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• *ABSTRACT*

This paper examines the transformation of local uncertainties encountered by working scientists into global certainty, or 'scientific facts'. It discusses six mechanisms by which scientists transform local uncertainty: attributing certainty to the results of other fields; substituting processual for production evaluations in the face of technical failures; ideal type substitutions; shifting clinical and basic evaluation criteria; ad hoc generalizing of case studies; and the subsuming of epistemological questions in internal debates. The data are drawn from a study of late nineteenth-century British neurophysiologists (surgeons, neurologists, pathologists, physiologists). The approach is drawn from the sociology of work.

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Participants in a particular scientific approach continually reconcile or absorb anomalous material into its basic tenets. This has been called 'unfalsifiability' or 'disconfirmation bias' by some, and this allegiance defines, in many ways, a Kuhnian paradigm.[1] I argue here that this persistence of belief or participation, in the face of anomalies or contradictions, is the dependent variable of a sociological process which transforms local uncertainties into global certainty.

By global certainty, I am not referring to Kuhn's description of a 'critical mass' of anomalies which eventually force paradigmatic change.[2] Rather, when sites have very different kinds of local difficulties — from which they develop and maintain the same global perspective — several different types of transformations occur. A complex interplay between long- and short-term scientific goals, proximate and distant audiences, specific historical circumstances, and the active synthesis and planning of a coherent group can help produce and maintain this global commitment.

Perhaps by understanding the ways in which researchers have to manage these many contingencies, audiences, and time lines, we can begin to understand better how and why unfalsifiable' commitments are made, and also how the homely exigencies of daily scientific work become transformed or absorbed into widely accepted truths.

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Scientists and Uncertainty

Scientists constantly face uncertainty. Their experimental materials are recalcitrant; their organizational politics precarious; they may not know whether a given technique was correctly applied or interpreted; they must often rely on observations made in haste or by unskilled assistants.

As many observers of science have noted, these contingencies rarely appear in published descriptions of scientific work. Knorr-Cetina's and Latour and Bastide's work on the transmogrification and deletion of uncertainties from lab work to published reports have emphasized this point; similarly, Latour and Woolgar's *Laboratory Life* documents in some detail the transformation of everyday uncertainties into facts via 'deletion of modalities' and progressive reification. Lynch's work on artifacts and on neurobehavioural tests notes similar phenomena under two different types of conditions. The work of Woolgar and that of Pinch has also described a similar situation.[3]

One major thrust of this research thus far has been to document the *presentation* of data, from observation to publication, as increasingly certain. The published data reveal, rather than hint; articles state, rather than guess at; subjects line up and are counted — by the time they get to the journals, they don't run away and hide behind the lab equipment or try sabotaging experiments. These observations about the deletion of uncertainty have added valuable insight about the process of conducting science.

However, the deletions have rarely been described with reference to the details of *both* large-scale organizational/political factors *and* hands-on laboratory or clinical practice: they have also tended to focus on single cases or lines of work.[4] (The emphasis in one school of social studies of science has been literary or focused on discourse. Here, the dependent variable becomes language transformation via uncertainty deletion. This type of inquiry, especially, de-emphasizes institutional and work-related variables.[5])

Studies such as *Laboratory Life* richly document the reduction of complex laboratory results and uncertainties into vastly more simplified formats of formal scientific publications. Several aspects of this transformation might be further explored in order to understand multiple-site, long-term changes. These unexplored contingencies are central to understanding the transformation of uncertainty from the laboratory to larger-scale and longer-term enterprises, be they 'paradigms', schools of thought, or laws of nature. These contingencies are:

1. The long term effects of *combining* multiple sorts of evidence (for example, of clinical evidence with basic research); and the cumulative creation of certainty from multiple lines of evidence;
2. The *varieties* of uncertainty faced by scientists;
3. The double focus of uncertainty resolution: with respect both to local, daily work and to broader, longer-term issues;
4. The *sociology* of uncertainty as the collective behaviour of meeting contingencies at both these levels of analysis.

Collective actions of this sort are important topics for the sociology of science if we are to understand the organization of work leading to the points that Knorr-Cetina, Latour and Woolgar, and others have made about uncertainty.[6]

Scientists are intelligent problem-solvers, and their methods are sensible in the context of their constraints and resources. Scientists manage *local* uncertainty by the successful negotiation of a myriad of obstacles, and the articulation of difficulties present in any work site. These can include moral, political and managerial pressures, design complexities, access to research materials, information and skill, all of which may be recalcitrant or scarce. Scientists develop *global* certainty with reference to larger spheres of work and longer time frames — multiple sites and audiences, perhaps other disciplines and institutions, debates with opposing schools of thought.

Scientists in the field often characterize the management of local uncertainty as 'not really science' (and with perjoratives including 'mere administrative work', 'dirty politics', 'beancounting', 'mere logistics' or even 'sociology'). Real science, on the other hand, means contributing to Truth, *despite* these local 'glitches' or 'kludges'.[7] But global certainty develops as local uncertainties are jettisoned, minimized, distributed, or resolved. The management of local uncertainty, including its transformation to global certainty, is not incidental, but *central* to research organization, despite its deprecation by scientists.

We cannot assume that uncertainty management is universal or that uncertainty itself is monolithic. The conditions and responses to the uncertainties presented here are not historically or situationally invariant, nor do the types represent an exhaustive list. I hope this model for the complex transformation of daily uncertainty into larger historical contexts of action will be modified, extended and revised.[8] At times, political uncertainty will drive a problem-solving enterprise; at other times, diagnostic uncertainty prevents work from proceeding until it is resolved.

This paper falls between several disciplinary stools. Medical sociologists have written about the uncertainties of diagnosis or medical training, but scarcely dealt with those of basic medical research.[9] Sociologists of science, as noted above, have elaborated individual-site or line-of-work uncertainties and their resolution, but rarely multiple-site, multiple-line transformations. And, in the historical literature, when transformation of uncertainty is not simply analyzed as 'progress', uncertainties may be dealt with purely as technical discoveries or difficulties to be overcome.[10]

Although my substantive topic concerns medical research, I do not see the 'diagnostic' uncertainties referred to in this paper as in any way analytically distinct from those faced by other scientists. A taxonomist facing a fuzzy species identification question, a paleontologist with a scarce fossil record, an astronomer with a foggy lens or a maverick physiologist unable to receive NIH funding will be familiar with the types of uncertainties described below, and perhaps also with their transformations.[11]

General Background and Data

Early in the 1870s, a handful of researchers envisioned a map of the brain which would match each mental function with a single physical area; such a map would help order an array of puzzling malfunctions, including epilepsy, aphasia, and the effects of brain tumours, syphilis and tuberculosis. Most of the medical and scientific community ridiculed their initial mapping efforts as 'neo-phrenology'. But by 1906, the map and its underlying premises had become an unquestioned fact in the medical and physiological world of the West.

Between 1870 and 1906, the physicians and physiologists mapping the brain faced constant, severe uncertainties, most of which were never resolved. How these researchers moved from ridicule to prominence is a long and complex story.[12] This paper explores one aspect: how the different kinds of daily uncertainty in the laboratory, at the bedside or in the surgical theatre became transformed into the unquestioned premises of the map of localized functions.

The commitment of this group of researchers to a model of brain function which would locate behaviours in a physical substrate began in a time of professional upheaval. In medicine, physiology, and in other areas of science which touched on the 'mind' or 'soul' there were heated debates about human nature, the limits of science, and the notion of 'progress'. The scientific stakes for demonstrating a physical basis for mind were extraordinarily high in terms of the audiences affected and the resources in jeopardy.

In some ways the 'hows' of the transformations from daily uncertainty to the commitment to localizationist ideology are easier to describe than the 'whys'. It is easy to think of this commitment as somehow emerging from the 'spirit of the age' in Victorian England: laissez-faire capitalism, materialism, scientism and new divisions of labour. While all of these may be true — intuitively, for example, it is easy to see individualism and free enterprise reflected in a model of the brain that has independent, enterprising parts of its own — I feel that a more detailed picture is called for, one which includes the material exigencies of daily work and institutional form. There were important interactions between daily contingencies (such as the clinical management of desperate patients with brain cancer) and larger-scale images, paradigms or pictures of nature which emerged during this period.[13]

From the beginning, the group I studied bet their scientific reputations and resources on functional maps of the brain. They succeeded in convincing the medical and scientific world that this model was correct through a number of strategies: entraining the resources of medical professionalization in the service of the theory; eliminating opposing points of view via gaining ownership of the means of knowledge production (journals, teaching posts, and so on); linking a successful clinical programme (and a desperate population) with the model; and uniting (with powerful scientists from other fields, such as Darwin and Huxley from evolutionary biology) against a common enemy (antivivisectionists).[14]

In simplest form, localizationist theories of brain function state that certain parts of the brain are responsible for specific functions — for example, a 'speech area' or 'motor cortex'. Scientists from several lines of work were involved in building this theory. They faced different contingencies and uncertainties, and resolved them according to the practical demands of their respective situations. Briefly, the lines of work involved in British neurophysiology during this period were:[15]

— *Neurology*. Neurologists classified and located nervous diseases, tested, diagnosed, and examined patients, wrote up and published cases, and exchanged information with support staff, including house (general) physicians. Much of their work consisted of administering and refining tests.

