Introduction</u>

This thesis has described the emergence and institutionalization of the localizationist perspective in neurophysiology. The elements of the perspective were:

1. <u>Anatomy carries mental functions</u> in the same way that electrical power lines carry electrical current;

2. The mind is contained in the <u>individual</u> nervous system, especially in the cerebral hemispheres;

3. The mind is <u>divisible</u>, localized in a <u>hierarchically ordered</u> nervous system;

4. The mind/brain relationship can be investigated through a <u>logic of deletion</u>, that is, by removing or deleting parts of the anatomy that perform the function, and then observing changes in behavior.

As I have shown, each component of this perspective was formed from and by a daily and institutional work context. In this concluding chapter, I will discuss the <u>persistence</u> of the localizationist perspective in the face of changing work contexts, with a brief overview of developments in neurophysiology since the turn of the century.

2. <u>Anomaly Management</u>

Localization theory in pure form was based upon discovering a one-to-one correlation between parts and functions. This research goal was never reached. Instead, localizationists were persistently confronted with an anomaly: lack of correlation or varying correlation between part and function. Sometimes this anomaly appeared as physical damage with no subsequent dysfunction; sometimes as dysfunction with no evident physical problem. It also arose with recovery of function. An area was injured, and a patient or subject became dysfunctional for a time, but then regained function in spite of lacking the supposedly necessary "area" (see e.g., Horsley, 1884).

The anomaly of inconstant correlation was managed in different ways at different times. When the perspective was first forming, localizationists tried to de-emphasize its importance, and often ignored it. Later, as the perspective grew stronger, the anomaly was examined in more detail and given more significance. By 1906, it was given the status of a discovery (Sherrington, 1906). This reflected the strength of the perspective by this time. The emergence of a bona fide discovery that function and area did not correlate was absorbed as a theoretical adjustment to the perspective--not contradictory evidence for its validity.

Sherrington's The Integrative Action of the Nervous System," was dedicated to "David Ferrier, In Token Recognition of his Many Services to the Experimental Physiology of the Central Nervous System" (Sherrington, 1906). The book described experiments done within the localizationist perspective (Greenblatt, 1972), but which took as a major starting point the anomaly of low correlation between area and function. Sherrington described the brain as a labile organ without discrete fixed areas in the strict sense. He emphasized its integrative aspect--the ways in which he saw action as arising from combinations of reflexes and adaptive functions--not, like earlier localizationists, from its modular components (Swazey,1969).

Sherrington's work marked the end of one era of research and the beginning of another. His work (for which he won the Nobel Prize) was widely hailed by localizationist physiologists. It is a testimony to the obduracy of the perspective that this was so. As Walshe (1947) notes:

> If the physiologist, absorbed in these studies, has concerned himself at all with the problem of the functional organization of the excitable motor cortex, it is because his punctuate electrical stimuli soon began to reveal to him a perplexing instability in the reactions of the cortex. The factors underlying this were later to be analysed by Graham Brown and Sherrington, but this analysis cannot be said to have had any deep influence upon the general conclusions formed by the following generation of experimental physiologists as to the plan underlying the representation of movements in the cortex. This plan is still generally held to be that the motor cortex is a close-set mosaic of points in each of which is represented, or localised, a physiological unit of movement...[Re Sherrington and Leyton, his collaborator]...It is a striking indication of the influence of the original punctuate localisation theory that

> these significant findings should have been interpreted in terms of it, for surely they were the heaviest blow this theory had so far received (pp. 6-8).

Walshe goes on to say that successive experimental studies of the cortex could not account for the anomalies which Sherrington and Leyton claimed as discoveries. He gives three reasons for this:

1. Successive generations have assumed the validity of localization theory and interpreted findings in that light;

2. "They have departed from the conception of a representation of processes, that is, movement, in the cortex in favour of one of a representation of structures, that is, muscles";

3. They have ignored artifacts, and all that did not harmonize with the theory.

As Peacock (1982) cogently argues, while Sherrington's work pointed to a diffusionist model for higher cognitive functions, neurophysiologists' ties to localizationist ways of working prevented them from adopting a systemic point of view. Rather, they renegotiated the significance of the failure of the central proving point of localizationism to _fit_ the localizationist perspective. [1]

The renegotiation of the anomaly of noncorrelation did not affect localizationism or subsequent perspectives for several reasons. First, this renegotiation occurred after physiology had become distinct from medicine. In addition, there was a growing technical expertise in physiology, including an ability to control many of the sequelae of surgery on experimental animals. The position of localizationists at this time was a virtual theoretical hegemony. Sherrington's model of lability and diffuseness coexisted with the more fixed localizationist models, was used to explain the artifacts they had generated, and did not seriously challenge them.

This state of affairs was strengthened by conditions arising in medicine and physiology with the onset of World War I. The war situation drastically changed the status and resources of neurological surgery. Because of newly-invented weapons and the extraordinary number of soldiers involved, this was the first time in history that medical doctors and surgeons worked together with virtually un limited numbers of cases involving brain injuries. Head wounds, battle shock, and mental disorders resulting from trauma occurred on a hitherto unknown scale, and with new types of injuries (Horrax, 1952; Sachs, 1952).

Sherrington's subtle physiological model did not fill the desperate need for simple, quick, and effective diagnostic procedures which could correlate an injury with a loss of function, and thus locate a site for surgery or bullet removal (Cushing, 1935; United States Surgeon-General's Office, 1919; McGill University, 1936). A simpler location-function model was revived, and Sherrington's "discovery" again became an acceptable, insignificant artifact. As such, it did not interfere with subsequent clinical and research programs based on one-to-one models of the relationship between area and function.

By the end of the war, an enormous volume of data had been collected on head wounds, shock and trauma. The data was slowly sorted through. Professional associations formed during the war obtained funding and increased legitimacy (Cushing, 1920). Much of the data so obtained was interpreted by another group in the brain research arena, Freudian psychologists and psychiatrists. An important alliance was formed at this time between neurosurgeons and psychiatrists, reflected in the emergence, at this time, of the term "neuropsychiatry" (Star, 1982b). Partly because of the large number of mentally injured soldiers, there was an enormous demand for psychiatrists to treat veterans. Neuropsychiatry as a full-fledged field quickly emerged at this time.

The alliance was an important one, similar to the earlier meshing of resources by neurology and surgery. Psychiatrists were able to provide explanations for supposedly non-physical phenomena, and neurosurgeons provided physical techniques for organic problems. Together, the two specialties could explain almost all behavioral and perceptual problems (Merritt, 1975).

By the 1930s, specialized neurosurgical and neuropsychiatric institutions had developed, such as the Montreal Neurological Institute (McGill University, 1934). Training in the specialty was instituted in most medical schools. In all of these research programs the localizationist perspective continued to prevail.

It reached its apex with the rise of psychosurgery and the work of Penfield and Roberts (Peacock, 1982). They used a localizationist approach to specify locations for memories and parts of the personality. A famous photograph of an operation by Penfield shows an exposed brain covered with tiny numbered slips of paper, representing the different functions thought to be located in each area--Ferrier's enterprise transferred directly to the living brain and incorporated into clinical practice (Penfield and Roberts, 1959). The psychosurgery programs of the 1940s and 50s sought to remove the sites of "aggression" and anti-social behavior (Vaughan, 1975).

In all of this work, we can see the persistent conception of the physical "substrate" carrying mental function; of approaches based on a logic of deletion; of the divisible, particulate, and individual mind. We can also see the effects of triangulation biases and the cumulative reification of discoveries in science. This also appears in physiological work which attempts to address the mind-body relationship, for instance, this quote from Fulton (1938):

> Having subdivided the nervous system in this manner, he should be in a better position to build it up again into an integrated whole than those who have approached the problem more philosophically in the normal and intact animal; but actually the physiologist has made less progress in this division than the psychologist: mental experience still eludes analysis in terms of reflex action. Yet there is little doubt in the mind of any neurophysiologist that mental phenomena represent some feature of the organization of nerve cells (p. 542).

After Sherrington's time, the institutional segmentation between physiology and medicine happened very quickly. Therefore the developments in World War I were much more internal to medical work than they would have been during a previous generation. While medical doctors and other personnel still saw their work as physiologically based, in fact it was based upon the reified physiology of another generation. But because triangulation persisted, neuropsychiatry and World War I had a large impact on subsequent research in physiology. The insights of Sherrington became assimilated into the numerous going concerns in medicine and other fields which were based on the localizationist perspective.

The amount of research on the functions of the brain has geometrically increased since Sherrington's time, in terms of resources, numbers of lines of work and problems involved, and in impact on other lines of scientific inquiry. As the field has grown, new lines of work have become involved which did not develop historically from institutions based on localizationism. The fields involved range from rehabilitation medicine and biochemistry to computer science and physics.

A fascinating situation thus exists in the present-day "neurosciences." Older disciplines with epistemological bases in localizationism are incorporating results from outsiders who do not share those assumptions. Often, this incorporation is in the form of technologies--for example, the development of the PET-scan device represents the intersection of nuclear physics and localizationist neurology. The nature and kinds of debate which have arisen in recent years reflect both the obduracy of localizationism and the capacity for scientists to confound, work with contradictions, and partial resolutions to fundamental problems.

3. The Persistence of the Localizationist Perspective in Modern Neuroscience

Research in modern neuroscience is currently conducted at levels of organization ranging from the submolecular to the social. The issue of scale is itself a major debating point in neuroscience. Some neuroscientists working at a single level of organization favor inter-level approaches, and see their work as forming part of a larger whole with multiple levels of organization. Others see their specialty as forming the basic or exclusive level of organization from which higher functions of the nervous system should be extrapolated. For example, Amari (1977) proposes a theory of concept formation which builds up the function of abstract thought from cell-to-cell connections. This theory bypasses psychological research on concept formation.

