The Emergent Materialism in French Clinical Brain Research (1820-1850): A Case Study in Historical Neurophilosophy

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1. Introduction

In the period running roughly from 1810 to 1860, French brain research remained split into two large provinces, each of which provided its own epistemological principles, methodological rules, and theoretical aims for the study of man's mind. The controversies resulting from this split concerned issues as diverse as the intelligibility of mental processes, the unitary or the modular structure of cerebral activities, the relations holding between organic matter and mental function, the relevance and evidential weight of clinical and of experimental—i.e., physiological—data for the explanation of the brain's workings, and other similarly troublesome conceptual, ontological, and metaphysical riddles pertaining to something that had not yet been baptized 'the mind-body problem'.

The fact that physicians, physiologists, and philosophers would not agree upon the true, or even upon the most convenient, theory of man's mind and brain is trivially unsurprising. For the mind-body problem, which, in one version or another, had already caused a lot of ink to be spilt before 1800, was handed down unsettled to nineteenth-century theorists, who would, in turn, bequeath it—as unsettled as before—to later generations of physiologists, neuroscientists, and neurophilosophers. However, the manner in which this problem came to be approached in, say, 1825 significantly differed from the way it had been thematized before the onset of the French Revolution.

This essay presents a reconstruction of the mind-body problem from the perspective of French clinical brain research in the above-men-
tioned lapse of time. Since neither the clinicians' practices, the mode (or style) of reasoning nor, finally, the standard neurological concepts of the time can be assumed to be readily familiar to today's readers, I shall proceed somewhat like an archaeologist who tries to identify various layers within the chunks of published words: the descriptive layer, the nosological layer, the anatomical layer, the theoretical layer, and so on.

I shall begin with an outline of some doctrines that lie in the historical background of French clinical brain research in the first third of the nineteenth century. I shall then describe in detail the work of clinical brain researchers, i.e., the medical acts that were performed with (and on) patients suffering from brain lesions of all sorts. Finally, I shall analyze the notion of the localization of cerebral (and of cerebral mental) functions as the expression of a new materialist approach to the mind.

The thesis underlying this paper may be spelled out and illustrated by reference to the notion of subalternatio scientiarum—a notion which, in the context of the Copernican revolution, played an undeniably crucial role in terms both of rhetorical strategies and ontological claims.

On Giovanni Maria Tolosani's view, the Copernican theory violated a strictly binding rule according to which an inferior science needs the assistance of a superior science—which is to say that the new astronomy not only contradicted traditional cosmology, but also offered misleading opinions concerning the architecture of the universe because it rejected the leadership of the superior science, i.e., theology. Likewise, the historical process that my article tries to articulate involves, though without manifest interference on the part of a powerful, intellectually influential institution like the Roman Curia, the issue of which science (or epistemic realm) is subordinate to another science (or epistemic realm). My thesis is (1) that, until approximately 1800, broad metaphysical beliefs were guiding the interpretation of psychological, physiological, and medical observations relating to the human mind and its organic embodiment, and (2) that, from approximately 1800 on, psychological, physiological, and medical observations have guided the now subordinate philosophy of mind. To put it differently, the prior subalternatio scientiarum (metaphysics vis-à-vis physics) came to be reversed, thus giving epistemic priority to physics over metaphysics.

2. The Historical Background of Early Nineteenth-Century Brain Research in France

Albrecht von Haller's (1706-1777) experimental discovery of irritability as a specific power inherent in some, but not all parts of complex live-
ing bodies (such as parrots, sheep, donkeys, mice, dogs, cats, humans, etc.) had a lasting impact upon the community of eighteenth-century scientists. For Haller's experiments showed that the principles of life operating in such bodies did not hold homogeneously for the whole organism (i.e., the body in the entirety of its spatial extension), since the activity of some parts of the organism was determined by the power of irritability (*vis insita*), whereas that of other parts was ruled by the power of sensitivity (*vis nervosa*). Thus, nerves, which obviously look, and are, anatomically different from most other parts of an organism, do not contract when stimulated, whereas muscles, which evidently form other parts of the same organism, do contract under stimulation. This stimulation may be nervous or, for instance, mechanical, as could be shown by experiments performed upon single muscles detached from an organism shortly after its death. Such experiments were crucial, for they demonstrated (in the epistemological as well as in the perceptual sense) that the *vis insita*, to which the capacity of causing contractions was ascribed, did not depend, as Haller asserted, upon the weight, the attraction, or the elasticity of matter; therefore, it seemed to constitute a power of its own.

In contradistinction to the irritable parts, the parts defined by sensitivity looked as if they were designed for transmitting impulses of physical excitation from the periphery to the brain or from the brain to the periphery of an organism. But these sensitive parts, it was asserted, did not add their inner movement—that is, the movement of the nervous fluid—to the movement of irritable parts. To put it differently, they did not move, but were instead modified in their state of activity by other parts of the organism or by external forces such as light, temperature, attraction, etc.⁴

Haller's taxonomy of organic tissues was supported, and followed, by an entire series of physiological discoveries and led to a reorientation in the biological sciences. Indeed, purely mechanical explanations of the Cartesian type or hydraulic theories of the type developed by Hermannus Boerhaave (1668-1738) could no longer account for either the workings of individual bundles or compounds of tissue (muscles, glands, nerves, etc.), for these workings seemed to be rooted in distinct properties of organic matter. Moreover, traces of life were shown to last beyond the organism's death. Thus, the Italian physiologist Lazarro Spallanzani (1729-1799) observed that digestive processes would not be halted at once by an animal's abrupt death, and experimental results such as these were interpreted as interesting indications that some parts of the body possessed a kind of autonomous activity or a sort of spontaneity resulting from the interplay of natural (but possibly vital) forces present in organs such as the guts—an interplay, however,
which could not be explained within the frame of traditional physiological theories.