— *Surgery*. Surgeons assessed whether operation for tumour was possible, located disease or tissue damage, and removed tumours or repaired lesions. They invented new operative techniques and instruments, and also published cases.

— *Pathology*. Pathologists obtained their own materials, including negotiating postmortem permissions. They dissected, sectioned, and stained tumours or other tissue. They then microscopically examined this material.

— *Physiology*. Physiologists tried to recreate, in animals, the malfunctions of human disease. Their techniques included surgical removal of tissue and applying electricity to the exposed cortex to chart nervous pathways and reactions.

I focused my study on a group of British researchers based at the National Hospital for the Paralyzed and Epileptic, Queen Square, London,[16] several of whom became quite famous: David Ferrier, John Hughlings Jackson, Victor Horsley and William Gowers. Queen Square was one of many specialty hospitals which developed in the mid-nineteenth century in England, and was, like many of them, primarily a charity hospital. Many important medical developments informed localizationist research in this period: brain surgery was developed;[17] antisepsis became accepted and properly practised; instead of being assigned to general wards, patients were more frequently grouped into disease categories in special wards or specialty hospitals, making comparative study possible; and methods for physiological experimentation on animal nervous systems were more fully developed.[18]

Types of Local Uncertainty

Workers in all four lines of work experienced great uncertainty. *Taxonomic* uncertainty arose as they tried to develop classification systems (for example, what kind of thing was epilepsy?). *Diagnostic* uncertainty appeared as they attempted to apply classifications (for example, did this patient *have* epilepsy?). *Political* uncertainty arose in the course of creating or maintaining divisions of labour, collaborations, and alliances. *Technical* uncertainty was created by the vicissitudes of instruments and experimental materials.

Local uncertainty emerged in the face of the following conditions:

— Physiological events which were rare, unstable, episodic, self-reversing or spreading.

— Individual differences and lack of information for comparison.

— Materials or data which were difficult to obtain or recalcitrant to work with; other uncontrollable experimental or clinical conditions.

— Specialist-nonspecialist handling of the same cases.

— Unequal status between investigators; poor management-staff relations.

— Financial insecurity.

— Delays and lack of communication between investigators.

Researchers transformed taxonomic (or in medical terminology, nosological) uncertainty into ideal types. Diagnostic uncertainty gave way to elaborated taxonomies and a search for definitive symptoms. Political uncertainties formed the basis for development of specific markets, specializations, and alliances. Technical uncertainty was handled by cutting problems into small pieces, standardizing techniques, and jettisoning intractable anomalies.

Taxonomic Uncertainty

Classification was a difficult problem with nervous diseases — symptom configurations both overlapped and were highly variable within groups. In addition, brain tumours and some nervous diseases were quite rare. Arriving at a 'typical picture' of a given neurological disease was particularly complicated for these reasons.

From the early part of this period (roughly 1860–70), Queen Square (which was founded in 1860), admitted patients by symptom: fits, paralysis, and inability to speak were the most common. Physicians there were faced with the problem of constructing disease categories from a heterogeneous patient group: those with tuberculosis, neuraesthenia, syphilis, stroke, tumours, lesions, deficiency diseases, lead poisoning, and so on. Symptoms could also be transient within groups: patients would sometimes relearn or reproduce lost functions after damage or with slow tumour growth. The *episodic nature* of symptoms, their *transience*, and the *overlaps* between disease categories made finding a typical picture extremely difficult.[19]

From ancient times, epilepsy had been a medical and social mystery; it had many forms, perhaps constituted many different diseases, and certainly had many different catalysts.[20] Brain tumours were equally vague and multiform. Symptoms included those common to many diseases: nausea, headache, dizziness, vision difficulties. Furthermore, because brain tumours were rare, each case's individual quirks loomed large in constructing the taxonomic picture. Comparative samples were impossible.

Taxonomic uncertainty in the physiological work of the 1870s and '80s, arose from similar conditions. The physiologist's mandate was to discover the bases of nervous function, primarily by reproducing human disease conditions in healthy animals. Taxonomic uncertainty included: to what extent ablations or lesions could be compared with human diseases; and how differences between animals of the same or different species affected the comparability of results.[21]

A clean link between cortical areas, and resulting movements or dysfunctions in experimental animals was requisite for an unambiguous taxonomy. However, uncertainty arose in the application of techniques designed to elicit these responses. Electrical current spreads and cannot be contained in one area. Critics attributed many of the muscle-movement results which Ferrier obtained in his early (1870s) research to diffusion of current over the surface of the brain or into deeper structures, thus claiming that his results did not indicate localized areas, but rather procedural artifacts.[22] (A similar 'spreading' effect was observed in surgical procedures where infection or haemorrhage distorted the boundaries of the areas under investigation, with similar criticisms.)

Lack of information or materials could also contribute to taxonomic uncertainty. There were very few neurosurgeons during this entire period, thus few colleagues with whom they could compare cases.[23] Because of antivivisection restrictions, which were severely enforced after the 1876 Antivivisection Act was passed, surgeons also had difficulty obtaining animals for practice; similarly, pathologists had difficulty in obtaining materials. It was hard to get permission to do postmortems, and once permission was given, to do careful work.[24] Because of lack of comparative data, what constituted a 'typical' disease could not be clearly characterized.[25]

There were two major responses to taxonomic uncertainty:

(a) *Standardization*. One of the comparable types of clinical information was the temporal progression of symptoms through the body — for example, epileptic fits could start in a fingertip and move up an arm, gradually involving a whole side of the body. Beginning in the 1860s, doctors, patients, kin, and attendants tracked this 'march' of symptoms and transformed it into taxonomic data.[26] As localization theory gained prominence, Jackson (and later, Ferrier) linked this 'march' with specific areas of the brain, sorting fits by *region* of origin. After about 1875, and with increasing refinement, physicians used this method to classify epilepsy. To facilitate recording, neurologists made pre-printed 'fits sheets'. The location of the spasms and temporal particulars were checked off on these forms. They helped sift taxonomic uncertainty in a number of ways: they provided a checklist so that categories did not have to be regenerated for the natural history of each fit; recording was speeded up and thus more information within a narrow range could be gathered; fuzzy intercategory data was ruled out by the checklist nature of the sheet.[27] Physiologists, too, attempted to standardize their procedures, materials and protocols in order to get clear data for taxonomic purposes. After his early experiments, Ferrier, for example, began using pre-printed outline sketches of brains, in order to label functional areas according to muscle reactions.[28]

(b) *Exemplary cases*. Another response to taxonomic uncertainty was to find *exemplary*, unambiguous cases of particular diseases. Such cases were often the ones written up in the medical journals or presented in classrooms. By thus filtering for 'classic' cases, physicians were able to create taxonomies untainted by overlapping symptom boundaries.[29]

Diagnostic Uncertainty

Diagnostic uncertainty arose when investigators tried to fit individual cases into a classification scheme. Taxonomic and diagnostic uncertainty are interactive: if you are not sure what a given disease *should* typically look like, how do you tell if someone has it? If symptoms are highly variable, or hard to identify, how do you create a typical picture?

Diseases of the nervous system are very labile, and rarely contained in location or effect.[30] Tumours did not fit maps of the brain or grow in neat patterns; rather, they expanded beyond the boundaries of functional neurological theories. They also had many unpredictable side effects, including increases in intracranial pressure, scar tissue, and changes in veins and other circulatory patterns. Infections were common and uncontrollable, including some unique to brain operations (for example, brain fungus and meningitis). Individuals produced widely varying 'pictures' of different nervous diseases, another source of uncertainty.[31]

Both surgeons and neurologists faced uncertainty due to the fact that advanced syphilis and brain tumours shared many symptoms — paralysis, epileptic-like fits, blindness. Conservative physicians wanted *all* patients with these symptoms to undergo a prophylactic course of treatment for syphilis *before* the diagnosis of brain tumour would even be considered. This meant a delay of at least six weeks.[32] Both surgeons and neurologists bitterly fought this conservativism but it continued to interfere with diagnosis and treatment until the Wasserman test was developed in 1908.[33]

There were many responses to diagnostic uncertainty, several of which involved postponing decisions or substituting information:

(a) *Temporal segmentation*. At admission, patients with fits, aphasia or paralysis were provisionally diagnosed. These categories were then often refined according to surgical or pathological data after the patient's death. The immediate diagnostic uncertainty was often thus postponed; and postmortem or physiological evidence added to the body of taxonomic, not diagnostic data. It was not immediately helpful with daily clinical problems. Many classifications were only provable after patients died, or by indirect inference. The uncertainty, then, was *temporally segmented* within the workplace.[34]

(b) *Substitution of Taxonomy for Diagnosis*. Diagnostic uncertainty in neurology was rarely addressed directly in terms of better neurological exam techniques per se, or better test equipment. Rather, neurologists focused on improving taxonomic category systems. The familiar reflex test battery of modern neurology, for example, was developed at Queen Square during this period. During the latter part of the period, neurologists relied heavily on general taxonomic theories to solve diagnostic puzzles.[35]

(c) *Division of labour*. Neurologists often hired out diagnostic and taxonomic consultants. For instance, Semon, an ear-nose-and-throat doctor, was consulted on a regular basis to see if aphasic patients were unable to speak due to organic disease of the larynx. Various opthalmologists were also frequently consulted for patients with vision loss. Treatment was also segmented — lower-status physicians gave electrical treatment; nurses and attendants administered massages and bromides. Neurologists were thus free to concern themselves with elaborating taxonomies, lecturing, and writing articles.[36] This separation of care and diagnosis made both more certain in the short run, since attendants and consultants screened out complications and side effects, and collected data for the forms; neurologists culled unambiguous cases for publication and demonstrations.