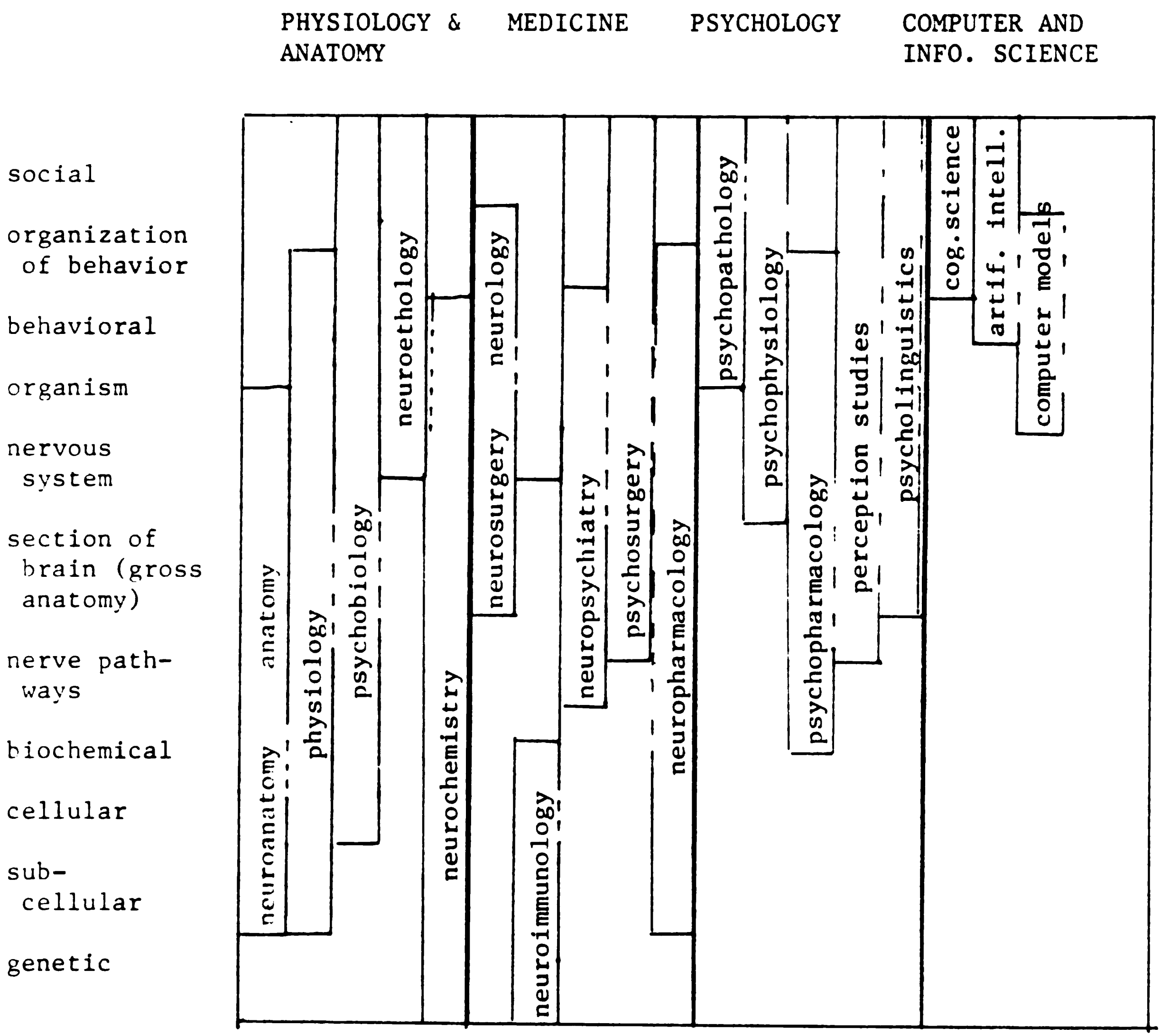
The debate and different approaches to the problem of scale illustrate the persistence of the dimensions of the localizationist perspective discussed above. The two broad classes of position on the subject of level of analysis--single level or multi-level--both tend still to look for mental functions within the individual, as a result of physical operations.

Figure 2 illustrates the several lines of inquiry in modern neuroscience and their corresponding level of analysis of inquiry. Note that many of the fields represented move from one, lower level of analysis to higher levels of function despite lack of analysis at intervening levels. Thus, movement from the cellular to the organization of behavior level would span six intervening levels of analysis; from cellular to nerve pathways, just one.

For <u>no</u> type of movement up or down levels of analysis have clear analytic procedures been devised (Wimsatt, 1976). Thus, while psychobiology may encompass material from the sub-cellular to the behavioral organization levels, this does not assure that movement between those

Figure 2: Level of Organization of Analysis by Line of Work

At what level of organization should one study the nervous system?



(Dotted lines indicate levels the field does not address, between levels that are studied by the field.)

levels have been analytically well worked out (Reichardt and Poggio, 1981). In part, this movement up and down levels reflects the legacy of the triangulation tradition in neurosciences. Combining results from different levels of analysis was well institutionalized in the early days of the discipline, as we have seen.

Rose (1980) discusses the institutional context and the simultaneous obduracy of localizationism in this work:

> For a lot of their working lives, neuroscientists do not show too much concern for such questions; embedded in a methodological sandwich of disciplines looking at brain and behavior, as biochemists, neurophysiologists, psychologists or whatever, we work on problems set by our own disciplines. Yet at the back of our thinking must lie the issue of relationships, of biochemistry to physiology to psychology. How do they fit together?...In reality, despite the proliferation of research and publication, reductionist neurobiology has conspicuously failed to meet its prospectus; that is, I would claim, no example of a mental or behavioural phenomena which can be traced by simple "causal" chains to a lower level one. Instead, the literature sinks back contentedly into a welter of "correlations," seen as a class of "soft" causes with the unspoken hope that eventually, after enough instances have been studied we will trust correlation between protein synthesis and memory formation, say, into causal statements which carry the same weight as those which relate, for instance, addition of cyanide to a respiring tissue slice to the cessation of oxygen uptake.
>
> In the meantime, there is an infinite number of possible correlations, and the corresponding possibility of a steady stream of grant money and output of research papers...(pp. I-III).

The debate about level of organization is impacted by the diversity/decentralization of neuroscience institutions and research programs, as separate research endeavors claim primacy for their level of organizational scale. For instance, biochemists claim that the chemical level of organization analysis will yield the basic units of information about the nervous system; psychologists insist that behavior should be the level analyzed. Rapid development in the field influences this debate as several lines of work, with different organizational levels (for instance, cell-level immunology and behavioral level psychophysiology) turn up new findings at the same time. These findings are used to legitimate claims for primary level of organizational analysis--leading to an ever-more complicated debate (see e.g., Quinn and Gould, 1979).

Rose's criticism of "reductionist neurobiology", above, is a common one in neurosciences. Neuroscientists realize that by the strict rules of inference and causality ideally exemplified by the scientific method, they are on shaky philosophical ground. There is a constant call for "overviews" and "syntheses" in the field (e.g., Edelman and Mountcastle, 1978; Popper and Eccles, 1977; Reichardt and Poggio, 1981; Schmitt and Worden, 1979).

Another major source of concern in modern neurosciences arises from a similar perceived mismatch between current procedures and philosophical foundations, exactly like that which obtained when localizationists adopted parallelism. This is the ongoing debate about the nature of the relationship between "mind" and "brain." While the relationship of the brain and mind is rarely itself the focus of a research program, it informs all research in neuroscience by influencing choice of technology, level of organizational analysis and terminology in which to express results. It is also the focus of numerous review papers, articles debating aspects of the philosophical foundations, and confounding in the field (see e.g., MacKay, 1978; Sperry, 1980).

The basic question involves two points:

1. Is there a division between brain (the material organ) and mind (perception, "thought" or behavior)?

2. If there is such a division, what is its nature? Does brain "produce" mind or does mind produce brain functions to suit its needs?

Positions on these fundamental questions range from those approaches which emphasize <u>brain</u> to the exclusion of <u>mind</u> ("radical materialist"), to those which reverse the emphasis ("disembodied idealist"). Participants in the brain-mind debate include philosophers, physicians, and neuroscientists, as well as a substantial number of outsiders, including journalists and theologians. Professional journals, meetings, and interest groups have formed around the debate.

The several positions taken on the brain-mind position and on the question of levels of organizational analysis all involve philosophical stances. As in Ferrier's day, neuroscientists are aware of philosophical problems and often agree with criticisms of their own work.

Sociologists of science have often observed that scientists give idealized views of their own work--deleting uncertainty, processes of production, and evaluating the work according to ideal canons of scientific method (see e.g., Dewey,1938; Mead, 1917; Latour and Woolgar, 1979; Collins, 1976; Collins and Cox, 1976). Yet the implications of this have never been fully explored by sociologists of science with regard to the dimensions of perspective outlined here.

I have made the argument here that the truth of a theory does not rest upon its match with logical formulations, but rather derives from historical, work-based and institutional processes within lines of work. This is true for scientists working in the field as well as for sociologists of science "standing outside." (It is also no less true for the sociology of science itself.) But scientists are normatively compelled to evaluate their work according to logical ideals.

Thus, as neuroscientists face logical inconsistencies in their work, there is a constant spate of reform articles and weak reform movements within the field to try to bring logical canons in line with practice. This is an attempt to be consistent and honest on the part of neuroscientists.

Yet, because the grounds for, and obduracy of the localizationist perspective are not understood in the field and have never been institutionally challenged, challenges to logical inconsistencies as well as attempts to synthesize the entire field fall flat.

The persistence of the localizationist perspective is reflected in the very terminology of the debates discussed here. Neuroscientists have discovered that declaring the brain and mind to be the same thing does not work; the institutional structure which have separated them are too entrenched. Pulling together multiple levels of analysis in a single project does not challenge the segmentation of fields nor the view of the mind as hierarchical and made of incrementally-assembled parts. Attempts to address the anomalies and problems of correlation and causality which do not address the institutional issues end up in the same structural position as "medical ethics" in medicine. The ethicists may be right, but they have no impact, because the ethical decisions in medicine, like other fields, are based on going concerns that cannot be challenged in a simply philosophical way.

Changing and challenging a perspective which has become successful is very difficult, then. Yet I do not mean to imply that it is impossible; to the contrary, the history of science is full of examples of such change. Yet we know as little about the mechanisms of this change as we do about the mechanisms of success and stability, and for the same reasons. Future studies of perspectives would need to ask the following questions:

1. How is work from different perspectives brought together? What does collaboration between perspectives look like? How does it contrast with triangulation of results within a perspective?

2. Where multiple perspectives coexist within the same institutions, what does competition between them look like? How do they go about convincing sponsors to fund them? How do they argue with each other? Who wins, and why?

3. What makes scientists abandon one perspective in favor of another? (E.g., we need studies of phenomena like Lashley's (1929) failure to find the engram and his subsequent "conversion" to diffusionism.) What happens when they do?

4. Is "travel" between perspectives permitted in science? Can a scientist participate in more than one perspective? With what institutional result?

5. What is the impact of reform movements on perspectives? What is the impact of new technologies on perspectives?

4. Conclusion

This thesis has presented scientific theories as going concerns within the context of perspectives. The implications of science as collective work (instead of ideas, tools, or individuals) have been elaborated. A framework with which to evaluate the development of successful perspectives--truth--is proposed here. An explicit part of my own perspective in developing this analysis is that science is a material concern, not a matter of ideas. Two quotes, representing the two historical sources of this perspective, will serve to underscore this point, and to illuminate the type of analysis proposed here. The first is from Albion Small and George Vincent's An Introduction to the Study of Society, one of the first books to come out of the Chicago school of sociology:

> The activities properly called social may be said to consist of acquiescence in the requirements of physical and psychical contract between human beings, and appropriation of the opportunities of such context between human beings.
>
> In calling attention specifically to man as a cooperating animal, the reference is to those social facts which arise when men begin to take conscious account of each other, in attack and defense, purchase and sale, mastery and obedience, emulation, rivalry, organization, authority, persuasion, assent and dissent, with all further relations involving volitional combinations of man and man (1894: 61).