By 1800, most physiologists, physicians, and naturalists would to a large measure agree that, whatever the function embodied and instantiated by some organ, the activity of this bodily part resided, or had its proper place, in the portion of living matter by which it was subserved. Thus, afferent nerves were said to channel impressions that had been caused by some force on the receptor (the retina for vision, the skin for the sense of touch, etc.) and efferent nerves were said to transmit impulses stemming from the brain to the peripheral parts of the organism (muscles, glands, etc.); the function of the heart was recognized to be that of pumping blood, but not that of producing heat or that of causing salivation. There were, of course, dozens of problems and controversial questions as to how (exactly) the single parts of the organism fulfilled their functions and as to how (exactly) the various organs acted upon one another. However, it was generally taken for granted that each function's place corresponded to the anatomically distinct portion of space where the workings (activities) of living matter occurred.

Xavier Bichat thus wrote:

The proper life of each organ is composed of the different modifications that vital sensibility and mobility undergo in each, modifications which depend inevitably on circulation and the temperature of the organ. Each one in their several sensibility, mobility, temperature, and circulation has a particular mode of feeling, and of moving, a heat independent of that of the body, and a capillary circulation which, subtracted from the empire of the heart, only receives the influence of the tonic action of the part. 5

The only part of organisms, however, where the connection between spatially structured living matter and function appeared to be opaque, blurred, or otherwise fuzzy, and therefore truly problematic, was the brain. 6 On the one hand, data obtained in physiological experiments performed on the brain of living animals indicated that certain functions remained intact despite the fact that more or less extended portions of the cerebral or cerebellar tissue were damaged. On the other hand, data gathered in clinical observation indicated, on the contrary, that partial or total disruption of specific functions resulted from pathological alterations of the cerebral or cerebellar tissue in circumscribed areas only.

But these opposite pictures of the mind's embodiment are still too coarse to be either analytically exciting or philosophically challenging. What, then, was really at stake? Not so much the ensembles of propositions more or less faithfully reporting physiological and clinical findings relevant to the understanding of the functional organization of the
brain, but rather the consequences that these findings entailed for the notion of *Homo sapiens* as person endowed with intellectual and moral qualities. These consequences, to be sure, cannot be fully appreciated as long as the findings from which they have been drawn remain inarticulate. And a convenient way to articulate the findings—the raw material underlying the opposite pictures of the mind’s embodiment—is to reconstruct historically the manual and intellectual practices by means of which the data were collected, classified, interpreted, and used to support theoretical propositions.

The origin of the notion that single cerebral (and mental) functions are subject to more or less severe disturbances, or may even be totally destroyed—*because* more or less extended, but spatially circumscribed areas of the brain are *pathologically* impaired—has been connected by historians and philosophers of science as well as by historically minded neurologists and biologists with the advent of phrenology and with the clinical work done within the phrenological approach. The opposite notion that cerebral (and mental) functions survive without undergoing noticeable alterations, although more or less extended areas of the brain are destroyed, and that these functions suddenly break down altogether when physical devastation reaches a critical level, has been connected with the physiological experiments performed by Pierre Flourens on the central nervous system of animals. Crucial to both notions is, of course, the degree of convergence or of congruence between the unit(s) of organic matter and unitary function(s).8

3. Gall’s Approach and the Plurality of Organs within the Brain

The story of French clinical brain research in the epoch which interests us here thus begins quite conventionally with phrenology, a scientifically suspect but socially well-regarded doctrine initiated by Franz Joseph Gall (1739-1828). Gall correlated detailed anatomical descriptions of the brain with morphological features of the skull that cover the brain and with observations relating to the physical, hence measurable, nature of mental and moral faculties in man. The phrenologists believed that these correlations allowed one to recognize the relative weight of such faculties in each individual. In other words, a knowledgeable phrenologist was said to be capable of distinguishing a person with some propensity for mathematics from another person endowed with, let us say, a strong inclination for patriotism or for sociability by ‘decoding’ the physical signs that purportedly indicated the relative growth of such dispositions. But *which* physical properties, *i.e.*, properties open to public observations, indicated such mental and behavioral dispositions?
To be sure, the new organology (i.e., the Gallian approach to the brain as a system of interrelated individual organs), unlike physiology, was really not a bloody affair. One should bear in mind that Gall was a medical man whose aim was to cure, or at least to relieve his patients from their ills and pains. Hence, his attitude towards nature—a broad, malleable philosophical notion which was therefore easily applicable to a whole array of phenomena—differed in many respects from that of his contemporary physiological experimentalists. This explains why Gall and his close first-generation followers abhorred *in vivo* experiments and doubted that intrusive methods could yield unbiased data that would promote the understanding of the mechanisms of the brain in action. In consequence, they preferred to use smooth methods. Gall’s first approach was clearly morpho-analytical. Indeed, in his view, he had discovered that the bumps of the skull were the *tangible signs* of intellectual dispositions and moral faculties. And human beings just being what they are, that is, beings normally quite similar to one another (let us omit here the exciting theoretical and practical issue of monsters), their skulls most likely also reflected an overall family resemblance which could be iconically represented on maps or models showing the average distribution of the mental and moral faculties.