(d) *Pathognomic signs*. Another response of neurologists to diagnostic uncertainty was to seek a *pathognomic sign*, indicating a unique disease.[37] Neurologists early on thought that they had such a sign in optic neuritis, an inflammation of the optic nerve, directly visible on physical examination. Other signs provided more ambiguous information; a missing knee reflex, for example, could indicate several different conditions. In 1871, Jackson said that optic neuritis was the most common opthalmological condition in cerebral disease. By 1877, he was convinced that without opthalmology, methodical investigation of diseases of the nervous system was not merely difficult but impossible.[38]

(e) *Shotgun treatments*. 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, 'metallotherapy', even leeches.[39]

Political Uncertainty

Political uncertainty arose when information was lost or trapped due to the local division of labour or the circumstances of alliances and collaborations. There were often gaps in communication between doctors and patients, physicians and surgeons, or caretaking staff and management. Political uncertainty also arose from precarious sponsor relationships, funding negotiations, conflict about professional status, and debates with opposing schools of thought.

While the specialized nature of the hospital at Queen Square created research opportunities, it increased uncertainties in the relationships between specialists and nonspecialists. Nonspecialists often did not know how to diagnose nervous diseases — even specialists had only just developed their tests in the 1870s and '80s — and patients were often 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 elsewhere.[40]

During the nineteenth century, British surgeons were fighting for equal status with physicians. While surgeons had achieved nominal equality with physicians in professional associations by the 1880s, there were still significant inequities.[41] Physicians had control of patient care, and surgeons had to bargain with them to obtain cases. Thus relationships between physicians and surgeons were a source of uncertainty and antagonism.[42] Cushing also described the different skills and interests of surgeons and neurologists:

> . . . the neurologist spends days or weeks in working out the presumable location and nature of, let us say, a cerebral tumour. 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 tumour 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.[43]

Political uncertainty in clinical work often arose from management-staff conflicts. There was continual friction between the hospital administrator, the board of governors, and doctors. One bone of contention was control of admissions. Physicians wanted exclusive admitting rights, especially for those with acute, scientifically interesting cases. Hospital administrators and governors wanted to control admission for financial and political reasons. When administrators controlled admissions, physicians never knew when there would be an empty bed, or with what illnesses they would be confronted; when physicians controlled them, income was unpredictable.[44]

The major political uncertainty for physiology was lack of independent funding or institutional security, due in part to opposition from antivivisectionists, and in part to the lack of British resources available for physiological research.[45]

There were three main responses to political uncertainty:

(a) *Dividing groups and treatment responsibilities*. Political uncertainty between physicians and surgeons was handled in part by segregating patients with clearly operable tumours. Physicians submitted their diagnoses to surgeons for review for possible brain surgery, then helped surgeons locate tumours. Other patients were cared for exclusively by physicians and support staff.[46]

(b) *Focus on clinical impact.* Institutional precariousness was met with a conservative anatomically-oriented approach geared toward clinical validation. Physiologists did struggle to gain an institutional base separate from medicine, but in England this was successful only much later. Experimenters used clinical successes to validate localization theory, and then used the theory to legitimate vivisection.[47] In banding together to form The Physiological Society for the purposes of combatting antivivisection, physiologists (who were often physicians) formed powerful cross-disciplinary alliances. This linked pro-vivisection approaches with physiology.[48]

(c) *Focus on minute details.* The political uncertainties presented by pathological work required dealing with scarce materials and stigma. Pathologists took samples when they could, preserved them as well as they could, and focused on minute details of individual cases, especially of brain tumours. They relied on case-by-case microscopic examination. The rest of the body, and environmental conditions, were ignored.

Technical Uncertainty

Technical uncertainty developed as a result of inadequate tools or ambiguous information about techniques. The experimental subjects used by Ferrier and others included dogs, rabbits, monkeys, birds, even jackals.[49] Technical uncertainty arose from difficulties with subjects, equipment and procedures, including lack of standard measures.

The difficulty of working with reacting subjects, particularly monkeys, is vividly conveyed in Ferrier's laboratory notebooks. His notes 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 'mischievous', hostile, 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.[50]

In addition to being reactive in a behavioural sense, the animals were physiologically fragile. Damage to subjects, including operative complications, was common. The notebooks record frequent accidental deaths from haemorrhage or chloroform overdose.[51]

In 1873, at the time of Ferrier's initial experiments, antisepsis was not standard even for humans; there was also a belief that animals were resistant to infection, and that precautions need not be taken for operations.[52] Rabagliati, in describing the work of Munk, said 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'.[53] Where to place electrodes, how to make incisions, how to create lesions and then control them, were all problematic. While many of these technical problems were either resolved or standardized by the turn of the century, others, such as control and specificity, remain important even today.

Ferrier's exemplary experiments consisted of opening the skull of an animal and systematically applying electrical current to a sequence of minute cortical regions. He then recorded subsequent muscle movements. The points of application were carefully numbered. If indeed the animal survived surgery, identifying the various centres accurately, accounting for individual anatomical variations and controlling the amount and kind of electrical current were major sources of artifact and confusion.[54] Ferrier's notes depict these difficulties:

> No application could be made nor could the electrolisation be made to be localised.
>
> This movement was very difficult to analyse as the brain very speedily lost its excitability.
>
> Hard to distinguish where the electrodes were due to the bleeding.[55]

He speaks of animals exhausted, in states of stupor.[56] 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 whose sequelae Ferrier is not 'sure if existed before.'[57]

Again, investigators had several responses to these conditions:

(a) *Standardization*. Here again, standardization was a response to uncertainty and was concurrent with the adoption of notation conventions. Technical arguments about ambiguities in technique resulted in the adoption of one kind of current, standard incisions, and universal use of antiseptic procedures. Conventions for notating brain areas and current were adopted, and the types of animals experimented on became increasingly standard.

(b) *Shift down in focus*. Technical uncertainty was in part met by circumscribing the scope of the problems addressed. Famous experiments by Beevor and Horsley in the 1890s attempted to chart minute regions precisely; the uncertainty produced by trying to analyze larger areas was thus circumvented.[58]

(c) *Substitution of theoretical validity for technical consistency*. But many of the technical uncertainties faced by localizationists were and continued to be uncontrollable. One response to this on the part of localizationists was to substitute theoretical validity for technical consistency, and in the process to ignore intractable anomalies. For example, localizationists kept encountering the following anomaly: a lesion occurred in an area of the brain thought to be responsible for a function, but the function remained intact. Instead of abandoning either their techniques or theories, they categorized these (frequent) incidents as exceptions, and substituted theoretical validity for technical consistency.[59]

Analysis of Data

In sum, the local uncertainties faced by researchers included the conditions and responses summarized in Table 1.

Despite the many local uncertainties which they experienced, researchers were confident about the long-range global validity and reliability of their results:

> 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.[60]

TABLE 1
Summary of Local Uncertainties

	Conditions	Responses
Taxonomy	Rarity of events Multiple effects from single causes Individual differences Materials difficult to obtain Instability of events (episodic, self-reversing, spreading) Partially available data Diverse populations for study Unclear boundaries of causal phenomena; lack of causal models Uncontrollable experimental conditions Lack of information for comparison	Order data sequentially or by spatial location Standardize Find exemplars — filter for clearcut cases
Diagnosis	Labile and spreading symptoms and side effects Delayed information Individual differences	Segmentation of uncertainty Substitution of taxonomy for diagnosis Division of labour Search for sine qua non Shotgun treatments
Political	Specialist/nonspecialist handling of the same cases Unequal status between investigators Poor management-staff relations Lack of funding, sporadic funding; lack of institutional security Delays and lack of communication between investigators	Division of data along political lines for clarity Division of labour along technical/substantive lines Observations are limited to available materials Utilitarian emphasis on practical results
Technical	Reacting subjects Operative complications (experimental) Uncontrollable procedures Observational difficulties	Standardization of techniques and materials Standardization of observational techniques (forms) and protocols Observations are downfocused to smaller areas Substitution of theoretical validity for technical consistency

And Ferrier said:

> 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 a definite stimulation of whatever nature.[61]

Even Rawlings, the hospital administrator, seemed convinced of the certainty of localizationist research:

> 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 conditions 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 interrogation of science every fibre and filament of the complex structure yielded up the secret of its being.[62]

It would be easy to dismiss such claims as simply braggadocio or exaggeration without adequate substantive bases. But such an analysis is too simple, as well as *ad hominem*. The general public and the medical profession, not only the researchers involved (given their initial commitment), claimed certainty for localizationist findings. The laboratories and procedures were public, and in no way employed significantly different procedures from those common in science then or now.[63] The question is, then, how local uncertainties became global certainty.