The social processes of scientific inquiry are composed of the same processes. Description of the conditions of these activities is the material basis of understanding inquiry. As Dewey (1920) conceived of matter, this means a nondualistic description of institutions and work, thought and action:

> It thus turns out that the old, old dread and dislike of matter as something opposed to mind and threatening it, to be kept within the narrowest bounds of recognitions; something to be denied so far as possible lest it encroach upon ideal purposes and finally exclude them from the real world, is as absurd practically as it was impotent intellectually. Judged from the only scientific standpoint, what it does and how it functions, matter means conditions. To respect matter means to respect the conditions of achievement; conditions which hinder and obstruct and which have to be changed, conditions which help and further and which can be used to modify obstructions and attain ends (p. 72).

Thus, scientific perspectives as described here are material; they are the conditions of action, viewed with respect to the contingencies of work, especially problem-solving work.

FOOTNOTES

1. Peacock (1982) says that:

> Sherrington appears to have doubted whether or not there was any validity at all to a scientific approach to the study of the functions of the brain, but his extreme views were not totally shared by other 20th-century physiologists, whether "localizationist" or "holist" in inclination. "Busy commonsense" had been a very useful approach in both the 19th and 20th centuries. Knowledge of neurophysiology had been greatly advanced by the analytic approach to biological phenomena: The traditional line of demarcation between mind and body had been pushed further and further back, and there was no longer

much dispute that there were discrete areas of the brain representing "lower" motor and sensory functions. For the "higher" functions, there was no such agreement. Here the "localizationist" believed that all "higher" functions would eventually be localized, while the "holists" maintained that such functions were the product only of the integral brain. Thus, both parties agreed that a structure-function relationship existed; they were only disagreed about the interpretation of this relationship.

APPENDIX A: TRANSLATION FROM Mario Panizza, La Fisiologia del Sistema Nervoso e i Fatti Psichici (3rd edition). Rome: Manzoni, 1887, pp. 148-67.

Translated by Mirto Stone.

Note: Panizza was a diffusionist critic of localizationism (see pp.180-81). His work is not available in English, but was reviewed in the pages of Brain in 1881. I include the following translation for the convenience of readers who are interested in the details of the debate.

A problem remains still for the physiologist which, however, is relevant mostly to pathology. The effects of a stimulus tend to become generalized in the organism; and this conforms both to the teachings of anatomy on the connections of all the parts of this system, and to the nature of excitability common to all those parts. But this does not always happen; rather, such effects are limited more often to single areas. If we then consider a certain number of cases, in which a certain point on the nervous system has been injured, above all in the centers we can observe a relationship between the injuries and the peripheral parts that are affected by them, which by reason its constancy has allowed the birth of an illusion of an absolute constancy. Thus for example, an injury in a cerebral hemisphere limits its effects to only one side of the body, which can be the same or opposite side in which the injury occurred. However, the statistics show a greater number of cases of paralysis, which took place following an injury to one hemisphere, both to the other side and to the same side as the injury. [1] Thus, a direct connection by means of independent nerve fibers can no longer be argued, as well as between the injured part and the area showing the paralysis. We can no longer explain the crossing effect by means of a decussation; nor can we explain the numerous exceptions by means of anomalous anatomical structures in which the effect is missing. Therefore we must search elsewhere for the reasons for these facts.

II

What we deduce from the facts is contrary to the theory of localization.

Given these considerations, it is clear that the fact of partial sensory and motor paralysis following an injury to the nerve centers can never be brought up as proof that either sensitivity or the instigating principle of movement is located in the injured spot. In order to be used as proof, it would be necessary for no other area to be injured and give rise to the same effects, and for the same effects always to follow the injury of the same area with absolute constancy. But such constancy is universally contradicted by the facts. Not only can the irritation of _any_ point on the nervous system give rise to the same morbid phenomena identical to those obtained by irritating areas that are far apart and totally different, but the most varied phenomena can be obtained by irritating the same point: "Very numerous experiences," writes Brown-Sequard, "which confirm the teachings of human pathology have proved to me that producing the same injury in the same area of animals of the same species can give rise, as in man, to an immense variety of effects." [2]

III

Suggested hypotheses to reconcile the facts with the theory of localization.

Thus the physiologists who would like to assign a place in the cerebral centers to sensitivity and to will cannot derive any experimental proof from these facts. But in truth it cannot be said that any of them have ever made this claim; their efforts have been turned only toward the search for hypotheses that would reconcile the mass of facts, which they themselves recognize as contrary to the localization theory, with the theory itself.

I

Indirect paralyses.

All cases in which an irritation of peripheral areas has given rise to paralytic phenomena in distant organs had been explained by saying that it was a case of indirect paralysis, that is to say, it was supposed that the condition of irritation was not only diffused through the nerves or through peripheral ganglial centers, but that it first ran to the centers where the sensitivity and motility of the paralyzed organ is localized, and that its influence was shown on them. Thus for example, in the case i which a wound to the sciatic nerve produces a paralysis of the upper limb on the same side, the cause of paralysis would have gone directly to the cerebral centers to interrupt the action of those nerves which determine the movement of that limb. Even if we suppose independent centripetal routes of conduction, the connection of various cerebral centers and the different degree of resistance that they can oppose to the diffusion of the stimulus, would explain up to a certain point the variety of these effects. We must, however, point out that we are dealing with a hypothesis adjusted to support a theory, but one that does not prove at all the existence of the centers.

II

Flourens' hypothesis.

It had been more difficult to reconcile with the theory, the facts that demonstrate that the extended injuries of the cerebral hemispheres present neither sensitivity nor movement. And it is on these facts that the skill of the physiologists has for the most part applied itself.

Flourens had said that the cerebral hemispheres were the only sensing parts of the organism, meaning by this, that they were the parts to which impressions had to arrive in order for the animal to be aware of them. The animal deprived of its hemispheres was thus deprived of all sensitivity and was compared by Flourens to a man deeply asleep. [3] Cuvier however pointed out that a man immersed in sleep, who moves, who assumes a more comfortable position, can not be said not to have sensations, but rather should be described as "one who has neither the distinct impression nor the memory of it." [4] Flourens accepted the distinction and later wrote, "The animal that has lost its cerebral lobes has not lost its sensitivity; it still has it completely; it has lost only the perception of sensation; it has lost only its intelligence." [5]

It is therefore psychology that comes to the aid of physiology. Psychology made intelligence out to be a faculty that elaborates the material furnished by sensations, and one that is totally distinct and independent from the sense of touch; the physiologist assigned separate areas in the brain to the two faculties.

Cuvier said, "The cerebral lobes are the receptacle in which all sensations take distinct shape and leave lasting imprints; they are the seat of memory, the faculty with which they provide the animal with material for judgment." [6] Longet says that "the physiologist must be allowed to distinguish simple perception, which is in a certain way crude, of impressions, from the attention paid to them, and from the tendency to form ideas in relation with them." [7] Vulpian says similarly, "Such perception, as it must be understood in physiology (that is, Longet's "simple or crude perception," sensation) certainly takes place outside of the brain; but it is in the brain that the work of isolating different sensations from each other, of appreciating them at their proper value, of analysing them, of transforming them into ideas, takes place." [8] Lussana and Lemoigne say, "With sensation, the animal perceives the physical qualities of objects (contact, resistance, odors, flavors, light, sounds); with perception, the animal transforms these different sensations into ideas relative to intelligence and instinctual impulses (food, drink, danger, companionship, enemy, space, place, time, etc.)." [9]

We shall not now criticize the concept of faculty understood thus, which already presents itself as an empty abstraction, but we shall consider the hypothesis only from the perspective of physiology.

This distinction and this differing localization of the two faculties having been established, the physiologist used to explain how sensitivity and movements are not destroyed after the demolition of the cerebral hemispheres: only the intelligence, which has its seat in the hemispheres is destroyed, and thus only the movements which respond to the ideas are destroyed; but the movements which respond to sensations still remain, since sensitivity is outside the hemispheres.

Carpenter explains it thus: "The movements that are observed in animals with an intact nervous system are ideo-motor and senso-motor reactions; in animals that are deprived of their brains, they are simply senso-motor reactions."

It must be pointed out that there is no fact that serves to effectively differentiate these two types of movement: after removal of the brain, the animals perform movements identical to those they performed when the nervous system was whole. It is only the interpretation of this fact that varies according to the ideas professed by single authors. If a frog deprived of its cerebral hemispheres is thrown into a pond, it can be seen to begin swimming with totally normal movements, to reach shore, to leave the water, and to assume the position of rest. It could be supposed that the frog is still capable of voluntary movements: "A particular excitation," says Vulpian on the other hand, "of the entire surface of the body is produced upon contact with the water; this excitation initiates the swimming mechanism, and this mechanism ceases to act as soon as the cause of excitation is also removed when the animal has left the water." [10] But it is easy to see that the stimulus here has initiated not only the swimming mechanism, but also the mechanism of the animals' efforts to climb out on the bank to leave the water. Vulpian could not avoid making this observation: "I can see very well," he adds,"something singular in the series of movements by which the frog leaves the water and climbs up on the banks of the pond to resume its position of rest; but instead of seeing here a proof of the persistence of the animals' will, I see here a proof of the extent of the power of the excito-motor stimulations in animals" [11]. In other words, the physiologist sees in these facts, which always stay the same, what is convenient to him or what he chooses to see.

To support the hypothesis, the differences that can reflect the actions of two animals of the same species, in which one has undergone the removal of the brain and the other is left intact, have been greatly exaggerated. The state of somnolence or stupor or amnesia in which the former falls compared with the latter has been overstressed. It has also been noted that the movements of the former are less lively. Vulpian writes, "These movements lack that quality of random spontaneity which can be observed in intact animals which cause them to be considered voluntary movements."[12] But it can have occurred to nobody who has doubts about localization, as far as I know, to declare a perfect identity between an intact animal and an animal of the same species which has been deprived of its brain. The sole effects of such a grave trauma would suffice to place the two animals in conditions much different from each other. But such vague differences are not a valid argument for the localization exclusively in the brain of intelligence and will; while it is certain that we do not have experimental data that can demonstrate that an animal deprived of its brain is deprived of consciousness.