But mere cranioscopy, or rather, mere ‘skull-reading’ by touch (that is, grasping the physical, ‘bony’ peculiarities on the surface of the head that covers the brain and its adjacent parts and thereby determining which faculties are well developed, and which not) could not have been enough. In order to confer upon phrenology the scientific rigor that, according to Gall, was sadly missing in physiognomy, he had to take a far-reaching step from the psychologically relevant world outside of the head to the functionally equally relevant, but *basic*, flesh inside of the brain.

Gall turned out to be an astoundingly subtle and clever anatomist. Instead of cutting through the brain of dead organisms either from the top to the bottom, or from the forehead to the back, or in some other direction (as the anatomists of the time did), which makes the visual and mental representation of the three-dimensional intricacies characteristic of the central nervous system quite difficult, he would (schematically speaking) single out separate nerves and identify their paths within the brain’s tissue. This unusual technique required that the tissue be smoothly abraded rather than cut, and that the paths of the unscathed bundles be carefully observed and recorded.

For the sake of illustrating this point, let us do a bit of imaginary anatomy. Imagine a leg consisting just of blood vessels surrounded by only one sort of tissue, this homogeneous tissue being contained by the skin. You could cut this leg into slices and try to figure out, by mentally
linking each side of the slice to the opposite side of the next slice (except for the very first and the very last slice, of course), what the disposition or location of the vessels looks like. You could also start at the hip, find one or more vessels there, and then free them from the surrounding tissue. If you work downward in this manner, if your fingers move correctly, and if the instruments are used as delicately as they should be, you will finally have all vessels and interconnections between them laid bare before you for an unbiased longitudinal inspection.

This is, approximately, the way Gall the anatomist chose to proceed. However, the object he was studying was a little bit less structured and much more difficult to handle than the analogy with a chapter of fictional anatomy suggests. Notwithstanding the technical difficulties he was encountering in the physical analysis of the encephalon, Gall found out that a single center for all nerves did not exist, and that the organization of the whole nervous system, including the brain, resembled that of a tree—especially that of a grafted tree where parts of different origin coexist, communicate with one another, and still are subject to their own economy. More importantly, anatomical evidence had been gained supporting the thesis that mental dispositions and moral faculties were embodied in different organs. Gall’s view on what may be called the psycho-physiology of the human brain was summarized in the medical circles of the 1820s and 1830s in the following points: (1) the brain is the material organ of the mind; (2) there exists a more or less strict proportion between the volume of the organ and the power of its mental manifestations; (3) exercise of the mental faculties promotes the development of the cerebral organs; (4) the character of the human mind is determined by the configuration of the brain; and (5) the brain is a multiplex organ, composed of a definite number of compartments, each of which reveals itself to be the seat of a propensity, sentiment, or intellectual faculty.

The following situation obtained. From the outset, Gall took a firm stand with respect to the congruence of function and organ: each function had to be conceived as the manifestation of a recognizably distinct piece of living matter (a part of the body). In the course of the studies Gall undertook in his attempt to substantiate this genuinely functional view, an impressive array of fields were visited: comparative anatomy, zoology, anthropology, psychiatry, psychology, etc. The data found in the literature or obtained in straightforward observation or, finally, collected from minute anatomical analyses were either funneled into the construction of a morpho-cranio-logical theory of human characters, or used for the construction of a—complementary—theory of the organic fabric of mental dispositions and moral faculties. It turned out
that the two theories were parallel to one another, although the ear­lier—craniology—was dealing with the tangible outside of the head, whereas the later—the new organology—would refer to the depth and width of the material organization of cerebral (or rather, of cerebral cum mental) activities.

The fully-fledged version of phrenology of, say, 1812 (which ought to be carefully distinguished from the first, merely cranologically founded version of, say, 1805) rested, as has been briefly indicated, on nested correlations between the external features of human skull-morphology and the internal features of human brain-anatomy. For Gall, Spurzheim, and a number of their fellow phrenologists, these correlations were both strong and empirically evident. However, depending upon which lapse of time one considers, such correlations either did not exist (because they had not yet been spelled out), were judged to be ludicrously spurious, or were even discarded—so much so that the responses to Gall’s doctrine in progress varied greatly in character as well as in scope. To wit, the passages in the Phänomenologie des Geistes where Hegel dealt with Gall’s theory resulted from the philosopher’s having fed the machinery of dialectical mastication with only the latter’s “Schädellehre” (craniology without the complementary new organology). In contradistinction, Lamarck’s rather favorable remarks in the third part of the Philosophie zoologique of 1809 focus on the growth, the localization, and the material instantiation of the mental functions, but do not take the slightest notice of the craniological aspect of Gall’s doctrine.18

4. The Viability of Cerebral Localization and the Surplus of the Soul

The opposite approach—in terms both of method and of theory—is represented most outspokenly by Pierre Flourens (1794-1867), who, to be sure, never affirmed in any of his writings that the central nervous system worked like a more or less monotonically structured, functionally unitary piece of living tissue. On the contrary, to him the brain consisted of distinct masses that included the cerebral hemisphere, the cerebellum, and the medulla oblongata.19 In the essay “Physical Investigation into the Properties and the Functions of the Nervous System of Vertebrate Animals,”20 which had been read before, and examined by, the French Academy of Science in 1822, Flourens emphasized that “[t]he nervous system is not a homogeneous system; the cerebral lobes do not act in the same way as the cerebellum, nor the cerebellum like the spinal cord, nor the cord absolutely like the nerves.”21 However, the central nervous system as a whole was distinguished
clearly from other bodily systems such as the respiratory system or the digestive tract. As to the central nervous system, the following general propositions were said to hold: "it is a single system, all of its parts concur, consent, and are in accord; what distinguishes them is the appropriate and determined manner of acting: what unites them is a reciprocal action through their common energy."22