Creating and Maintaining Global Certainty

Several mechanisms were involved in the transformation of local uncertainty into global certainty at the institutional level. Several of these involved substitutions of various types;[64] all are rooted in work organization and political contexts. The mechanisms of transformation are:

1. Attributing certainty across disciplinary boundaries.
2. Substituting processual evaluations for technical failures.
3. Transforming ideal types into theoretical goals; with backing of sponsor, jettisoning anomalies and individual differences.
4. Ad hoc generalizing of case results.
5. Oscillation of evaluation criteria between clinical and basic concerns.
6. Debates subsuming epistemological questions.

Let us discuss each of these in turn.

1. *Attributing Certainty to Other Fields*

Evidence for localization was collected from several areas. As these were triangulated to describe the same phenomenon in a general argument, local contingencies from one field became invisible. Researchers tended to attribute certainty to *other* fields: physiologists relied on clinical evidence to supplement their anomalous or uncertain results; pathologists turned to physiological evidence when they could not find evidence for discrete areas. For example, an anatomical atlas by Campbell contains the following passage:

> 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 . . . 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.[65]

Campbell does not discuss the clinical work in detail, nor criticize it, in this volume. Thus, the effect is that clinical complications are shuffled into basic research domains, and basic research anomalies are shuffled into clinical domains.[66] The shuffled evidence became interlocked; anomalies were passed between lines of evidence; local uncertainties became buried and work processes rendered invisible, but the theory gained credibility as many fields added evidence. Because of the attribution of certainty across disciplinary lines, it was impossible for researchers to trace a simple *path* of uncertainty, responses, or negotiation of anomalous results. Lines of evidence became tangled, both in publication and demonstration, but there was no public way of verifying this nor of realizing it was happening, since the details of work were embedded in the local contexts.

2. *Substituting Processual Evaluations for Technical Failure*

Members of a profession often account for failures not as mistakes or threats to validity, but as processes which can only be understood by insiders. Bucher and Stelling call this the development of 'vocabularies of realism', and note that it is an important aspect of professional socialization. Such substitutions appear as expressions such as 'the patient died but the operation was a success'. There is a focus on 'doing one's best' and 'recognition of limitation', sometimes by ignoring the *outcome* of a given procedure.[67] An example of the use of vocabularies of realism in place of technical failures is found in this report from a contemporary of Horsley's:

> 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.[68]

The substitution of processual evaluations helped transform local clinical uncertainty to global certainty by focusing on the certain aspects of localization and ignoring others. A similar strategy can be seen in the oscillation of clinical and basic criteria.

3. *Ideal Type Substitution*

There was a vast demand in medical research for textbooks, atlases, and other representations which depicted typical diseases. Spillane discusses the widespread adoption of Gowers' textbook of neurology ('the bible of neurology'), which contained functional brain atlases. Jones' notes from his medical training at University College Hospital also indicate the desire for, and dissemination of, such information.[69] In the process of resolving taxonomic uncertainty, researchers thus created typical pictures of diseases. These pictures often included functional anatomical maps — for example, the *source* of loss of speech is *located* in a particular part of the brain. These maps were substituted for irregular or anomalous findings from the local site. The enormous demand for functional anatomical representations in medical education, diagnosis, and texts represented a market intolerant of ambiguity, and of individual differences. The theory became unambiguously packaged into the atlas. The ideal types represented in such maps were sold as context-independent (that is, as *the* brain, not *a* brain).[70]

Crucially, localizationists had the backing of sponsors who overlooked anomalies and even assisted in the process of deleting them. Ferrier was briefly sponsored by a small grant from the Royal Society to conduct his early 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 referee 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.[71]

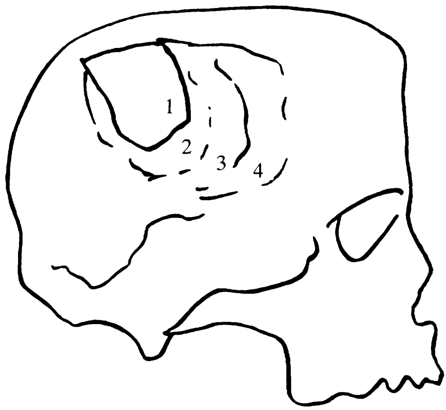
Rolleston goes on to suggest that Ferrier change the location of the numbers in question on the diagram. He also notes that Ferrier's earlier paper reporting work done at West Riding had circles and numbers placed differently, and suggests that Ferrier standardize the two sets of drawings. Note that the suggestion is to *standardize* the diagrams, not to redo the experiments in light of what might have been seen as inconsistent findings.

4. *Shifting Clinical and Basic Criteria*

Once a patient was diagnosed as having a brain tumour, neurologists tried to localize it. Localizations often failed.[72] In the casebooks at Queen Square, I discovered a tracing that Horsley had made of his incision through the skull of a tumour patient. The drawing shows four separate openings, made successively in the operation as the tumour was repeatedly not found (see Figure 1). Ultimately, the hole encompassed nearly one quarter of the entire skull area.[73]

Yet these failures (anomalies and uncertainties) on the local level were not presented as invalidating evidence at a global level. In part, this was due to shifting evaluation criteria at the reporting stage. For example, Gowers and Horsley collaborated on the first operation for removal of a spinal tumour. Gowers performed

FIGURE 1
Progressive Holes Cut for Tumour



Source: From Horsley's sketch, in Ferrier's casebook (National Hospital Records, 1886).

the neurological localization tests. When Horsley opened the spine at the vertebra which Gowers had indicated, he found no tumour. He had to remove several more sections of the man's spine before he found the tumour. However, because of the *clinical* cure, the operation was claimed as a successful instance of localization. On the other hand, Knapp and Bradford reported the death of a patient after 45 minutes (in 1889). But in this instance, the operation was acclaimed as a 'success', because the tumour was located. By shifting evaluation criteria in this way, local uncertainties were obscured and evidence adduced in favour of global certainty.

5. *Ad Hoc Generalizing of Case Results*

The conventions for publishing medical research in the nineteenth century included detailed case reports. Minute observations about patient progress, techniques, and medication were intermingled with basic research hypotheses. Such reporting was an important contributor to the transformation of uncertainty. Localizationist researchers worked in several loosely coupled sites.

Because of the reporting conventions, no controls (such as refereed papers or stringent methodological requirements) operated on the flow of information from research site to publication; information was added piecemeal and often in an inconsistent manner.

Thus, *challenges* to local validity were distributed. Researchers assembled evidence from both clinical cases and physiological experiments in making their case for localization of function.[74] In debates (both internal and external), evaluation shifted from clinical to basic. The distributed organization of research and the conventions of reporting in medicine de-emphasized local uncertainty. In addition, the oscillation between clinical and basic criteria for evaluation of evidence meant that clinical anomalies could be answered with basic rationales, and vice versa, as noted above. Thus, two pools of criteria for global success were available, while local uncertainties went unreported.

6. *Internal Debates Subsume Epistemological Questions*

Localizationism was actively opposed by another school of thought, 'diffusionism', or what today might be called 'wholism'. Diffusionists denied that functions could be located in particular parts of the brain.[75] Much of the evidence collected to prove localization was directed at rebutting diffusionists. This focused arguments [against] another position, not on establishing positive proof. Counterpoints to arguments raised from outside the localizationist research endeavour thus often served to bury local uncertainties as high-level issues were debated.

However, debates about technique, method, and so on, also took place *within* the localizationist camp, and here it is important to remember that *arguments within a position strengthen that position*. The more localizationists argued with one another about *how* to do, for example, ablation experiments, the less salient the question of *whether* to do them became. Higher-level uncertainties thus became transformed into more manageable, lower-level ones. Debate, both within localizationism and between camps, thus subsumed epistemological uncertainty.

Conclusion

Scientists work simultaneously in two incommensurate contexts: local settings and disciplines.[76] In the local setting, specific *actions* resolve uncertainty. These actions 'satisfice', in Simon's terms — it is often too expensive to obtain all the decision information needed to act.[77] Management of uncertainty in the local work setting is heuristic, often ad hoc, and tailored to cope with the material and political exigencies of the given local situation. On the other hand, scientists also justify their actions to larger audiences as contributors to a discipline. The results that they publish in this context cannot, by convention, reflect much local contingency. One of the mandates of science is to create generalizable, even universal results, and this is often conflated with deletion of local contingency.