Goltz puts two frogs in a vessel full of water, one frog being decapitated and the other only blinded to impede the production of voluntary movements that would follow visual impressions. At the time of immersion the water is at 25 C. It is slowly warmed to 50C. Now Goltz says that he has observed that the first frog attempts to escape and shows signs of distress from the beginning and dies when the temperature reaches 42 degrees, and that the other remains calmer and gives no signs of distress or pain, and dies when the temperature reaches 50 degrees. The calmness of the decapitated frog was not, however, absolute; since Goltz himself admits that it made defensive movements analogous to those of the frog to whose skin acetic acid was applied. We have repeated numerous times this experiment and have reached totally opposite results, which, in order not to negate Goltz's statements, forces us to think that he made the experiment only one time, and that in this time, as an exception, the shock of decapitation had immobilized the animal, or had greatly handicapped its sensomotor reactions.

When two frogs, under the same conditions, have been placed in a vessel of water at 25 C., we have always observed that the blinded frog floated there calmly with its head out of the water until the heat went over 35 degrees; and at the same time the decapitated frog was agitated, swimming in every direction, and made repeated attempts to jump out of the water; in one instance, the frog jumped out of the vessel twice.

When the temperature had reached 42 degrees, the decapitated frog always died, as did the blinded frog a few minutes later, and both in the same manner, with general trembling and convulsive movements. [13] When one thinks that Ferrier finds in Goltz's experiment the proof "the most conclusive that a frog without a brain and capable of reflex acts is perfectly insensible to stimulations which in a normal state give rise to symptoms of pain," [14] we will thus verify the strength of a preconceived idea even in the face of the clearest evidence.

The same hypothesis proves insufficient in light of other facts.

The hypothesis does not explain the cases recorded from human pathology in which the complete or partial destruction of a hemisphere did not destroy or disturb any of these manifestations that are considered necessarily tied to intelligence. Thus, for example, Smoller observed in the Clinic of Halla the complete suppuration of a hemisphere in an individual who had taken a long walk and had carried several buckets of water to a fourth floor a few hours before dying.

Further, this hypothesis does not explain the results of those experiments that demonstrate that an animal becomes paralysed immediately after the destruction of one hemisphere, but resumes shortly afterwards the use of all its faculties. Many animals, one of whose cerebral hemispheres has been removed, do not differ, as were observed by all experimenters, from intact animals. In some, however, disturbances of locomotion were observed, and also, particularly in dogs, paralysis of the front legs; but these disturbances were temporary and, with the healing of the wound, the animals were observed to resume their former state. If, when the brain was entirely removed, one could attribute the significance of senso-motor reactions to actions performed, there is no sign here that the removal of one hemisphere disturbs or suppresses intelligence. Gall had already supposed that the same faculty resided in symmetrical points of both hemispheres; and that therefore the surviving hemisphere could substitute for the other destroyed hemisphere by itself. But we can remove from an animal corresponding parts of the two hemispheres, proceeding from front to back or from back to front, or from top to bottom, and after the operation, the animal will not differ in any way from an intact animal.

These experiments were performed first by Flourens, then by Longet, by Vulpian, and by many other experimenters, always with the same results. Vulpian removed the anterior part of the cerebral lobes from a frog, and observed that, during the scarring, the frog hunted a fly placed in front of it, but could not reach it with its tongue in one shot; after some time had passed, the frog would capture it at the first try, and at the end the frog differed from an intact frog only in being less lively and in being less eager to escape.[15]

But there are also pathological facts in man that could be cited as evidence against Gall's hypothesis. Trousseau saw a man who had been wounded by a bullet entering one temple and exiting through the other, and who, for three months (that is, until he died) kept not only his intelligence intact, but preserved as well his peaceful and lively disposition. [16] Velpeau tells of an individual whose two cerebral hemispheres were destroyed in their anterior part to the extent of about 4 centimeters and had been replaced by a hard, fibrous, bilobal tumor, each lobe of which was as big as a hen's egg, which originated from the <u>dura mater</u>; the surrounding cerebral matter was furthermore red and soft. Before dying, this individual was in a totally normal condition and, in fact, was quite talkative. [17] Vulpian mentions as well a man, the anterior portions of whose cranium had been penetrated by a bullet, with the escape of a large quantity of cerebral matter, who for four months was shown to be in possession of his intelligence, speaking without any difficulty. [18] Other similar cases are mentioned by Longet. [19]

Flourens, faced with facts of this kind, thought that intellectual processes were possible even with a very limited portion of the hemispheres, and that intelligence would be abolished only with their complete and total destruction. This faculty, according to Flourens, was made possible, on the one hand, by means of the hemispheres, but on the other hand was essentially single, so that with one portion of the hemispheres destroyed, this faculty would retreat into the surviving portion, and if destruction were continued, would retreat further into the remaining cerebral matter until no trace of it would be left. Only then would it disappear. [20]

Vulpian does not find another resource to reconcile the facts with localization of intelligence in the hemispheres, except for this hypothesis: "Cerebral functions," he concludes, "can still be carried out even with a very limited portion of the hemispheres." [21] Carville and Duret also accept this hypothesis which was called "substitution" or "functional replacement."

It is evident that physiological criticism would never have been able to reach a doctrine within these premises;

but if other physiologists did not accept this, it is because they had different psychological bases.

III

Hypothesis of Ferrier

According to a more recent psychology, it seemed that the faculties, as understood by the authors quoted above, were not, in the final analysis, anything but sensations. Certain physiologists accepted this view more gladly, not so much because sensation, as a subjective element deriving from the modification of a nerve cell, is something arising from experience, but because it turned out to be easier to understand the relationships of the nervous system with the psychological element, relationships which were inconceivable with the abstractions of older psychology.

In other words, the problem could not be solved by reducing the entire psychological sphere to pure sensation; i.e., what does a sensation consist of?; but it was thought that this perspective at least allowed the establishment of the nature of the psychological condition that invariably accompanies a predetermined exterior excitation. And this represented an improved point of view, since it concerned an area of research whose goal is to establish how elements are united; elements that, as Lotze declares, must be supposed to be self-contained and separate. [22]

In the following chapters we will see how this psychological doctrine as well can not be absolutely accepted by science. For now we must consider only to what conclusions this doctrine has brought the physiology of the nervous system.

Ferrier starts from these premises and considers sensations as the subjective side of the modification imprinted in the nerve cells by a present object. [23] But since this modification can not be reproduced without the object being present, he then allows a reexcitability of the nerve cell, because of which sensation already received can be presented again to consciousness: "Sensation," says Ferrier, "Becomes thus idealized," and he adds, "Perception, and thus the idea of an object, is the renewal of cellular modifications in each of the centers that have contributed to the process of knowing." [24] A sensation's possibility of being renewed is what generates, according to the same author, judgments of analogy, agreement, difference: "The foundation," he says, "of the consciousness of agreement is the reexciting, with a present impression, of the same molecular operations that coincide with a past impression; the judgment of difference consists in the passage from one physical modification into another." [25] Feelings of pain and of pleasure, which accompany sensations can be considered "the subjective impression of harmony or of physical discord between the organism and the influences which act upon it." [26] Therefore, also "the sources of conscious activity, or the incitements of will are present sensations or are revived by the idea, together with what accompanies them." [27]

It is now understood how the physiologist who starts from these premises can not call sensation and perception two distinct faculties, nor assign separate organs in the nervous system to each one of them; he will necessarily find only sensory organs. Since it is always the same sensation that becomes idea and impulse, these various operations would be able to have no function other than that of sensation: "Since ideal or revived sensation," says Ferrier, "occupies the same areas as those activated during actual sensations, the revived feelings or emotions are localized in the same area." [28] And he concludes, "A sensory, ideational, and emotional center is one and the same thing." [29]

Voluntary movements require the functional activity of certain centers which should determine the movements themselves, when artificially stimulated; these are the centers which, according to the theory, must be called motor centers. [30]

Hence we must search in the brain only for sensory and motor centers. Ferrier does not explain why the brain must be the seat of these centers; he says only, "We can proceed from this fact, as if from a definitely acquired plan."

[31] However he goes even further with localization, and places these centers exclusively in the cerebral cortex. He says, "So that the impressions made on individual sensory organs may excite the subjective modification called sensation, they must reach the cells of their respective cortical centers and cause specific molecular changes there." [32] The reason for this further localization is easier to guess. It is not due to the idea, already well known to science, and later popularized by Gall, that there exists a relationship between the surface area of an animal's hemispheres, and its intelligence. This idea was abandoned after Leuret, Baillarger, and Dareste demonstrated that this relationship does not exist at all, and that there are animals superior to other, much more intelligent animals (including man himself) as regards the number and size of the convolutions. The reason, rather, resides in the following: from the moment in which Hitzig and Fritsch discovered the excitability of the cerebral cortex, and the movements stimulated by electricity were interpreted as senso-motor reactions, the centers of all movements and all sensations of which the organism is capable were found in a limited area of that cortex.

Fritsch and Hitzig found in a very limited area of the parietal lobes of dogs the motor centers of the muscles of the face, neck, back, abdomen, tail, and limbs. [33]

Ferrier found in a much larger area, but one still limited to the lateral areas of the hemispheres, (postero-parietal lobe and ascending circumvolution) the motor centers of the muscles of the face, tongue, mouth, biceps (which, according to the author, would have its own center in the ascending frontal circumvolution) or of all other muscles of upper and lower limbs. [34] Similarly, in a restricted area were found the centers corresponding to all possible modalities of sensation. Ferrier found in the fold the center of vision; in an upper point of the temporo-sphenoidal circumvolution he found the hearing center; in the lower part of the same lobe (<u>subiculum cornis ammonis</u> and related parts) he found the smell and taste centers. He assigns to the occipital lobes the centers of visceral sensations, but through simple conjecture. It is known that sexual appetite is stimulated by tactile sensations, and, especially in certain animals, by particular odors. Ferrier thinks that "a region closely joined to the centers of tactile and olfactory sensations could be considered as a likely seat of the sensations that constitute sexual appetite." [35]

Thus given, we see that not only do all the centers reside in the hemispheres, but also that a large part of them remains without purpose, as for example, the frontal lobes, the function of which no one has as yet managed to find a function. The sensory and motor centers were thus localized exclusively in the cerebral cortex, because they were all found here, and nothing remained to be localized in the other parts of the brain. Hence no other function was assigned to these parts but the one which remained available, that is to say, the function of "connecting the centers of the cortex with the periphery." [36]

As is easily foreseen, this way of understanding localizations had to meet difficulties even greater than those found by the proceeding doctrines, in order to be reconciled with the facts.

Here the destruction of a hemisphere should of necessity produce a permanent hemiplegia and abolish the intelligence since it requires the participation of at least one hemisphere.