The normal behavior, i.e., the behavior unaffected by pathological conditions and displaying itself in the usual habitat of vertebrate animals studied by Flourens (reptiles, crabs, molluscs, and insects were not taken into consideration), is subserved by one system containing several subsystems (the nerves, the spinal cord, etc.) related to one another by functional constraints, but working in synergy. Hence, distinguishably different sets of functions are being subserved by specific neural subsystems—a notion Flourens couched in words that seem to amend the above quoted general propositions:

In the last analysis, the cerebral lobes, the cerebellum, the quadrigeminal bodies [a portion of the midbrain—A.M.j, the medulla oblongata, the spinal cord, the nerves, all these essentially different parts of the nervous system have specific properties, appropriate functions, and distinct actions; and despite this marvelous diversity of qualities, functions, and actions, they nevertheless constitute but a single system. One stimulated point in the nervous system stimulates all others, one weakened point weakens all; they have reaction, change, and energy in common. Unity is the outstanding principle which rules. It is everywhere, it dominates everything. The nervous system therefore forms but a single system.23

It should be noted that the difference between Flourens' assertion of 1822 that the nervous system consisted of an assemblage of four parts, and the second assertion, made two years later, that the nervous system consisted of six parts, is in no way essential. There were intrinsic as well as extrinsic reasons for the difference in number. An extrinsic reason is that the paper of 1822 was not as comprehensive as the monograph of 1824. An intrinsic reason is that Flourens' studies, which had led to the relatively short paper of 1822, had in the meantime progressed and yielded new results. But the essential issue concerns the manner in which Flourens came to split the nervous system into distinct functional parts or units. The act of allocating functions to parts of the nervous system reflects a specific experimental strategy and the mobilization of cognate discursive resources.

The question then is: what were the rules of thumb, the procedural imperatives, or the ordered set of manipulations to be performed, by which a sufficiently dense amount of evidence came to be collected so that the assertion concerning the allocation of specific functions to dis-
tinct parts of the nervous system appeared to be strong enough to Flourens and to his colleagues? A telling answer to the question may be obtained by the reconstruction of Flourens' dominating experimental script. As far as the brain itself was concerned, the script was one of intentional, precise, spatially circumscribed destruction of neural masses. It reads approximately as follows:

(i) Open the skull by removing parts of the cranial vault by way of a trepan.

(ii) Cut through the dura mater, the arachnoid membrane, and the pia mater, avoiding injuring or otherwise harming the blood vessels.

(iii) Cut out portions of the cortical matter of various size or cause lesions to that same portion by pricking or slicing it or by applying chemical substances, e.g., some drops of acid.

(iv) When the experimental animal has recovered from the operation, trigger—mechanically or otherwise—reactions by exciting the remaining parts of the brain and record these reactions.

Flourens reported the chain of manipulations he performed as well as some of the observations he made, for example, "[i]n a small rabbit, I removed both frontal bones; the animal lost little blood and it was as well after the operation as before it. . . . I pricked the cerebral hemispheres throughout their length without producing the least sign of muscular contraction anywhere." In other passages, our physiologist indicated that "[i]n a pigeon, I removed the cerebral hemispheres in successive layers; the animal remained stationary," or "I removed the whole left cranial vault in a young dog; I pricked and cut the cerebral lobe and the cerebellum of this side [sc., the left side]; the animal was neither disturbed nor agitated."

Several commentators have remarked that both the methods and the descriptions of the experiments were raw and superficial. It should be stressed, indeed, that the highly invasive procedures caused a state of traumatic (physiological) shock in the animal subjects, which impinged upon their behavior, and that the consequences of the experimental lesion were thus colored by the general condition of the organism constrained to undergo a massive modification of its neural processes. Hence, the experimental setting did not even vaguely imitate the natural setting—a point that did not even seem to bother Flourens. In addition, the (not at all untypical) passages quoted above show that the record of behavioral manifestations was also rather vague. In fact, Flourens did not analyze the behavioral modifications in his animal subjects with regard to the most important factors; rather, he registered merely the immediate and gross effects of the experimental lesions, or of the stimulations applied to the experimentally impaired brain, upon his animals' behavior.
The concluding part in Flourens' research script concerned the generalizations that could be drawn from the bulk of experimental data. These generalizations were given a linguistic touch that gave them the appearance of theorems. They read as follows:

(i) The cerebral lobes are the exclusive site of sensations, perceptions, and volitions.

(ii) All these sensations, perceptions, and volitions concurrently occupy the same area in these organs.

(iii) The cerebral lobes, the cerebellum, and the quadrigeminal bodies may lose a portion of their substance without losing the exercise of their functions. They are able to recover it after having lost it completely.30

The reconstruction of the physiologically-based research script of the Flourensian type has thus far not yet reached the level where we could clearly settle the issue of congruence versus lack of congruence of cerebral substratum and mental function. Some additional interpretive work therefore needs to be done. Let us consider the descriptive propositions (1) that the animal is still capable of exercising mnestic functions if one cerebral lobe is removed, (2) that memory vanishes at once when both lobes are removed, and (3) that an animal still hears and sees when merely some portion of the lobes have undergone experimental destruction. These propositions refer to, or may be considered as experimentally tested deductions from, the above quoted theorem three. The connection between theorem three and these descriptive propositions entails that the substance of the cerebral cortex is redundant, for the removal of some bits from either lobe does not cause the impairment of the functions subserved by the lobes in their entirety. Hence, these functions are not individually localized in some specific portion (or area), which means that both cerebral lobes indistinctly constitute the organ of sensation, perception, memory, and volition. The idea that sensation, perception, memory, and volition are indistinctly subserved by the totality of the cerebral cortex is to be understood in neurophysiological terms. But, structurally speaking, the corresponding psychological functions are hierarchically ordered—a notion which is made explicit in the following passage:

Animals deprived of their lobes no longer have sensation, judgment, memory, or volition; for there is volition only if there is also judgment; judgment if there is memory; memory if there has been sensation. The cerebral lobes are, therefore, the exclusive site of all sensations and of the intellectual faculties.31

The consequence resulting from both the neurophysiological and the psychological aspects is that, strictly speaking, mental functions (except sensation) cannot be impaired separately. There cannot possibly occur...
a loss of volition, if sensation (vision, hearing and, one might assume, the other modes of perception), judgment, and memory remain intact; and there occurs, again strictly speaking, no amnesia if the other functions work normally. However, all perceptual and intellectual functions collapse simultaneously as soon as the cortical substrate is totally removed.

Moreover, Flourens repeatedly affirmed that he did not observe one single sign of muscular contraction when experimentally stimulating the cerebral lobes of his animal subjects. This contention refers to theorem one, according to which the lobes are the exclusive site of perception, memory, judgment, and volition—hence, not of any kind of motor behavior. Thus, motility (the capacity of executing bodily movements such as climbing upon a rock or running away) reveals itself to be a function (or a set of functions) of its own, which is active independently of the will, but which may be monitored by the latter. Intelligence is, one might say, essentially self-contained and devoid of motor aspects. Paraphrasing the commissioners of the French Academy of Science, who were in charge of examining Flourens’ memoir, one could say that, in depriving an animal of its cerebrum, one puts it into a state of sleep, whereas the removal of the cerebellum arouses in the animal a state of drunkenness.

In sum, the will, mingled with—or being hierarchically on top of—other mental functions such as perception, memory, and judgment, is both functionally and cerebro-geographically set apart from locomotor centers (in animals and man) and from the deliberate planning and execution of motor acts (in grown up humans). Moreover, there is no special organ (in the Gallian sense) for volition or memory or, for that matter, for seeing or hearing or judging or valuing. Thus, such ‘intellectual’ functions are not completely congruent with the material substance that harbors them. That is to say, the mental (as property of an ensemble of functions such as keeping traces of past experiences or recognizing things that have already been experienced as ‘the same’, etc.) characterizes itself by a sort of vital ontological surplus that purportedly cannot be reduced to the physical processes occurring in the organic substrate that is believed to exercise the said mental functions. Hence, there obviously must exist a principle, a force, or something like that which enacts itself in the absence of some corresponding neural matter. The resulting partial absence of congruence between the physical activity taking place within the lobes and the outcome of such activities is indicative of Flourens’ dualistic notion of mental functions, since even substantial destruction of the neural material does not cause impairment to the four types of functions listed above.
5. The Clinical Approach to Brain Research

The leading French physicians interested in neurological disorders drew from Gall's approach as well as from that of Flourens. This is the reason why both conceptions had to be presented in outline here. However, in doing so, these physicians did not endeavor to blend the two approaches. Their view was no cocktail consisting of fifty percent Gallian organology and fifty percent Flourensian dualism. Rather, the study of neurological impairments gently, but firmly, forced them to endorse Gall's organology (i.e., the notion according to which mental functions are localized in split, distinct, organ-like parts of the brain). Yet, they would use Flourens' techniques of analysis when they wanted to check certain clinical findings by way of in vivo physiological experiments performed on animal subjects.\(^{32}\)

The flip side of the history of French clinical brain research is that it flatly contradicts the traditional account according to which Flourensian neurophysiology successfully destroyed (by way of refutation) Gall's purportedly ridiculous theory; in addition, some Gallian believers survived in hidden niches of the medical institution despite the fact that their neurological heterodoxy had been proven to be false. The sources indicate, on the contrary, that phrenology (i.e., the doctrine that the bumps on human skulls are indicators of mental faculties and moral dispositions) had no impact at all upon the leading French experts of neurological disorders, and that Gall's organology was the dominant and socially accepted frame of reference in the realm of medicine (and within the medical institutions). In other words, French clinical brain research was essentially materialistic (and thus opposed to Flourens' dualism), organological (and thus opposed to Gallian phrenology), and heavily tending towards split localization (and thus, once again, opposed to the loose localization of the Flourensian type).

The clinical research script can be reconstructed on the basis of medical texts (handbooks, articles, monographs, notes, etc.) with a fair amount of certainty. In contradistinction to the physiological script, the clinical one relied on data of a different type. These are to be classified into two large groups. On the one hand, there were the givens of semiology, i.e., data referring to visible or otherwise graspable morbid phenomena such as fever, transpiration, accelerated breathing, motor impairment, and so on. On the other hand, there were anatomical data gathered according to the then prevailing practices of dissecting human bodies. Since the social role of the medical community was to provide care for patients, the relevant clinical brain research script which I try to extract from the medical texts partly reflects the professional activi-
ties. Indeed, my reconstruction deliberately ignores the various acts that were performed within the context of healing.

Antoine-Etienne Reynaud Augustin Serres (1786-1868) sketched the clinical strategy by saying that once an illness of the encephalon seems to become manifest, the physician ought to localize the illness. The script of clinical brain research reads thus:

(i) Examine all physical and behavioral symptoms and focus on those signs which seem to indicate the presence of a lesion in the brain.