The result of balancing the two contexts results in a series of strategies which must satisfy local constraints *and* create global certainty. Two central points of my analysis have been 1) to define types of local uncertainty and to point out their different consequences, and 2) to analyze the mechanisms by which such local uncertainty supports, creates and maintains a larger perspective.

Although my data are drawn from nineteenth-century neurophysiology, the analysis is more generally applicable. Taxonomic uncertainty is common, as Volberg's study of the species question in biology demonstrates. She notes a similar ideal type substitution as scientists in different fields encounter rare, overlapping, or ephemeral phenomena, and respond with a search for species exemplars. Yet the basic problem of defining 'species' remains technically unsolved.[78]

Similarly, diagnostic puzzles are common, and not simply in medicine. Engineers, for example, often face 'buggy' systems which exhibit common symptoms in varying configurations, with individual differences. Much engineering standardization and delegation of technical work, like that described above, may have arisen as an attempt to resolve these uncertainties.[79]

Political uncertainty is omnipresent in small business endeavours and educational enterprises, professional reform movements, new specialties, unstable bureaucracies or those with information processing overloads,[80] to name a few. The development of 'safe' commercial products, a focus on small details and the segmentation of organizations into those dealing with uncertainty and those performing routine tasks are often responses to political uncertainty of the type described here.

Finally, the substitution of theoretical validity for technical certainty, in the face of technically-based uncertainty is common to problem-solving endeavours. The history of psychoanalysis and its critics, for example, shows a continuous debate about the merits of the theory in light of technical uncertainty.

The local contingencies of work are not normally criticized by outsiders, nor open to debate. Thus, when descriptions of them are absent, debates proceed on the basis of published work. Creating and applying classification systems, managing organizational and technical problems, especially of recalcitrant materials, are always part of scientific work.[81] The framework presented here needs to be tested and elaborated in a number of sites, both contemporary and historical, in order to understand more about the process of managing different types of uncertainty at different levels of organizational analysis.

Uncertainty and the Sociology of Science

Much recent work in the sociology and philosophy of science has focused on the production of facts, the treatment of anomalies, and the degree to which theories, once established, are not subject to disconfirmation. Many researchers have noted the obduracy of established perspectives or paradigms, and the degree to which anomalous information may be ignored if it disconfirms basic assumptions.[82] Sometimes this is attributed to qualities of the problems themselves; sometimes it is attributed to institutional factors which determine the acceptability of findings. New intermediary variables are sometimes introduced to bridge the hypothesized gap between 'internal' and 'external'.

Yet despite the wide interest in paradigms and fact production, the obduracy of perspectives has rarely been analyzed processually as a characteristic of complex, multiple-site work organization. The analysis presented here posits that the transformation of local uncertainty into global certainty is one important aspect of obduracy and the persistence of theories.[83] The transformation is rooted in work organization. This includes local, daily work and larger institutional and political contexts and contingencies, including sponsorship and the uses of a theory by various audiences.

Obduracy arises as a process. It is the establishment of conventions of production and use, alliances, evaluation criteria, standard operating procedures, and agendas. The continuing power of a perspective in the face of anomalous evidence is an inextricable part of scientific work organization because scientists must work at both the local and global levels, because they must face many kinds of uncertainty.

• *NOTES*

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1. See, for example, G. Holton, *Thematic Origins of Scientific Thought* (Cambridge, Mass.: Harvard University Press, 1973); M. Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (New York: Harper and Row, 1964); I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).

2. T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 2nd edn, 1970).

3. See, for example, J. Dewey, A. W. Moore, H. C. Brown, G. H. Mead, B. H. Bode, H. W. Stuart, J. H. Tufts and H. M. Kallen, *Creative Intelligence: Essays in the Pragmatic Attitude* (New York: Henry Holt, 1917), especially G. H. Mead, 'Scientific Method and the Individual Thinker', 176–227; B. Latour and S. Woolgar, *Laboratory Life* (Beverly Hills, Calif.: Sage, 1979); Latour and F. Bastide, 'Essaie de science-fabrication mise en évidence expérimentale du processus de construction de la réalité par l'application de méthodes socio-sémiotiques aux textes scientifiques', *Etudes Françaises*, Vol. 19 (Automne 1983), 111–33; Latour, 'Is it Possible to Reconstruct the Research Process? Sociology of a Brain Peptide', in K. D. Knorr, R. Krohn and R. D. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, Vol. 4 (Dordrecht and Boston, Mass.: Reidel, 1980), 53–73; K. Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon Press, 1981); H. M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205–24; H. Garfinkel, M. Lynch and E. Livingston, 'The Work of Discovering Science Construed with Materials from the Optically Discovered Pulsar', *Philosophy of Social Sciences*, Vol. 11 (1981), 131–58; Lynch, 'Turning Up Signs in Neurobehavioral Diagnosis', paper delivered to the American Sociological Association, San Francisco, September 1982; Lynch, *Art and Artefact in Laboratory Science* (London: Routledge and Kegan Paul, 1985); and R. Williams and J. Law, 'Beyond the Bounds of Credibility', *Fundamenta Scientiae*, Vol. 1 (1980), 295–315.

4. With exceptions, and this is of course not meant to be an exhaustive list: E. M. Gerson, 'Styles of Scientific Work and the Population Realignment in Biology, 1880–1925', paper presented to the conference on history and philosophy of biology, Granville, Ohio, July 1983; M. Zenzen and S. Restivo, 'The Mysterious Morphology of Immiscible Liquids: A Study of Scientific Practice', *Social Science Information*, Vol. 21 (1982), 447–73; S. L. Star, 'Tactics in the Debate about Cerebral Localization', paper presented to the Society for Social Studies of Science, Philadelphia, October 1982; Star, *Scientific Theories as Going Concerns: The Development of the Localizationist Perspective in Neurophysiology, 1870–1906* (unpublished PhD dissertation, University of California, San Francisco, 1983), Chapter 2; R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge* (Keele, Staffs.: *Sociological Review* Monograph No. 27, 1979); B. Latour, *Guerre et paix suivi de irreductions* (Paris: A. M. Metailie, 1984); B. Barnes and S. Shapin (eds), *Natural Order: Historical Studies of Scientific Culture* (London: Sage, 1979); see also note 10.

5. For example, see M. Mulkay and G. N. Gilbert, 'Accounting for Error: How Scientists Construct their Social Worlds when They Account for Correct and Incorrect Belief', *Sociology*, Vol. 16 (1981), 165–83.

6. See, for example, S. Woolgar, 'Discovery, Logic and Sequence in a Scientific Text', in Knorr et al. (eds), op. cit. note 3,239–68; T. J. Pinch, 'The Sun-Set: The Presentation of Certainty in Scientific Life', *Social Studies of Science*, Vol. 11 (1981), 131–58. This latter work is especially interesting in discussing the combination of 'applied' and 'theoretical' work.

7. E. M. Gerson, 'Passion and Moral Dilemma in Scientific Work', in preparation.

8. But it does seem that this uncertainty is inescapable in basic research. As one referee pointed out, the very taxonomy of uncertainty presented here is itself an example of taxonomic (if not diagnostic, technical and political . . .) uncertainty; I too have to ascertain 'what type of thing is this' from an array of phenomena. I have tried to resolve the uncertainties here, however, not by converting them into unempirically testable ideal types or jettisoning individual differences, but rather by delineating varieties and conditions. William Wimsatt and James Griesmer have already suggested an extension to this work, which is mapping the interactions between types of uncertainty (for example, between diagnostic and political) under varying conditions. (Private communication, July 1984.)

9. But see A. E. Clarke, 'Materials, Constraints, Opportunities and Resources in Reproductive Biology: 1880–1940', paper presented to the American Association for the History of Medicine, San Francisco, 1984; and also Star, op. cit. note 4.

For general discussions of uncertainty in medical sociology, see, for example, D. Light, 'Uncertainty and Control in Professional Training', *Journal of Health and Social Behavior*, Vol. 6 (1979), 141–51; R. C. Fox, 'Training for Uncertainty', in R. K. Merton, G. Reader and P. Kendall (eds), *The Student-Physician* (Cambridge, Mass.: Harvard University Press, 1957), 207–41. Little attention has been paid to *varieties* of uncertainty, or work-based strategies for its management, although organizational theorists have begun the analysis (for example, S. Fiddle [ed.], *Uncertainty: Behavioral and Social Dimensions* [New York: Praeger, 1980]) and several psychologists have worked on decision-making under uncertainty (for example, D. Kahneman, P. Slovic and A. Tversky [eds] *Judgement Under Uncertainty: Heuristics and Biases* [Cambridge: Cambridge University Press, 1982]; R. D. Tweney, M. E.