Several cases of permanent hemiplegia, following a hemispheral lesion in man and in monkeys, are given to support the theory. There are others, however, such as those of Smoler mentioned here, which refute it absolutely; these are not now taken into consideration. But even in the cases of hemiplegia because of lesion in one hemisphere it is known that often intelligence is not in the least disturbed. Ferrier is therefore forced to return to Gall's hypothesis, that is, that one hemisphere can substitute for the other in these functions: "If one hemisphere is removed or destroyed through disease," he writes, "movement and sensation are abolished on one side; but mental operations can still be performed by the remaining hemisphere." [37] This hypothesis is contradicted, as we know, by all those facts which demonstrate that intelligence still exists even when corresponding parts of the two hemispheres are removed.

[38]

The total removal of the brain, according to the authors who profess this doctrine, completely abolishes sensitivity and voluntary movement in the two sides of the body, together with intelligence. They must therefore admit that all movements by animals deprived of the brain, those movements that had obliged other physiologists to assume sensory centers outside the brain, are due to a mere mechanism. Hence they deny, against all experience, the spontaneity of these movements, and they explain their coordination to jumping, swimming, running, crying, defense, by affirming that the mechanism itself is preset by nature to respond to predetermined stimulations by jumping, swimming, crying; and, what is more interesting, by removing those influences from oneself once the brain has been removed, they are no longer perceived as sensation but, in a state of consciousness, had to be perceived by the animal as bothersome or painful. It is supposed that centers of even more complex automatic and special actions exist in the <u>medulla oblongata</u> and in the <u>mesencephalon</u>, and among these the reflected impression of emotion is included. Thus "the jumping from side to side performed to avoid an obstacle of a frog deprived of a brain would only be," according to Ferrier, "the result of two simultaneous impressions, one on the leg, the other on the retina," and "the cry that a rabbit makes in the same circumstances, when its paw is pinched, would be a purely reflected phenomenon, which does not depend on any real sensation of pain." [39]

But the question then arose: how could those actions, which in the wholeness of the nervous system require the intervention of consciousness in order to be performed, can still be performed when consciousness is lacking because of the removal of the hemispheres?

Ferrier answers with another hypothesis: he declares that this is possible only because <u>those actions are automatically organized in the ganglia of the base</u>. It has been seen how, according to Ferrier, the optic thalami and the striated bodies contain fibers that serve merely to connect the centers of the cortex with the periphery; however, they also contain gangliar matter. He then imagines that precisely this matter fulfills such an inexplicable function. An act which required, in order to be performed, the awareness of the sensory impressions, becomes, because of frequent repetition, fairly easy to follow impressions without discernment or attention. The gangliar matter of the thalami or of the striated bodies would serve, according to Ferrier, to constitute the connection between the impression and movement in all those actions that, being voluntary at first, then became unconscious because of habit. He writes, "In these cases, we may suppose that the impressions made on sensory organs go towards the optic thalami, and pass from there to the striated bodies rather than going through the larger and conscious circle, that is, through the motor and sensory centers of the hemispheres." [40] Given this, it is natural, according to the author, that these actions, being so automatically organized in the ganglia of the base, would still exist even following the removal of the hemispheres.

But no one, I think, would believe that an impression ends up establishing its own habitual passage through base ganglia, only because it passes through the cortex repeatedly. Not even this hypothesis illuminates, therefore, but rather, obscures the functioning of the mechanism of these actions.

The actions performed by animals without hemispheres are also held to be unconscious, not so much because of any of their characteristics, but only because they are performed without the intervention of cortical layers: "The reaction," writes Ferrier," between the optic thalami and the striated bodies being outside the realm of consciousness (localized in the cortical levels) is still outside the sphere of properly-called psychological activity"[41].

But we know that the cortical layers must be considered conscious by their very character, and we have shown above how we can distinguish them through experience form those that are due to mere excitability [42]. We can here add that there are as well other criteria to distinguish an automatic coordination from a conscious one. In the first case, although the movement depends on stimulation of the muscle fiber by means of the nerves, coordination is a fact, so to speak, which is exterior to the nervous system and depends on the arrangement in which the muscle fibers are found in the area that is set in motion. This coordination is found in the heart, the stomach, and other organs where the muscle fiber, contracting always in the same way through the stimulation received, still determines varied and coordinated movements in the organ. And no one could explain this automatic coordination by placing the seat of one faculty in the ganglia of these organs, a faculty, as Goltz would say, self-adapting.

But this mechanism is lacking in conscious coordination; the so-called muscles of animal life do not present a mechanism whose architecture is organized for a purpose, and thus, excitability's effects on them are resolved only in clonic or tonic contractions, and the intervention of will is always necessary in order to organize them into a special mechanism. We can not say how movements which have become habitual, and to which we pay no attention, have become unconscious; nor is it our purpose to discuss them here, but we will explain it in full in the practical (i.e. not theoretical) part of this study.

On the other hand, Ferrier's hypothesis of an automatic organization of the voluntary movements in the ganglia of the base, which is a problem in itself, does not solve other problems and must be supported by new and stranger suppositions. It is well known how this author bases his theory essentially on the fact, which he believes to be constant, that in the higher animals, such as man and monkeys, a circumscribed lesion of the cerebral cortex induces a permanent sensory and motor paralysis. [43]

We can now ask why the automatic organization of the movements in the ganglia of the base does not function in these animals, and why it remains a privilege of only a few animals of the lower orders. Here another hypothesis was indispensible. This happened, says Ferrier, because "Animals differ according to the degree of independence of the organization of their motor activities in the lower and mesencephalic centers" [44].

But it is not understood why this difference exists. Ferrier thinks that in the higher animals, since actions are more complex, they require the will from the beginning in order to be accomplished; and thus it is training which gradually comes to perfect their strength. In lower animals, on the other hand, since will is almost nonexistent, the control and the coordination of their actions are complete from the beginning and need no effort of training. He writes, "These animals are, to a large extent, conscious automatons"[45]. This is why "their cortical motor centers are of little importance and can be removed without causing disturbances in their ordinary activities"[46]. Thus Ferrier finds that the animals whose hemispheres are easier to remove are those which, as soon as they are born, perform all the movements of which they are capable, as, for example, birds that "hatch totally equipped, like Athena springing from the head of Zeus" [47]. In man, in whom training is long and difficult, "automatism in itself is barely separable from the centers of consciousness and will"[48]. But aside from the fact that these data are rather in relationship with the degree of development to which, at the moment of birth, the nervous system has arrived,a degree which varies in different orders, this author forgets that the pigeon is an animal which requires a rather long period of time to acquire all its movements; and it is in fact on the pigeon that the nonexistent effects of hemisphere removal have usually been demonstrated.

But we shall not dwell on this any longer. We believe that this, and many other difficulties, could be set aside in the same way; proceeding with this method, never before heard of in proper sciences, there will never be a fact impossible to accomodate under any theory.

IV

Brown-Sequard's hypothesis.

A hypothesis which, by considering the localization in general in the encephalon of sensitivity and will, functions better in reconciling localization with all facts presented to us by experience, is the one formulated by Brown-Sequard: "The nerve cells of the encephalon which constitute the centers controlling any specific function whatsoever, far from forming a group or a conglomeration in a distinct and well-circumscribed area, are instead spread about, so that each function has elements fort is purpose in very different areas of the encephalon. Hence, there exists a localization of functions, but in scattered cells, which do not form, as believed, distinct aggregates either in the circumvolutions, or elsewhere"[49]. We can only oppose to this hypothesis a criticism of the concept of the function that some have thought to be localized in predetermined nerve cells, and which can not be accepted by science, as we will have occasion to prove later; and the difficulties which the transmission of sensory impressions and of motor stimulations on will, which we have described in preceding pages, presents.

It is however certain that given as a general thesis the localization of sensitivity and movement in the encephalic organs, once would succeed better with this hypothesis than with the previous ones in reconciling it with results which are clearly contrary to experience. But we are satisfied with having demonstrated that no proofs of any kind exist that sensitivity and will have an exclusive seat in the nerve centers and that we are in no way obliged, regarding this issue, to maintain the concept of a transmission.

CONCLUSION

Summarizing the above:

1. Science maintains this concept not based on fact, but based on a merely speculative idea of the ancients;

2. Given that concept, we must admit within the nervous system an anatomic tendency which is contradicted by all the teachings of anatomy;

3. We must assume distinct conductive routes which go in the opposite direction from the sensory impressions and the motor impulses of will. We must support this hypothesis, which is contradicted by experience, with other hypotheses equally inconsistent;

4. There are no facts that can be used to support a proof of transmission; many of these facts can not be explained by this concept unless we use another endless

series of suppositions, which would still be contradicted by experience. We can conclude that the concept itself is false and must be absolutely rejected.

However, it would still be impossible to deduce, as a practical result of this study, that sensitivity is diffused and that will determines nerve action on the spot. It is first necessary that another age-old prejudice be removed, one that impedes the understanding of these functions. This prejudice would take away equally from the physiology of the nervous system and characteristic of experimental doctrine, even if the concept of double transmission is eliminated.

Notes

1. Brown-Sequard, Doctr. relativ. ecc. l. c., p. 19.
2. Flourens, l.c., p. 78.
3. Cuvier, Rapport sur le mem. de Flourens, Accad. des science. de l'Istit. 22 Luglio 1822, V. Flourens, l.c. p. 78.
4. Flourens, l.c. , p. 79, nota.
5. Cuvier, Rapport ecc. V. Flourens, l.c., p. 79.
6. Longet, l.c., Ed. 1850. V.II. Part. II, p. 38-39.
7. Vulpian, Lec. sur la physiol. gener. et compar. du syst. nerv. Paris, 1866, p. 672.
8. Lussana e Lemoigne, Fisiol. dei centri nervosi encefal. Padova, 1871, p. 35.
9. Vulpian, l.c., p. 681.
10. Vulpian, l.c., p. 682.
11. Vulpian, l.c., p. 679.
12. Queste esperienze furono sempre istituite presso la Clinica Media di Roma alla presensa di molti colleghi.
13. Ferrier, l.c., p. 32.
14. Smoler, Viert. Jahrschrift., Bd. 79.
15. Vulpian, l.c., p. 709.
16. Trousseau, v. Vulpian, l.c., p. 711.
17. Velpeau, Gaz. de Hopit., 1864.
18. Vulpian, l.c., p. 711.
20. Flourens, l.c., p. 244 e 264.
21. Vulpian, l.c. , p. 708.