(ii) Determine the immediate cause of the illness.

(iii) Verify the diagnosis by post mortem analysis of the physical features of the brain.

(iv) If necessary, perform physiological experiments in order to determine the relationship between cause and effect (i.e., between the lesion and its physical and behavioral outcomes).

The script aimed directly at the twofold identification of (1) the cause that produces behavioral and other corporeal alterations and (2) the focus of the morbid condition in the brain. Note that the cause was to be sought in the deterioration of neural matter itself, and not in some external circumstance (e.g., in yearlong exposure to damaging hygienic conditions). The underlying idea turns out to have been as simple as it was plain: the morbid signs, which clinical expertise would interpret as direct offsprings of some pathological alteration, were understood as being caused by a disease that affected the usual functioning of the organs that were believed to harbor some specific function. Since Serres and other contemporary physicians were dealing in particular with neurological symptoms, the proximal causes had to be sought in the malfunctioning of the nervous system (and not in the liver).

Serres was a highly regarded clinician, with above-average anatomical skills. He collected anatomical reports—like an entomologist collecting insects on an expedition to tropical regions—in masses. Exceptional historical circumstances, viz., the concentration of military personnel around Paris following Napoleon's defeats in 1813 and 1814, leading to the arrival of a huge number of injured soldiers, not only created problems of health care, but also a boom of diagnostic, therapeutic, and anatomical work. Serres treated some two thousand cases of paralytics from the point of view of 'internal pathology' as well as from that of 'external pathology', as he affirmed. 'External pathology' was the common name denoting the series of manipulations performed by a physician inasmuch as observable symptoms (here: observable heavy motor disruptions or loss of motility) were at stake. 'Internal pathology', in contradistinction, denoted the anatomical inspection that aimed at the identification of the focus of the illness. Such great numbers of cases did, in general, permit one to set up taxonomic clusters and thus to con-
struct a nosological system, thanks to which more accurate diagnoses are obtained as time goes on.

This clinical approach was crucially relevant on at least two counts:

(i) If motor disorders revealed themselves to be caused by lesions of either (or both) cerebral lobe(s) without being concomitantly caused by other lesions in the nervous system, then clinical data would contradict Flourens' physiological dogma.

(ii) If mental disorders could be shown to be differentially caused by lesions of the cerebral lobes or of parts thereof, then the relevant clinical data would also cast doubt upon Flourens' conception.

The clinical observations pertaining to either of these disorders were not of the same type. Motor disorders caused by any lesion of the cerebral lobes were relevant in the sense that, if it could be shown that dysfunctions of motility were exclusively linked with cerebral lesions, Flourens' sweeping assertion that the cerebral lobes were void of motor functions would be refuted. Clinical observation was thus expected to show whether there could be a narrowly circumscribed lesion in a cerebral lobe, which would cause a real motor impairment without there being other, concomitant, observable symptoms and lesions.

In contradistinction, clinical observation of mental impairment could show whether a single psychological function (e.g., perceiving cubes as cubes, uttering one's thoughts, etc.) might obtain without there being a loss of other psychological functions.

Let us consider the passages where Serres dealt with cases of hemiplegia. He obviously took an outspoken interest in such cases, for he seems to have been convinced that they were lying at a new frontier of brain research.

According to standard nosological knowledge, Serres expected that pathological affections of the spinal cord would cause "paralyses of the nerves or of the limbs located below the affected parts." But no pathologist, he asserted, had ever really considered more closely "cases of hemiplegia where only the leg or only the arm is paralyzed." Therefore, thorough investigations were lacking that could permit one to settle the problem as to "whether the organic alterations of the encephalon affected different or analogous regions." And he added that "[t]he action of the cerebellum is even more doubtful as far as the movements of the limbs are concerned; one does not know whether it [sc., the action of the cerebellum] is crossed as it is in the encephalon."

In order to shed some light on this issue, Serres examined several patients who suffered from a paralysis either of a single arm or of a single leg. But the figures included in his text are far from clear. In the years 1814 and 1815, he treated seventy-six cases of hemiplegia; forty-seven patients were suffering from paralyzed arms and legs; twenty-
two could no longer move their arms, whereas their legs were less affected, so that they could still walk, though with some difficulties. In five patients, only one leg was paralyzed, and in nine other patients, one arm was motionless, all other limbs remaining unaffected. In the years 1816, 1817, and 1818, seven other patients showed up, of whom five displayed a motionless arm, whereas the other two suffered from a paralyzed leg.

The nineteen cases of hemiplegia turned out to be the truly interesting ones, although one does not know with precision how many patients were examined and treated by Serres under the nosological heading of hemiplegia. One might add, however, that nineteen very similar cases constituted a sufficiently large group, and that everything depended upon the description of the disease if these cases were to be used to prove (or refute) anything at all.

At this point, things become intricately complicated and slightly confusing. For there is a second thread, which got knotted with the first one. The second thread relates to another nosological category—that of apoplexia cerebelli, i.e., cerebellar stroke. In fact, the main thread of the story, which shows Serres heating up his talents as a practitioner and medical writer, concerned cases of cerebellar stroke, whereas the phenomenon of hemiplegia came to be examined as an interesting diversion. Another striking contrast ought to be noted. The passages on cerebellar stroke are filled with detailed case histories. The analysis of hemiplegia, by contrast, remains somehow abstract: it contains not one case history and the report of anatomical data is scarce, as if Serres had not encountered a prototypical case, but rather only an array of more or less similar cases. These contrasts show that our author was quite aware of the distinction between rough clinical reports yielding, or supporting, a composite picture of a disease, on the one hand, and detailed studies of individual cases, which, because they are tellingly paradigmatic, illustrate a disease, on the other hand.