Doherty and C. R. Mynatt [eds], *On Scientific Thinking* [New York: Columbia University Press, 1981]).

10. It is also perhaps worth noting that the gaps between history and philosophy of medicine, history of physiology, and historical and medical sociology are also subject to many of the same processes cited in this paper, through alliances, audience changes, and reform movements.

In the analysis of data for this paper, I used *grounded theory*, and a qualitative analytic approach for this analysis. Briefly, this generates codes from observations and uses comparative data. The details of the method have been described elsewhere: see B. G. Glaser and A. L. Strauss, *The Discovery of Grounded Theory* (Chicago: Aldine, 1967); Glaser, *Theoretical Sensitivity: Further Advances in the Methodology of Grounded Theory* (Mill Valley, Calif.: The Sociology Press, 1978); Strauss, *Qualitative Analysis* (in preparation). I especially relied on Strauss's exposition of 'Discovering New Theory from Previous Theory', in T. Shibutani (ed.), *Human Nature and Collective Behavior* (New Brunswick, NJ: Transaction Books, 1973) 46–53.

My analytic perspective is symbolic interactionist and pragmatist: see, especially, H. Blumer, *Symbolic Interactionism: Perspective and Method* (Englewood Cliffs, NJ: Prentice-Hall, 1967); J. Dewey, *The Quest for Certainty: A Study of the Relation of Knowledge and Action* (New York: Minton, Balch, 1929). Like the analyses of Becker, Hughes, Strauss, Gerson and Volberg, it is focused on work. See H. S. Becker, *Sociological Work* (Chicago: Aldine, 1970) and *Art Worlds* (Berkeley, Calif.: University of California Press, 1982); E. C. Hughes, *The Sociological Eye* (Chicago: Aldine, 1971); A. L. Strauss, *Mirrors and Masks: The Search for Identity* (Glencoe, Ill.: Free Press, 1959) and *Negotiations: Varieties, Contexts, Processes, and Social Order* (San Francisco, Calif.: Jossey Bass, 1978); Strauss, L. Schatzman, R. Bucher, D. Ehrlich and M. Sabshin, *Psychiatric Ideologies and Institutions* (New York: Free Press of Glencoe, 1964) and Strauss, S. Fagerhaugh, B. Suczek and C. Wiener, *The Social Organization of Medical Work* (Chicago: The University of Chicago Press, forthcoming); E. M. Gerson, 'Work and Going Concerns: Some Implications of Hughes' Work', paper presented to the Pacific Sociological Association, San Jose, Calif., April 1983, 'On "Quality of Life"', *American Sociological Review*, Vol. 41 (1976), 793–806, and *Scientific Work Organization: The Population Realignment in Biology, 1880–1925*, in preparation; and R. Volberg, *Constraints and Commitments in the Development of American Botany, 1880–1920* (unpublished PhD dissertation, University of California, San Francisco, 1983).

Science is one kind of work. This means that my units of analysis are tasks and activities, not individuals or their allegiance to theories. These tasks and activities are politically situated and must be understood with reference to large- and small-scale institutional contexts.

An additional set of analytic concerns which informed the study can be found in the *social worlds* approach: see, especially, A. L. Strauss, 'A Social World Perspective', *Studies in Symbolic Interaction*, Vol. 1 (1978), 119–28. 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: see G. Simmel, *Conflict and the Web of Group-Affiliations* (New York: Free Press, 1955); R. E. Park, *Human Communities: The City and Human Ecology* (Glencoe, Ill.: Free Press, 1952). Shibutani expanded this work: see T. Shibutani, 'Reference Groups as Perspectives', *American Journal of Sociology*, Vol. 60 (1955), 562–69; 'Reference Groups and Social Control', in A. M.

Rose (ed.), *Human Behavior and Social Processes* (Boston, Mass.: Houghton Mifflin, 1962), 128–47.

Several authors have elaborated these ideas to describe the shared work of reference groups. These groups continually change: they come together (intersect) and splinter apart (segment) as separate kinds of work. They argue, and occasionally form large-scale, stable debating structures, or *arenas*. See Strauss (1959), op. cit.; R. Bucher and A. Strauss, 'Professions in Process', *American Journal of Sociology*, Vol. 66 (1961), 325–34; Bucher, 'Pathology: A Study of Social Movements in a Profession', *Social Problems*, Vol. 10 (1972), 40–51; Becker (1982), op. cit.; E. M. Gerson, 'Scientific Work and Social Worlds', *Knowledge*, Vol. 4 (1983), 357–77, and op. cit.; R. Kling and Gerson, 'The Social Dynamics of Technical Innovation in the Computing World', *Symbolic Interaction*, Vol. 1 (1977), 132–46, and 'Patterns of Segmentation and Intersection in the Computing World', ibid., Vol. 1 (1978), 24–43.

11. A good analysis of taxonomic uncertainty in botany is provided by R. Volberg, 'The Use and Abuse of the Species Concept in Biology', paper presented to the conference on history and philosophy of biology, Granville, Ohio, July 1983. See also D. L. Hull, 'Are Species Really Individuals?', *Systematic Zoology*, Vol. 25 (1976), 174–91, and Hull, 'Exemplars and Scientific Change', *PSA 1982*, Vol. 2 (1982), 479–503.

For purposes of consistency, I primarily use the past tense in describing the situations faced by these investigators. I do not mean to imply that these problems no longer exist in neurology or neurosurgery; many remain unresolved: see, for instance, J. Franklin and A. Doelp, *Not Quite a Miracle: Brain Surgeons and their Patients on the Frontier of Medicine* (New York: Doubleday, 1983).

12. See Starr (1982 and 1983), op. cit. note 4.

13. See, for example, K. Danziger, 'Mid-Nineteenth-Century British Psycho-Physiology: A Neglected Chapter in the History of Psychology', in W. R. Woodward and M. G. Ash (eds), *The Problematic Science: Psychology in Nineteenth-Century Thought* (New York: Praeger, 1982), 119–46; R. Richards, 'Darwin and the Biologizing of Moral Behavior', ibid., 88–115; L. Daston, 'British Responses to Psycho-Physiology, 1860–1900', *Isis*, Vol. 69 (1978), 192–208. I am grateful to Elihu Gerson for clarifying some of these issues for me.

14. Star (1982), op. cit. note 4, and (1983), op. cit. note 4, Chapters 2, 3 and 4.

15. Elsewhere I discuss the consequences of triangulating this evidence after it had been acquired from the various domains: Star, op. cit. note 4, Chapter 3; S. L. Star, 'Triangulating Clinical and Basic Research: British Localizationists, 1870–1906', paper presented to the American Association for the History of Medicine, San Francisco, May 1984.

16. Now the National Hospital for Nervous Diseases, London. While localizationist work was taking place in other countries and sites (especially Germany, Italy and the United States), it was developed most clearly and thoroughly, and propagated most widely, by this group of English researchers.

17. Although primitive forms of surgery on the skull and outer parts of the brain had been attempted since earliest times, modern intracranial neurosurgery began in 1884: see A. Bennett and R. Godlee, 'Report of Tumour Removal', *The Lancet* (10 December 1884), 1090–91; C. Ballance, 'A Glimpse into the History of the Surgery of the Brain', *The Lancet*, Vol. 202 (28 January 1922), 165–72.

18. Star (1983), op. cit. note 4, Introduction and Chapter 2; B. Abel-Smith, *The Hospitals, 1800–1948* (London: Heinemann, 1964); J. D. Spillane, *The Doctrine of*

the Nerves: Chapters in the History of Neurology (Oxford: Oxford University, 1981).

British terminology and distinctions between types of medical jobs can be confusing. For this paper, I use the following terms: 'doctors' is generic, referring to all 'medical persons', including surgeons, house and consulting physicians; 'surgeons' were doctors who performed operations; 'physicians' diagnosed and consulted. 'Neurologists' were physicians. The lines of work I describe here are not necessarily strictly divided by personnel. That is, physiological experiments were done by both surgeons and physicians; pathological work by both, and so on. For the sake of simplicity, I refer to 'physiologists' and 'pathologists' as separate. Since the analysis is based on tasks, not persons, this should present no analytic problems (see Gerson, op. cit. note 10).

19. H. Cushing, 'The Special Field of Neurological Surgery: Five Years Later', *Bulletin of the Johns Hopkins Hospital*, Vol. 21 (1910), 325–39, at 333; O. Temkin, *The Falling Sickness: A History of Epilepsy from the Greeks to the Beginnings of Modern Neurology* (Baltimore, Md: Johns Hopkins University Press, 1945), 274. Paget, discussing a speech of Queen Square surgeon 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'.

S. Paget, *Sir Victor Horsley: A Study of His Life and Work* (London: Constabel, 1919), 122–23.

20. Temkin, op. cit. note 19; A. Waller, N. Buboff and R. Heidenhain, 'On Phenomena of Excitation and Inhibition in the Cerebral Motor Centres', *Brain*, Vol. 5 (1882), 138–40; G. Rosen, *Madness in Society* (New York: Harper and Row, 1968), Chapter 9.