22.Lotze, _Princip. gener. de psychol. physiol._ Trad. Penjot, Paris, 1876, p. 73.
23. Ferrier, l.c., p. 410.
24. Ferrier, l.c., p. 414-415.
25. Ferrier, l.c. p. 416.
26. Ferrier, l.c. p. 418.
27. Ferrier, l.c., p. 418.
28. Ferrier, l.c., p. 418.
29. Ferrier, l.c., p. 418.
30. Ferrier, l.c., p. 321.
31. Ferrier, l.c., p. 410.
32. Ferrier, l.c., p. 413.
33. Fritsch ed Hitzig, _Reichert's und Du-Bois Reymond's Archiv_. 1870, Het. III.
34. Ferrier, l.c., p. 322 e seg.
35. Ferrier, l.c., p. 318.
36. Ferrier, l.c., p. 400.
37. Ferrier, l.c., p. 442.
38. Munk trova il centro visivo, non piu come Ferrier....(Luciani e Tamburini, l.c., p. 77-78).
39. Ferrier, l.c., p. 66.
40. Ferrier, l.c., p. 406.
41. Ferrier, l.c., p. 408. Come abbiamo visto piu sopra...(l.c., p. 58)....(l.c., p. 58).
42. V.Cap. III, Art. I, p. 118-119.
43. L'esperienza non ha dimostrato tuttavia a Ferrier....l.c., p. 827); (l.c., p. 335).
44. Ferrier, l.c., p. 404.
45. Ferrier, l.c., p. 426.
46. Ferrier, l.c., p. 427.
47. Ferrier, l.c., p. 427.
48. Ferrier, l.c., p. 428.
49. Brown-Sequard, l.c., p. 7.

APPENDIX B

From: Transactions of the International Medical Congress, 7th Session, London, August 1881. Edited by W. MacCormac. Vol. I. pp. 218-37. London: Kolckmann, 1881.

Translated by Sigrid Novikoff, October, 1982.

The following appendix is a translation of the remarks of F. Goltz at the International Medical Congress, originally presented in German. See discussion in Chapters 2 and 4. The remarks in brackets are summaries.

DISCUSSION ON THE LOCALIZATION OF FUNCTIONS IN THE CORTEX CEREBRI.

If we set ourselves the task to investigate the functions of a nervous organ, there are two research methods at our disposal, the stimulation method and the extirpation method. The former seemed to let us down altogether with regard to the research regarding the functions of the cerebrum; for the older authors admitted unanimously that a stimulation of the exposed surface of the cerebrum did not have any results at all. Fritsch and Hitzig proved that this conception nevertheless rests upon an error. They found that, upon electrical stimulation of certain points of the parietal lobe of the dog, convulsive movements occurred in specific muscle groups of the opposite side of the body. Stimulation of a certain spot in the brain causes movements of the front legs, of another spot movement of the hind legs, of the fascial muscles, etc. The section of the cerebrum which contains the stimulation points, is called the sensitive (excitable zone or actually the motor zone. But there are also large areas of the brain surface, the stimulation of which remains without reaction, one of which is the occipital lobe (Hinterlappen).

The fact that the result of the stimulation is so extremely different according to the location of the stimulated point, suggested that the different sections of the cerebrum could be equally different in their functional meaning. But Fritsch and Hitzig also realized that the stimulation method alone would be insufficient to successfully pursue this idea any further. If we see a group of muscles, for example the muscles of the foot, twitch as a result of a stimulation, the nervous system stimulated can be of very different importance. Whether we stimulate the motor nerves, the spinal cord, the brain, or certain sense nerves, in all cases twitches of the foot muscles can be caused. Therefore, I cannot deduce with certainty from the twitching foot muscle caused by the stimulation of a section of the nervous system, what the functional importance of the stimulated organ is....

Therefore all scientists agree that the extirpation method has to complement the stimulation method in order to test it. Hopefully, many will agree with me when I maintain that the extirpation method is to be preferred because it induces a smaller number of false conclusions.

Fritsch and Hitzig immediately made some extirpation tests. They found that, upon removing that part of the cerebral cortex, the stimulation of which had caused twitches in the opposite side front leg, strange disturbances in the movement of the same limbs occurred which they interpreted as a result of the loss of the muscle-consciousness. Later Hitzig observed blindness of the opposite side eye after removal of a piece of the occipital lobe. Fritsch and Hitzig considered these experiences to be sufficient for the establishment of a hypothesis which constitutes a crass opposition to the teaching of Flourens which had been generally recognized to that date. Flourens taught that all sections of the cerebrum served the same functions. Fritsch and Hitzig believed it to be proven that the cerebral cortex is to be divided into definable individual areas or centres, each of which heads a special function. The investigations of Fritsch and Hitzig were continued, first by Ferrier and then by a number of other scientists...The hypotheses of all these scientists have in common that they divided the cerebral cortex into small defined fields, centres of spheres and assign special functions to each of them. The most striking differences exist, however, in the spatial delineation and arrangement of the fields and the distribution of the individual functions assigned to them, especially between Ferrier and

Munk.

Unconcerned by these contradictions, the medical public accepted these modern localization theories with great applause. A number of text-books were enthusiastic about them, while (on the other hand) one should have hesitated to accuse such a clear mind as Flourens of such serious mistakes in his observations. The new theories were obviously so trustingly accepted because the ground was prepared for them, as, on the basis of pathological observations, the need for a spatial division of the functions of the cerebrum was felt. The delicate rounding-off of the new theories, the apparent excellent agreement between the stimulation and extirpation methods, had a seductive effect. But a fruit may look very enticing and yet be worm-eaten in its core, and it is not difficult to identify (demonstrate) the worm-eaten point in all the localization doctrines mentioned so far. It is the ways and means by which the various authors resign themselves with respect to the so-called restitution-question.

Hitzig, Ferrier, and all their successors admit that the disturbances which occur after the extirpation of a piece of the cerebral cortex, can disappear amazingly fast. In order to explain this experience, numerous hypotheses have been established, all of which are absolutely unsatisfactory and to a large extent irreconcilable with clearly observable facts. If it has been pointed out that the destroyed cortex substance could regenerate, any anatomic proof for such a hypothesis is missing. On the contrary, I can demonstrate that, after the mutilation of the cerebrum, the rest of the brain shrinks. Therefore, there remains nothing else to conclude than that the restoration of the lost function is taken over by a part of the brain that was not destroyed. But any such supposition clearly contradicts the basis of the entire localization doctrine. For, if a center that performs a special function, should be able to take over the function of another, destroyed center with a different function, then the same section of the brain obviously executes different functions at the same time. Once the possibility of such an adjustment is admitted for the mutilated animal, it cannot be conceived by it should not exist also in the uninjured. If, for example, a dog who is blind as a result of the removal of the vision center governing both sides, learns to see again later, one should, however, not be permitted to maintain that the tail-center or the taste-center should suddenly also govern the vision-center, and if one sets up such an assumption anyhow, then one gives up the entire basic principle of the localization doctrine. In order to avoid this embarassment, Munk has chosen a new amazing alternative. He supposes that each center with a specific function is, in a certain way, surrounded by a non-active fallow-land of virginal cortex substance which only starts to function as a substitution when the center usually employed is destroyed by chance. According to this strange theory, we would thus possess an extraordinary surplus of available brain substance in order to be protected against the case in which we suffer a mutilation of the brain. As artificial as such a hypothesis may appear, it is not even sufficient, as will be shown later.

Considered closely, Hitzig and his successors are found to be in much closer agreement with the theories of Flourens than they would like to admit. Flourens asserts that the cerebrum can be mutilated without causing any impairment of its functions. Hitzig and his successors also teach that shortly after the mutilation of the cerebral cortex no functional disturbance can be observed anymore. The entire difference between the doctrine of Flourens and the one of the modern localizationist concerns only the amount of time required for the restitution. According to Flourens the restitution is supposed to be there immediately after the mutilation, while his opponents claim a certain, even though short, time for the restitution of the function after the mutilation of the cerebrum.

When my investigation of the function of the cerebrum began, about six years ago, it seemed important to me most of all to clarify this point in which Flourens agreed with the more recent experimental scientists. It seemed hardly believable to me that the larger part of the cerebrum should actually be totally superfluous, since supposedly a total restitution takes place nevertheless after a certain time upon the mutilation. In an extended sequence of tests I accomplished the task to cause most extensive destructions of the cerebral cortex and at the same time to keep the animals alive as long as possible for observation. In order to avoid bleedings, I flushed out the cerebral substance by a jet of water.

The main result of this sequence of tests was the proof that Flourens' doctrine, according to which a small rest of the cerebrum is still capable to execute all the functions of the cerebrum, rests on an error. An extensive destruction of both hemispheres always causes conspicuous, permanent disturbances of the higher psychical activities of the animal, especially the reduction of the intelligence. All of my predecessors, with the exception of Ferrier, had, strangely enough, not mentioned this phenomenon at all. Judging from the assertions made by Hitzig and his predecessors one could have believed that the cerebrum brings about the movements of the limbs, the muscle-sense, seeing, hearing, etc., but that it has nothing to do with the functions on the basis of which we deduce intelligence. Before me, only Ferrier declared that the frontal lobes, a part of the cerebrum, presides over what we call intelligence. But I have very carefully exempted the frontal lobes of my dogs in most cases and only destroyed the parietal lobes and the occipital lobes, and still I have observed, especially with these animals, the most prominent disturbance of their intelligence. Therefore, it cannot be maintained that the frontal lobes have to be considered chiefly as the center of intelligence.

In the cases of dogs with extensive flushing of the cerebrum I was able, in addition, during the first period after each operation, to regularly observe the same motor disturbances as have been accurately described by the old and the new authors. If the flushing affected one hemisphere, the animals are paralyzed on one side during the first days, hemiplegically. But soon the hemiplegia recedes and after weeks the dogs run around with the use of all their limbs, similar to healthy dogs. I could further confirm, in agreement with Hitzig and Ferrier, that, after a one-sided extensive destruction of the cerebral cortex, a dog is blind on the opposite side eye for the first days. But this blindness is only temporary. If it is possible to keep the animal alive, it will then learn to see again; but after great damages done it will show a very peculiar disturbance of the visual perception. Such a dog will, with great certainty, avoid obstacles when walking. In this respect it thus makes a very practical use of its visual impressions. But the animal does no longer get frightened when it is threatened with a fist or a whip. It is apathetic at the sight of people or strange animals. It does not recognize food thrown to him and knows to find it only very slowly. Munk, who has later confirmed this disturbance of the visual perception discovered by me, has introduced the expression of psychical blindness for this phenomenon. But since, by adopting this term, the error could creep in that I accepted the fantastic explanation Munk has given for this condition, I prefer to reject this term. Therefore I suggest to call the perceptional disturbance discovered by me cerebral blindness, or, even better, cerebral feebleness of vision.