Serres was focusing on cases of cerebellar stroke because he aimed at improving the methods by means of which one could know which lesion of the cerebellum was unambiguously signaled by the presence of one or several symptoms. And it so happened that he and his collaborators observed that male patients not only became drowsy, but also displayed signs of over-stimulation of the genitals as a consequence of a cerebellar stroke. Hence, whenever a male patient would show up who was both in a state of drowsiness and of sexual over-stimulation, these signs were taken by logical implication to be necessarily indicative of a cerebellar disorder, i.e., of a physical deterioration in the living substance of the cerebellum, but in no other neural or non-neural parts of the body.
This series of clinical studies revealed to Serres that the reproductive faculty in man was localized in the cerebellum—as had formerly been asserted (and predicted) by Gall. By the same token, the same clinical analyses of cases of cerebellar stroke showed that this part of the brain was not the seat merely of motor faculties, as Flourens had presumptuously stated. But Serres did not yet, at this point, openly declare his sympathy either for the lumping approach of Flourens or for the splitting approach of Gall.

The study of hemiplegia now turns out to have been an appendix to the analysis of cerebellar disturbances that, in turn, was part and parcel of an encompassing research program of clinical neuropathology. But instead of further considering the cerebellar-cerebral analysis as a whole, I shall focus on a seemingly subordinate, but theoretically explosive, clinical detail.

Serres reports having found a lesion in the cerebral lobe in the brains of two patients suffering from paralysis of only one arm, their cerebella having remained entirely healthy. The passage to which I refer indicates that:

in both of them [sc., in both patients], the anterior parts of the corpus striatum were unaffected, whereas the posterior part of the median lobe and the posterior lobe were the exclusive areas of disorganization; in one of them [sc., patients], the focus had destroyed the posterior and superior parts of the hemisphere on the side opposite to the paralysis; in the other one [sc., patient], the disorganization extended from the center of the median and posterior lobe to the bottom and the top of this lobe, cutting across the posterior optic radiation.¹⁰

Even if Serres had misread both the signs of external pathology (the observable changes in the patient’s behavior) and the alterations found on the cerebral tissue (by anatomical inspection of the dead patients’ brain), the proposition that the impairment of the arm was exclusively due to a single lesion in the cerebral lobe could in no way square with any of Flourens’ theorems concerning the localization of motor acts. In other words, whatever the accuracy of Serres’ external and internal pathological examination may have been, the theoretical idea that a destruction in the cerebral lobe (and nowhere else) would cause a specific motor impairment (the paralysis of a limb) amounted by and large to a substantive corroboration of Gall’s organological approach.

This point is of utmost importance. It shows that French physicians who were interested in brain research observed pathological conditions that, to judge by the medical language that was used at the time, indicated (1) that motor functions were also localized in the cerebral lobes
and (2) that cerebral functions were distributed over the surface of the
cortex.

Yet there is more to it. Let us grant that no terribly misleading mis­takes had been made in the usual process that led from diagnosis to
anatomical analysis, and let us assume that Serres did not make any­thing when he was putting his observations and thoughts on paper for
publication. Then we have to acknowledge that Serres’ view barred the
possibility that there existed a purely motor module (the cerebellum in
Flourens’ theory) monitored, or acted upon, by a module of intellect and
will (the cerebral lobes in Flourens’ theory). It is evident from Serres’
report that no physical disruption of the cerebellum has been found in
the aforementioned patients. It is also evident that only some portions
of the lobes had undergone pathological modification—not enough,
according to Flourens, for a breakdown of the volitional and intellectual
dispositions. Hence, Serres in fact implicitly asserted that some motor
functions were under direct command of one or both cerebral lobes, and
that motor command was, as one might say, split or fragmented, since
the two patients could deliberately move their legs, lips, eye-lids, etc.,
but not one of their arms.

Serres’ examination of the fabric of cerebellar activity thus ended up
with a truly Gallian, anti-Flourensian view of the functional split of the
cerebral lobes. Along the same lines of analysis, and founded on the
same research script, one finds the investigation of Jean-Baptiste
Bouillaud (1796-1881) into the disorganization of (broadly speaking)
linguistic performance caused by local brain lesions.

6. Aphasia and the Localization of Speech

Bouillaud’s contributions to the study of the syndrome that, in the
omenclature of Broca, was christened ‘aphemia’, and only later on
came to be renamed ‘aphasia’, are masterpieces of the medical litera­
ture of his time: not only do they describe the state of the art among
French clinicians better than many other printed sources, but they also
depict the standard practices of those experts who, like Bouillaud, were
exploring new territories of neurology.

Bouillaud’s primary intention was to test two hypotheses—the gen­
eral hypothesis that mental functions constitute topologically separate
units, and the specific hypothesis that the center of linguistic perfor­

cance, the organ of verbal utterance, ought to be defined as just one
signal region (or field, or portion) among the variety of cerebral organs.