21. Gowers notes that '. . . 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': W. R. Gowers, *Lectures on the Diagnosis of Diseases of the Brain* (London: Churchill, 1885), 170.

22. For example, J. Burdon-Sanderson, 'Note on the Excitation of the Surface of the Cerebral Hemispheres by Induced Currents', *Proceedings of the Royal Society of London*, Vol. 22 (1873–74), 368–70; W. J. Dodds, 'On the Localisation of the Functions of the Brain: Being an Historical and Critical Analysis of the Question', *Journal of Anatomy and Physiology*, Vol. 12 (1877–78), 340–63, 454–93; A. Rabagliati, 'Review of the Work on Localization of Luciani and Tamburini', *Brain*, Vol. 1 (1879), 529–44. Similar debates have continued to inform electrical work on the brain to the present day, especially in EEG research.

23. From 1886 to 1891, Horsley was the only neurosurgeon at Queen Square: G. Jefferson, 'Sir Victor Horsley', in *Selected Papers* (Springfield, Ill.: Charles Thomas, 1960), 130–69. Physicians only saw patients one or two times a week at Queen Square, and patients could have many fits per day. Thus this division of labour was significant.

24. Horsley, for example, in order to obtain bodies for postmortems, 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: J. B. Lyons, *Citizen Surgeon* (London: Dawnay, 1966). There was no other way to get a steady supply of bodies. Yeo, a physician at Queen Square, remarks

that the postmortem examination of the brain of a tumour patient 'had to be performed hastily, and under difficulties which precluded the possibility of a detailed examination of that organ being made': J. B. Yeo, 'A Case of Large Tumour of the Left Cerebral Hemisphere, with Remarkable Remissions in the Symptoms', *Brain*, Vol. 1 (1878), 273–76, quote at 275. In this same case report, Yeo notes 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 clinical and basic uncertainties overlap. 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 postmortem. Even where permission was received for autopsy, there were difficulties. Atkins 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.

R. Atkins, 'A Case of Right Hemiplegia, Hemianaesthesia, and Aphasia, Having for its Prominent Anatomical Lesion Softening of the Left Lateral Lobe of the Cerebellum', *Brain*, Vol. 1 (1878), 410–17. Bennett, in the same year, notes great difficulties in getting the brain of a dead patient who had had a cerebral tumour, since it had been accidentally thrown away: A. H. Bennett, 'Case of Cerebral Tumour — Symptoms Simulating Hysteria', *Brain*, Vol. 1 (1878), 114–20. Ferrier describes similar difficulties: D. Ferrier, 'Pain in the Head in Connection with Cerebral Disease', *Brain*, Vol. 1 (1879), 467–83.

25. The autopsy report on the brain of a dog observed by Ferrier's partner, Yeo, 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 to 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.

E. Klein, J. N. Langley and E. A. Schafer, 'On the Cortical Areas Removed from the Brain of a Dog, and from the Brain of a Monkey', *The Journal of Physiology*, Vol. 4 (1883–84), 231–326 (quote at p. 250).

26. J. H. Jackson, 'On Epilepsies and on the After Effects of Epileptic Discharges', *West Riding Lunatic Asylum Medical Reports*, Vol. 6 (1876), 266–309; Jackson, 'On Affections of Speech from Disease of the Brain', *Brain*, Vol. 2 (1879), 323–56; Temkin, op. cit. note 19.

27. Horsley's casebook (1904) contains the following directions to patients: 'State

very clearly on which side the movement commences, and whether it is confined to that side or spreads to the opposite side': V. Horsley, Casebook, Archives of the Thane Library, University College, London, MS/UNOF 12/1–2, 1904. But the forms did not assure certainty. I saw many which had been partially or vaguely filled out. Patients often left blanks and used phrases such as: 'Yes, no,? . . .', 'Feels lethargic', 'Twitches all over', 'I don't know', or 'felt poorly': National Hospital for the Paralysed and Epileptic, Queen Square, London, unpublished Hospital case records; Archives of National Hospital for Nervous Diseases, London, 1860–1910. I am grateful to William Wimsatt for pointing out several uses of such forms.

28. D. Ferrier, unpublished laboratory notebooks, Library of the Royal College of Physicians, London, MS 246/1–19, 1873–83.

29. See, for example, E. Jones, Class notes from 1899 class with Risien Russell (of Queen Square) at University College Hospital, MS/UNOF/14/1–2, Thane Library, University College Hospital, London, 1899; and Jones, *Free Associations* (London: Hogarth, 1959).

30. See, for example, Gowers, op. cit. note 21; B. Bramwell, *Intracranial Tumours* (Edinburgh: Pentland, 1888); Paget, op. cit. note 19, 180–81.

31. Buzzard, for example, notes that one patient with a huge tumour appeared unaffected until he died: '*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': T. Buzzard, 'Pain in the Occiput and Back of Neck', *Brain*, Vol. 4 (1881), 130–32, quote at 132.

32. As this letter from neurologist Gowers to surgeon Horsley attests:

> . . . possible syphilis and urgency to act *as soon* as *ever the absence* of result from treatment is *just* 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?

J. B. Lyons, 'Correspondence between Sir William Gowers and Sir Victor Horsley', *Medical History*, Vol. 9 (1965), 260–67, quote at 263–64.

33. H. Cushing, 'The Special Field of Neurological Surgery', *Bulletin of the Johns Hopkins Hospital*, Vol. 16 (1905), 77–87; V. Horsley, 'On the Technique of Operations on the Central Nervous System', *British Medical Journal*, Vol. 3 (25 August 1906), 411–23; A. deWatteville, 'Review of *Maladies de la Moelle* by A. Vulpian (Paris: Octave Doin, 1881)', *Brain*, Vol. 3 (1881), 516–28; Cushing, op. cit. note 19.

34. For a general discussion of this, see J. G. Hougland and J. M. Shepard, 'Organizational and Individual Responses to Environmental Uncertainty', in Fiddle, op. cit. note 9, 102–19.

35. R. A. Hunter and L. J. Hurwitz, 'The Case Notes of the National Hospital for the Paralyzed and Epileptic, Queen Square, London, before 1900', *Journal of Neurology, Neurosurgery and Psychiatry*, Vol. 24 (1961), 187–94.

36. G. Holmes, *The National Hospital, Queen Square* (Edinburgh: Livingstone, 1954).

37. L. S. King, *Medical Thinking: A Historical Preface* (Princeton, NJ: Princeton University Press, 1982).

38. Spillane, op. cit. note 18, 354–58. In the same vein, Gowers wrote to Horsley:

> 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.

Lyons, op. cit. note 32, 263, (no date given; 1894?, spelling as in original). 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, op. cit. note 18, 412. Spillane also describes Gowers' reaction to the problem:

> Optic neuritis was *the* 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. (ibid., 360)

The need for brain surgery was not immediately apparent to the medical profession. Surgeons and localizationist neurologists had to convince them that brain surgery was *possible*, and, once possible, prudent. But there were no effective alternatives for tumour patients. Neurologists had not developed effective treatments for such diseases: H. Cushing, 'Realignments in Greater Medicine: Their Effect on Surgery and the Influence of Surgery upon Them', *British Medical Journal*, Vol. 2 (9 August 1913), 290–97. As Horsley notes:

> As in all special branches of medicine and surgery which are in a process of evaluation, 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 connexion with many diseases . . . (op. cit. note 33, 411)

And Cushing asks:

> For what eager student of medicine can face without dismay the 'poverty of therapy' that characterizes 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? (op. cit. note 33, 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: Spillane, op. cit. note 18, 398; see also Cushing, op. cit. note 19, and his 'Psychiatrists, Neurologists and the Neurosurgeon', *Yale Journal of Biology and Medicine*, Vol. 7 (1935), 191–207, for a review of these histories.

39. B. B. Rawlings, *A Hospital in the Making* (London: Pitman, 1913), 189; A. R. Urquhart, 'Cases of Cerebral Excitement Treated by Mustard Baths', *Brain*, Vol. 1

(1878), 126–27; A. H. Bennett, 'Metalloscopy and Metallotherapy', *Brain*, Vol. 1 (1878b), 331–39; National Hospital Records, op. cit. note 27, Buzzard casebook, 1901.

40. One casebook of Ferrier's from 1894 records that a patient with a cerebral tumour 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, op. cit. note 27). Here there was an interaction with diagnostic uncertainty.

41. This 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. A division between diagnostic methods and lines of work and those employing manual/anatomical skills was institutionalized in organizational practice.

The inequities were reflected in the recordkeeping at Queen Square. I had some trouble locating surgeon 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 physician in cases requiring brain or nervous system operations. The physicians' names 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.

42. Letters between physicians Gowers and Horsley also reflect this situation. One patient was getting much worse because Gowers could not locate Horsley to consult him about operating. The letters also complain of the negative attitude of much of the medical patient community towards brain surgery: Lyons, op. cit. note 32.