The permanent disturbances of the remaining sensory perceptions after extensive mutilation of both hemispheres are by far not of such an evident nature, so that they escaped me at the beginning when my attention was captured by the very obvious manifestations of the cerebral feebleness of vision. Later I have proved that all sensory perceptions show permanent anomalies. The animals with extensive loss of substance of both hemispheres do still hear; for they make movements when a loud noise is produced in their vicinity, but they do not react to sound impression in the same way they did before the operation. For example, they no longer bark when someone knocks at the door and no longer participate in the barking of other dogs. They also do no longer know how to orient themselves with the aid of sound perceptions. When called, they often run away in a totally different direction. The faculty to perceive through scent or taste is also permanently damaged after an extensive mutilation of the cerebrum. Such dogs do not refuse to eat dogmeat, while a healthy dog disgustedly turns away from it. Tobacco smoke and chloroform vapor, for which normal dogs show the greatest disgust, are breathed in by the mutilated dogs with comparative equanimity. Finally, I have also found that the perceptions transmitted through the exterior skin are duller in every respect for such dogs. They still have a perceptive faculty in all points of the skin, but they only react to stronger stimuli. They do not mind remaining in cold water with their feet for a long time, express pain only after intensive pressure on the skin, and do not find their way easily by groping.

Thus the permanent functional disturbances after extensive mutilation of the cerebrum, not known before me, because nobody kept the animals alive for such a long time, consist of a disturbance of the intelligence in connection with strange disturbances of all sensory perceptions. I have called these permanent disturbances deficiency manifestations because they are caused by the loss of a large portion of the cerebral cortex. I attribute the highest importance to these deficiency manifestations. The temporary disorders which exist only a short time after the operation and then disappear, seem of lesser importance to me. I believe that these cannot be deduced from the removal of the cerebral cortex, because then they could not be evened out so fast. I much rather suppose that nerve centres located deeper which are not directly damaged, nevertheless experience a temporary restraint of their functions. When, with the healing of the wound, the restraint disappears, the centres paralyzed only temporarily take up their functions again, and the surprising establishment of apparently lost functions becomes evident.

This explanation which rests most of all on analogous experiences occurring after the injury of the spinal cord, has been attacked by many. But the recognition seems to break through more and more that it is advisable to distinguish between permanent and transient manifestations after injuries of the cerebrum.

The experiments just described at first only pursued my aim to obtain clarity in the question of restitution; but the results obtained justify my immediate expression of the most serious doubts as to all the modern localization doctrines by which the cerebral cortex is divided into small delineated fields with separate functions. I had never aimed at causing exact symmetrical lesions during my operations, but nevertheless the disturbances were always amazingly similar on both sides of the body. Whenever I observed very pronounced disturbances of the visual perception after the mutilation of the cerebral cortex, all the remaining sensory perceptions were also damaged. This did not seem reconcilable with the hypothesis of strictly defined centres for each sense.

When these and other considerations forced the conclusion upon me that the hypotheses of Hitzig, Ferrier and their successors could impossibly be correct, then this did not exclude, of course (the possibility) that some totally different kind of spatial distribution of the functions of the cerebrum does actually exist. In fact, I have been blamed (said) to deny any localization of the function of the cerebrum, but that is wrong. I absolutely believe it possible that the individual corteces of such a powerful organ as the cerebrum, have varying functions. But whether and how far this possibility proves true, that will still have to be investigated, dispassionately investigated.

[The following paragraphs deal with the details of his experiments and the conclusions he has come to.]

Upon having carried the work on the question of restitution to a sufficient completion, I have tried in the course of the last two years to actively advance the localization problem which I had only touched upon critically and occasionally. Since the flushing out method seemed inappropriately to me to cause clearly delineated losses of substance, I chose another method. I am using the drilling machine invented by White, by the aid of which I put small instruments I devised into fast rotations...In each separate operation I have at the most removed the fourth part of the cerebral cortex observable from above. In the case of some dogs, I have conducted 2,3,4 and more operations and have carefully observed the resulting manifestations of deficiencies.

A dog which has lost both frontal quadrants, i.e. the excitable zones of both sides, will be permanently idiotic. The observer immediately notices the stupid expression of the eyes. Such a dog who, according to Ferrier should have lost the so-called psycho-motor centres, according to Munk the sensory sphere, and according to Hitzig the muscle consciousness, runs around quite vividly, moves the lower jaws, the tongue, the tail, eyes, ears, thus shows no paralysis at all, while immediately after the operation he had a distinct weakness of the opposite side's limbs. At all points of his body the dog has sensory perceptions, although they are dull. He also has muscle consciousness in the sense Hitzig conceived this term; for he will not let his paws be put just in any position without soon moving them back. With Munk one could perhaps object by saying that at first the disturbances actually occurring after the removal of the so-called motor centres do diminish because these centres, or sensory spheres, as he calls them, have not been destroyed deep enough. In order to face this evasion, I have...[made deep incisions, being able to keep even these animals alive. Although the hemiplegia was of rather long duration in these cases, it disappeared almost completely and a few weeks later these animals could not be distinguished from the ones that have suffered only five millimeter deep lesions--not lesions of several centimeters!] I therefore have to affirm that my experiences cannot be reconciled with the doctrines of Hitzig, Ferrier and Munk. The only permanent motor disturbance I observed in dogs of which both of the parietal lobes had been removed, was that the animals have more clumsy movements, and that they do not know how to keep a bone in their front paws while nibbling on it.

A dog with destroyed posterior quadrants, i.e., the non-excitable zone according to Hitzig, as far as visible from above, on both sides, always appears much more idiotic than the one whose parietal lobes have been removed. This can probably be sufficiently explained by the fact that the entire cortex of the posterior lobe has a much larger circumference than the cortex of the parietal lobe. According to Hitzig and Munk such a dog should be totally blind, because he has lost his whole visual sphere. But this is absolutely not the case. Such animals manifest the signs of cerebral feebleness of vision, for example, they do not pay attention to threats, but they do know very well how to avoid obstacles and even recognize the approaching human from a certain distance. Besides the cerebral feebleness of vision there exists also a reduced capability to have perceptions with the aid of scent, taste or hearing. These experiences prove that the so-called visual sphere of Munk does not exclusively serve the visual sense in the first place, and in the second place that animals can have visual perception without a visual sphere.

Manifestations of deficiencies can be amazingly insignificant for a dog of which only one side of the grey cortex has been removed to a large extent. The animal whose skull I am showing you here, and whose cerebrum is represented in a drawing I will present to you, had a scarcely impaired intelligence. Although he seemed to prefer to use his left eye, when this was closed by us, we realize that he was able to see very well with it, showed fear when threatened, perceived meat from afar, etc.

While even a substantial destruction of the cortex of one hemisphere scarcely damaged the intelligence, the intelligence is reduced to an extraordinary degree when the loss of substance concerns both hemispheres and is large and deep in addition. The dog whose skull I am showing you here, has survived four large operations and has only been killed one year after the last operation. This dog was seriously idiotic and acted only like an eating machine of reflexes. He did not care about humans or animals. All his sensory perceptions were highly damaged, but we were able to observe that all his senses still functioned. He was neither deaf nor blind nor deprived of his gustatory or olfactory senses. Not a muscle of his body was paralyzed, not a point of his skin deprived of sensory perception. The cerebrum of this animal, hardened in Muller's fluid, weighs only 13 grams while the cerebrum of a healthy animal, hardened in the same fluid, weighs 90 grams. As a result of the removal of the cortex a very obvious secondary shrinking of the remaining cerebrum had taken place.

You will find detailed information about this case and other cases in my treatise in Pfluger's Archive soon to be published. At the same time, my collected treatises about the physiology of the cerebrum will appear in print.

[-Then he introduces the dog he has brought from Strassburg which has undergone five operations between November 15, 1880 and May 25, 1881, during the course of which the corteces of both parietal lobes and occipital lobes were removed. The frontal lobes and the temporal lobes were not touched. The dog represents one of his most interesting cases because of his lively movements, since generally dogs with extensive cerebral defects are very apathetic.

-In this paragraph he explains the behavior of the dog when his box is opened and when he moves around. Very often he raised himself without support of his front paws on his hind legs and walked around.

-He describes the peculiar disturbances of the sensory perceptions of the dog: idiotic expression of the eyes, no reaction to a finger held in front of his eyes, no expression of fear when threatened by a whip, a fist, an angry cat, a burning candle.

-Although the dog is not blind, as proven by the fact that he realizes the removal of the cover of his box (paragraph 22), avoids obstacles put in his way, and even sometimes avoids imagined obstacles. But he is deprived of his instinct to choose a warm spot to rest on, etc.] The dog shows very explicitly the manifestations of perception deficiencies which I have described as cerebral feebleness of vision.

Our dog hears, but he responds to sound impressions very differently than other dogs. He does not express fear when yelled at or when a whip is cracked. He never participates in the barking of other dogs.

-The dog now eats dog meat without any disgust. He also shows no dislike of tobacco smoke or chloroform vapor.

-He has sensory perceptions on all points of his skin. it was not necessary to investigate this fact any further because we had a little, lively dog, quite young, which loved to play. As soon as it was left with the feeble minded dog, the latter let himself be attacked in a playful way and even played with the little dog on occasions. But when the little dog bit him more seriously, he made loud noises of pain, got angry and tried to bite the little dog. Once he was angry, he would repel any further attacks of the small dog, growling and baring his teeth. By means of these observations, we made sure that mechanical abuse of the skin at any point of the body of this animal causes expressions of pain and actions of defense.

Our dog is by far not as deeply idiotic as was the animal the cerebrum of which to be demonstrated weighed only 13 grams. He is friendly to humans, cares little about other dogs except for traces of sexual drive, shows his content when let out, his discontent when locked up, shows no envy and lets other dogs take things away from him. He has retained the instinct of hiding his food and then later eating the hidden food when he is hungry again.

These descriptions are sufficient to prove that the animal has to be considered as a harmlessly idiotic being. Another experiment we conducted explains how much this dog has lost what we call intelligence. Placed in a spot surrounded by a very low fence, he does not know how to climb over it, although the fence only reaches up to his breast. In his box he pulls himself up along a barrier which is much higher than the fence. But he does not find the means to overcome the obstacle as he does in his box. He is too dumb to do that.

The autopsy of the cerebrum will show to hat extent the cortex of this animal has been destroyed. It is possible that somewhere a border portion of the so-called visual sphere on one or both sides has been spared. With forced efforts the proponents of the localization doctrine would then want to ascribe the visual functions which this animal still possesses to the retained sparse remains of the visual sphere. But it will be more difficult, if one wishes to insist on the localization hypotheses, to demonstrate why this animal also reacts differently than a normal dog to impressions of hearing and scent, when his auditory and olfactory spheres have not been attacked directly.

But it seems totally impossible to me to make the actual observations obtained with this dog agree with all those localization hypotheses which have been established in connection with the so-called excitable zone; if Munk's doctrine which posits his so-called sensory sphere into the excitable zone, had only a shadow of a foundation, then the dog would have to be numb on large portions of the skin of both halves of the body. He would have had to retain sensation only on the regions of the skin the sensory spheres of which are not destroyed. But we were able to convince ourselves that the animal has a sense of touch not only at the skin of the trunk, whose sensory sphere it still possesses, but also at the head, the limbs, and the tail; for it defends itself by biting against injury of these parts of the body.

It is equally easy to prove that this dog has muscle-consciousness in the sense of Hitzig even though he has lost the centres of the muscle-consciousness. In no way is he indifferent against the position of his paws but returns them to the original comfortable position immediately when they have been placed artificially into a divergent position.

Finally, with respect to the proponents of the psychomotor centres, it has to be pointed out in face of them that this dog, even though he has been deprived on both sides (in both hemispheres) of at least a large number of those centres, is nevertheless able to move all his muscles apparently voluntarily. He also does not only execute the general, more machine-like motions of walking and running. As we can see, he is capable of raising himself on his hind legs. He bites when he plays with a dog. He shows his content by wagging his tail. He often methodically scratches his head and other parts of the body. As I still want to mention, he tries to get rid of a cap which one tries to bind over his head in order to close his eyes, by stripping it off with both front paws. actually one would not want to call these actions simple reflex-movements, however.

But I have described that our dog really has some motor disturbances. His movements are more clumsy than the ones of healthy dogs. He slips easily and acts awkwardly in certain operations. Thus he does not know how to hold fast to a bone with his paws. These disturbances discovered by me seem to be the only ones which regularly remain after an extensive mutilation of the sliding lobes.
[Schleifenlappen]

Thus the experiences we made with this animal have confirmed the hypotheses I established earlier:

1) The cortex of the cerebrum is the organ of the higher psychic activities. Upon removal of large parts of both halves of the cerebrum the intelligence is reduced.

2) It is impossible to paralyze any muscle by destroying any one portion of the cerebral cortex. The mutilated animal retains the voluntary use of all muscles.

3) It is equally impossible to destroy any sensory activity permanently by destroying any one portion of the cerebral cortex. The animal retains all his senses. But after the removal of extensive portions of the cerebral cortex weakness of perception occurs.

4) Animals with destroyed parietal lobes have clumsier movements permanently and duller cutaneous perception than the animals with destroyed occipital lobes. Dogs with

destroyed occipital lobes are in general more idiotic than animals which have only lost their parietal lobes.

Gentlemen ! I herewith introduce the dog (to you) who has made the trip from Strassburg to London with me. The animal has lost by far the largest portion of the cortex substance of both parietal lobes and both occipital lobes, as I described it to you this morning. Of the five operations which resulted in this enormous mutilation of the cerebrum, the last one took place on May 25 of this year. The deformation of the skull of the animal is very obvious. If I hold his head, you can easily put several fingers next to each other into each of the enormous cavities on both sides.

Upon letting the dog roam around freely, you will see him wag his tail in a lively manner, walking to and fro, avoiding obstacles carefully. the movements of the four limbs are generally absolutely normal. Sometimes the animal slides, and that happens more often involving the hind paws than the front paws. Once in a while the dog hits objects with the left side of his body. In the movement of his limbs, of the head, the ears, the tongue and the tail no trace of asymmetry can be observed.

We now want to examine the individual sensory functions and observe in how far they show anomalies.

The position of the eyes of the dog shows no deviation. The pupils contract well upon the stimulation by light. When the animals moves his eyes, the muscles of both eyeballs act together as they should. But you will notice that the expression of the eyes is fixed, stupid. An intelligent dog concentrates his gaze at the eyes of the person looking at him. Our dog does not do that.

The fact that the dog can see is proven by his avoidance of obstacles. But visual impressions do not at all produce the same manifestations as in the case of unmutilated dogs. I threaten him now with my fist, while he approaches me wagging his tail in a friendly manner. You see that he does not pay any attention to the threat, but instead continues to wag his tail. I now take up this whip and move it back and forth, as if I wanted to beat the animal with it. But the dog does not show any signs of fear in face of the threatening whip. Just as little does he notice a burning candle. While a healthy dog takes off or at least turns his head away, our dog remains completely impassive and not even twinkles his eyelids if we bring the flame of the candle close to his eyes. You can see, therefore, that this animal is not moved to any motor reactions from which we could conclude that he has any reactions of fear, fright, interest or curiosity. This dog remains indifferent and apathetic in face of visual impressions.

In my discussion of this morning I told you that this dog even avoids imagined obstacles. I observed that, when walking around, he avoided a spot lit brightly by the sun. Because of this experience I made the following experiment with him. I had a flag sewn which consists of a glaringly white, broad strip of linen bordered by two black strips. If we put this flag on the ground, the dog avoids stepping on the white strip. I have brought the flag along and shall dare to repeat the test here.

(The lecturer spreads the flag on the floor of the hall. The strolling dog walks around the flag for several minutes. As soon as the flag has been removed, the animal soon steps again on the part of the floor which it avoided before.)

That the actions of this dog are governed by his visual impressions is, in addition, proved convincingly by the fact that his whole behavior changes when his eyes are artificially closed. I brought a cap along which I will put over his head. Thus no beam of light can now reach his eyes. You see how the animal which earlier avoided all obstacles, now hits his head repeatedly. You observe how he tries to pull off the cap with both his front paws. We are now taking off the cap and we see that he takes up his wanderings in the same way as before the cap was put on his head.

Our dog is just as little deaf as blind. But he does react to sound impressions in a different way than an uninjured animal. How he stands right in front of me. I violently yell at him: "Be off ! Forward ! Get away !" He does not flee, he does not give evidence of fear by any movement. Just as little can he be intimidated by the crack of a whip.

(The lecturer strongly cracks a whip.)

You see that the animal continues to wag his tail in a friendly manner. But you could perceive that the dog did hear the intense sound itself when he, immediately after the crack of the whip, made a movement with his head which could be interpreted as something like a sign of amazement. I will try to call him over to me. You see, that he heard my call; for he was at first startled and now keeps a faster pace, but the animal does not know how to choose the proper direction towards me, and errs around aimlessly.

This animal still possesses the faculty of smelling. When meeting other animals, it still shows a sexual drive. He knows how to find pieces of meat thrown to him by smelling them out. But you can see for yourself that he does not react to olfactory impressions as distinctly as an uninjured dog. He licks the human hand extended to him, but licks the face of a living cat held before him in a similar way. Cigarette smoke which is disagreeable to healthy dogs, leaves him quite unconcerned.

(One of the gentlemen present lights a cigar and blows the smoke against the nose of the dog. The dog does not flee and turns his head only when very thick smoke is blown at him.)

The cutaneous perception of the animal seems to be somewhat dulled all over; but it cannot be doubted at all that this dog possesses sensory perception at all points of the outer skin and the mucous membranes. He reacts to very more intense injury of the surface skin with signs of displeasure. It seems almost superfluous to support this fact by an experiment; but I will perform one anyhow in order to show you this little instrument which Mr. Ewald has constructed. This Aesthesiometer permits to expose a part of the body, for example a paw, to increasing pressure. The instrument, at the same time, contains a device which makes it possible to immediately release the pressure. When using this instrument, the pressure is increased up to the point when the animal shows a definite sensory manifestation, and then the squeezing tool is immediately released by a manual manipulation. We are now putting the left hind paw into the apparatus. With a slight tightening of the screw that causes the increase of the pressure, the animal squeals and tries to get loose.

From what I have told you, you will have, as I have, won the conviction that this dog has to be judged as being harmlessly idiotic. He still possesses all his senses, but he does not know to utilize his sensory perceptions as efficiently as an uninjured animal. Therefore, he is also not capable to provide any samples of reflection. He cannot cope with the smallest embarrassment. I will show you this by means of a very simple experiment. We have put up here a small enclosed fence, the height of which is so minimal that it reaches only the breast of the animal. We are now putting the dog into the enclosure and try to entice him by a call to climb over the fence. You see that the animal paces up and down within the enclosure, knocks against everything with his breast, bends his head over the fence, but can absolutely not reach the decision to climb over the very low fence. You may now believe that the animal is altogether not able to get up on his hind legs. But that is absolutely erroneous, as I will prove immediately. I now put the dog into this deep case in which he has spent the trip to London. As soon as I will have removed the cover without any noise, the dog will immediately appear, raising himself on his hind legs, and will put his front paws on the rim of the case wall.

(The lecturer opens the case, and the dog shows himself immediately.)

The wall of this case is more than double as high as the enclosure [I believe] to have shown that this dog does not show any kind of paralysis. His movements are somewhat clumsy and awkward; but he executes the most varying movements. Thus the dog would easily get out of the enclosure if we would employ the same muscle effort within the enclosure as was familiar to him in the case. No muscle is deprived of voluntary use. Thus he has to be classified as idiotic because he does not know how to use such a simple resource at his disposition in order to free himself from his dilemma.

Thus closing my demonstrations, gentlemen, I hope that, in addition, you have satisfied yourself about the fact that no sensory function of this animal has ceased to exist. It can see, hear, smell, feel. But the dog shows most noteworthy deviations in his attitude towards sensory perceptions. His actions are so inept, yes, partly even absurd, that there is no denying that he is devoid of the faculty of reflection and has to be declared idiotic.

The dog will be killed by chloroform within the next few days and then you will be able to see for yourself, by personal observation, the immense extension of the injury.

APPENDIX C:
NAMES AND DATES OF SIGNIFICANT PERSONS MENTIONED IN TEXT

A. Bain (1818-1903)
H.C. Bastian (1837-1915)
A.H. Bennett (1848-1901)
C. Bernard (1813-1878)
P. Broca (1824-1880)
J. Crichton Browne (1840-1938)
C.E. Brown-Sequard (1817-1894)
S. Burdon-Sanderson (1828-1905)
H. Cushing (1869-1939)
C. Darwin (1809-1882)
H. Duret (1849-1921)
D. Ferrier (1843-1928)
M.J.P. Flourens (1794-1867)
M. Foster (1836-1907)
G. T. Fritsch (1838-1897)
F.J. Gall (1758-1828)
C. Golgi (1844-1926)
R.J. Godlee (1849-1925)
F.L. Goltz (1834-1902)
W. Gowers (1845-1915)
H. Head (1861-1940)
E. Hitzig (1838-1907)
V. Horsley (1857-1916)
T.H. Huxley (1825-1895)
J.H. Jackson (1835-1911)
T. Laycock (1812-1876)
W. Bevan Lewis (1847-1929)
J. Lister (1827-1912)
W. MacCormac (1836-1901)
W. MacEwan (1848-1924)
H. Munk (1839-1912)
L. Pasteur (1822-1895)
W. Penfield (1891-1976)
S. Ramon y Cajal (1852-1934)
G. Rolleston (1829-1881)
E.A. Schafer (a.k.a. Sharpey-Shafer) (1850-1935)
C.S. Sherrington (1857-1952)
H. Spencer (1820-1903)
J.C. Spurzheim (1776-1832)

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