43. Cushing, op. cit. note 33, 78.

44. Rawlings, op. cit. note 39; Holmes, op. cit. note 36.

45. R. D. French, *Antivivisection and Medical Science in Victorian Society* (Princeton, NJ: Princeton University Press, 1975); G. Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society* (Princeton, NJ: Princeton University Press, 1978). Michael Foster vividly describes the antivivisection 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 *penal act*. 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.

W. MacCormac (ed.), *Transactions of the International Medical Congress*, 7th Session, 4 Vols. (London: Kolckmann, 1881), 218.

46. Lyons, op. cit. note 32.

47. Geison, op. cit. note 45, 27–28. Crichton-Browne, a founding editor of the localizationist journal *Brain*, wrote a letter to the *Times*:

> Sire, — 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 bereavment 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, op. cit. note 18, 398.

48. See E. Sharpey-Schafer, *History of the Physiological Society During its First Fifty Years, 1876–1926* (London: Cambridge University Press, 1927).

49. Ferrier notebooks, op. cit. note 28.

50. Ferrier notebooks, op. cit. note 28, MS 246/5, 5 January 1875.

51. Ibid.

52. French, op. cit. note 45.

53. Rabagliati, op. cit. note 22, 537–38. 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. (ibid., 532)

See also A. Rabagliati, 'Review of "Clinical Researches on the Motor Centres of the Limbs", *Journal de Therapeutique*, 1877 (no author or pages given)', *Brain*, Vol. 1, (1878), 138–39. Yet one repeatedly 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.

J. Crichton-Browne, 'On the Weight of the Brain and its Component Parts in the Insane', *Brain*, Vol. 1 (1879), 504–18, and Vol. 2 (1880), 42–67, quote at 65.

54. H. Duret, 'On the Role of the Dura Mater and its Nerves in Cerebral Traumatism', *Brain*, Vol. 1 (1878), 29–47; Dodds, op. cit. note 22; Burdon-Sanderson, op. cit. note 22.

55. Ferrier notebooks, op. cit. note 28, MS 246/2, 8 August.

56. Ibid., MS 246/4, December 1874.

57. Ibid., MS 246/7, October 1879.

58. C. E. Beevor and V. Horsley, 'Electrical Excitation of the So-called Motor Cortex and Internal Capsule in an Orang-utang', *Philosophical Transactions of the Royal Society*, Vol. 181 (1890), 129–58; Beevor and Horsley, 'A Further Minute

Analysis by Electric Stimulation of the So-called Motor Region (Facial Area) of the Cortex Cerebri in the Monkey (Macacus Sinicus)', *Philosophical Transactions of the Royal Society*, Vol. 185 (1890), 439–81.

59. Compare, for example, the statements made by D. Ferrier, in *The Functions of the Brain* (London: Smith, Elder, 1876), on pages 38 and 148; in one place he notes the inseparability of parts of the brain, and thus the insignificance of the anomaly (38); in another, however, he calls into question the significance of his findings if this is so (148). Yet the whole book is an argument for localizationism.

60. H. Head, *Aphasia and Kindred Disorders of Speech* (New York: Macmillan, 1926).

61. D. Ferrier, *The Localisation of Cerebral Disease* (London: Smith Elder, 1878), 131.

62. Rawlings, op. cit. note 39, 121.

63. S. L. Star, 'Simplification in Scientific Work: An Example from Neuroscience Research', *Social Studies of Science*, Vol. 13 (1983), 205–28.

64. Volberg, op. cit. note 18.

65. A. W. Campbell, *Histological Studies on the Localisation of Cerebral Function* (Cambridge: Cambridge University Press, 1905), 222.

66. This general phenomenon is discussed in more detail in Star, op. cit. note 4, and a fuller discussion of the management of anomalies can be found in S. L. Star and E. M. Gerson, 'Management of Anomalies in Scientific Research: I. Varieties of Anomaly', and 'II. Properties of Artifacts', submitted for publication, 1984.

67. R. Bucher, R. and J. Stelling, 'Vocabularies of Realism in Professional Socialization', *Social Science and Medicine*, Vol. 7 (1973), 661–75. See also E. Freidson, *Professional Dominance: The Social Structure of Medical Care* (New York: Atherton, 1970), and C. Bosk, *Forgive and Remember: Managing Medical Failure* (Chicago: The University of Chicago Press, 1979).

68. C. Jones, 'Some Founders of British Neurology', *New Zealand Medical Journal*, Vol. 2 (1946), 143–54, quote at 153.

69. Spillane, op. cit. note 18; Jones (1899), op. cit. note 29.

70. The work in progress of H. S. Becker and associates on representations has begun to analyze such packaging in a number of fields, including statistics and the production of computer graphics: unpublished memoranda, Northwestern University, 1983–84. Other important work in progress points in similar directions. See, for example, B. Latour, 'Visualisation and Cognition', in H. Kuklick (ed.), *Sociology of Art, Knowledge, and Science* (in press); and John Law's recent work on texts also looks very promising in light of these questions (private communication, July 1984).

71. Referees Report of the Royal Society, 12 May 1874.

72. Von Bergmann, in 1889, 'was able to collect seven cases only in which a brain tumour had been diagnosed by neurological evidence, localized, and removed by operation': G. Horrax, *Neurosurgery: An Historical Sketch* (Springfield, Ill.: Charles Thomas, 1952), 56.

73. Ferrier casebook, 1886, National Hospital Records; see also Beevor casebook, 1894, for a similar case where no tumour was removed but a large amount of bone was removed in the search for it.

74. See, for example, Ferrier, op. cit. note 59.

75. See, for example, C. E. Brown-Séquard, 'Nombreux cas de vivisection pratiquée sur le cerveau de l'homme le verdict contre la doctrine des centres psycho-

moteurs', *Archives de Physiologie Normale et Pathologique*, 5ième série, Vol. 2 (1890), 762–73.

76. See, for example, S. L. Star, 'Preliminary Field Report on the Study of a Robotics Laboratory', paper presented at the Tremont Science Studies Seminar Series, December 1983; Gerson, op. cit. note 4.

77. H. Simon, *The Sciences of the Artificial* (Cambridge, Mass.: MIT Press, 1981).

78. Volberg, op. cit. note 11. See also Hull, op. cit. note 11.

79. E. M. Gerson and S. L. Star, 'The Sociology of Engineering Work', in preparation.

80. G. R. Barber, *Office Semantics* (unpublished PhD dissertation, Massachusetts Institute of Technology, 1982), and Shepard, op. cit. note 34, provide a good preliminary typology of these uncertainties.

81. See Clarke, op. cit. note 9.

82. Including Dewey, op. cit. note 3 and note 10; Kuhn, op. cit. note 2; H. Collins and T. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge and Kegan Paul, 1982), and J. Carrier, 'Misrecognition and Knowledge', *Inquiry*, Vol. 22 (1979), 321–42. Carrier's concept of 'masking' is particularly interesting in light of discussions of deletion of uncertainty.

83. This paper is part of a larger research programme analyzing the strategies, tasks, and contexts of science as part of the sociology of work. Other aspects of scientific work and change have been described in work on: simplification processes in science (Star, op. cit. note 63); science and social worlds (Gerson, op. cit. note 14); anomalies and scientific work organization (Star and Gerson, op. cit. note 10); problem substitutions in botanical work (Volberg, op. cit. note 10); recalcitrant materials in reproductive biology (Clark, op. cit. note 9); and institutional and stylistic realignments in population biology (Gerson, op. cit. note 10).

The work of Wimsatt and Griesmer in the philosophy of science has called for an understanding of science based on research strategies and work contexts; and the work of Hewitt, Scacchi and Gasser in computer science addresses these issues. See W. C. Wimsatt, 'Reductionist Research Strategies and their Biases in the Units of Selection Controversy', in T. Nickles (ed.), *Scientific Discovery: Case Studies* (Dordrecht and Boston, Mass.: D. Reidel, 1980), 213–59, and 'Robustness, Reliability and Overdetermination', in M. B. Brewer and B. E. Collins (eds), *Scientific Inquiry and the Social Sciences* (San Francisco, Calif.: Jossey-Bass, 1981), 124–62; J. Griesmer, *Communication and Scientific Change in the Macroevolution Controversy* (unpublished PhD dissertation, University of Chicago, 1983); C. Hewitt and P. de Jong, 'Open Systems', in M. L. Brodie et al. (eds), *On Conceptual Modelling* (New York: Springer-Verlag, 1984), 147–64, and Hewitt, 'The Challenge of Open Systems', *BYTE*, Vol. 10 (April 1985), 223–42; W. Scacchi, 'Managing Software Engineering Projects: A Social Analysis', *IEEE Transactions on Software Engineering*, Vol. SE-10 (1984), 49–59; L. Gasser, *The Social Dynamics of Routine Computer Use in Complex Organizations* (unpublished PhD dissertation, Department of Information and Computer Science, University of California, Irvine, 1983).

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