By speech (not language), Bouillaud meant the complex motor activ­
ity necessary for the production of audible, syntactically well struc­
tured, meaningful messages. Among the five hundred or so cases of
aphasia he studied in more than thirty years, he encountered many patients who, though unable to articulate their thoughts, feelings, impressions, etc., were still capable of moving the tongue, the lips, and other parts of the vocal apparatus, of expressing moods and emotions by physiognomic means, of laughing, of swallowing, etc. Furthermore, he made the crucial observation that the nerves that channel the impulses to the speech effectors (i.e., to the muscles of the verbal sound machine, as one might say), do not originate in the cerebellum—as one should remember by now, the cerebellum was, according to Flourens, the exclusive motor center of the brain. Aphasic disturbances were perceived by Bouillaud as being caused by physical alterations in an independently working cerebral region (or organ). Here is one typical case history presented before the Royal Academy of Medicine on February 21, 1812:

François Retard, 59 years old, was admitted at Cochin Hospital on the 17th of January, 1822. Due to some cerebral affection, he persistently displayed some difficulty in speaking, so much so that in spite of all his efforts, he merely managed to mumble a few words. He would somehow automatically repeat the last words of sentences which other persons uttered in front of him, and would become impatient because of his inability to respond. He died after some months spent at the hospital. In the course of the autopsy, a tumor was discovered on the anterior third part of the left cerebral hemisphere; the tumor had grown to the size of an egg and had the consistency of boiled albumine, which contained some blood clots and some drops of still liquid pus.

In a footnote, Bouillaud added: "In the case of this patient, the alteration of speech was linked as much to the lesion of the brain considered as the organ of intelligence as it was to the lesion of this same organ considered as a center of muscular movements."

The interesting aspect of both passages is their nosological ambiguity, for the total disturbance caused by the cerebral lesion was seemingly interpreted along two different lines. On the one hand, the lesion is said to have possibly caused a more or less severe disruption of the patient's intelligence, which could, in turn, have affected the propensity to articulate his thoughts and feelings. We would thus have the following chain of causally related links: the tumor caused a dysfunction or a disturbance of the patient's intelligence, and due to this mental disorder the patient was no longer really capable of uttering more than some few recognizable vowels—what else could he have expressed anyway, since his mind would have been in a state of utter confusion? On the other hand, the tumor is, in addition, said to have possibly caused the impairment of the cerebral center in command of speech performances, and, concomitantly, a certain confusion of the mind (due to the break-
down of finding the right words for the thoughts and feelings the patient was experiencing).

Once again, the roughness of Bouillaud's diagnosis is conspicuous—the diagnostic procedures that neurologists would be using some thirty or forty years later would have led to a more fine-grained, as well as to a more informative picture. Yet, such hypothetical improvements are not counterfactuals. Both the diagnoses and theoretical conclusions reached at in a certain epoch should be appreciated for what they are, and not for what they possibly might look like had the historical circumstances been different. Even so, Bouillaud's *theory* is captivatingly interesting because it sealed the study of mental disorders accompanying the neurologically describable disorganization of the nervous matter with the wax of materialism.

Bouillaud stressed on several occasions that lesions in the anterior portions of the (usually left) cortical lobe *selectively* caused the motor breakdown of speech. The patients he observed and treated could *intentionally* move their arms; they would stand up and move around *intentionally*; they could *intentionally* move their tongue; they would *intentionally* look at something, i.e., move their head and eyes in order to optimize visual acuity. But they could no longer fulfill the intention of speaking. Therefore, the propensity of performing linguistic acts had vanished from their repertoire. In contradistinction, the function of recognizing verbal messages remained more or less safe, as the case of François Retard showed, for otherwise the patient would neither have repeated the last words of sentences he had just heard nor would he somehow have noticed that his sound-producing machine could no longer be reined in upon command. Hence, the patient's intelligence, as far as it manifested itself in the fulfillment of acts of purposive locomotion or in the understanding of speech acts, was rather well off, whereas the same patient's intelligence, as far as the autonomous production of utterances was concerned, had suffered a severe breakdown. And this entailed, contrary to Flourens' fundamental principle of non-partitioned, non-'splittable', non-fragmented intelligence (and will), that higher mental functions not only could be unrelated to the cerebellum when being enacted in individual performances (speech acts or acts of gazing at something, etc.), but that they also were embodied by single cerebral organs that controlled them. Therefore, psychophysical processes, such as conveying one's thoughts by voice or focusing one's visual attention upon an object, were but the expression of the activity of *parts* of the working brain—and this, to be sure, was medical materialism in its most pristine form.

As indicated above, the work of Bouillaud has been perceived by many historians of neurology and neuropathology as well as by some
historically interested physiologists as constituting an exception—in the sense of ‘not representing mainstream brain research’ or being ‘merely clinical’ or ‘single-minded’. However, the articles and the clinical case studies published in the standard medical literature (e.g., in the transactions of the Academy of Medicine) between 1820 and 1850 suggest that, if the localizationist, originally Gallian approach in clinical neurology had been a queer trait characteristic of a group of physicians deviating from the golden path of mainstream brain research, these physicians would still have formed a pretty large group, would still have been well established in medical institutions, and, above all, would still have taught the approach that to them had turned out to be highly workable.

Let us take a look at the ‘localizationist spirit’ that permeates one of the most studied textbooks in general clinical medicine of the time. The fifth edition of Gabriel Andral’s (1797-1876) Clinique médicale was published in 1840. Volume five of this huge textbook is entirely devoted to disturbances of the central nervous system. It consists of a nearly unending series of individual case histories, all of which are presented to the neophytes according to the general schema (or script) of clinical brain research: first, the external pathological signs and symptoms are registered with care and as richly as possible; then, the anatomy of the internal, pathologically altered structure of the central nervous system is described; and finally, the cerebro-geographic seat of the lesion(s) is determined and correlated with the symptom(s) previously observed when the patient entered the ward. Though some observations were ultimately confusing—so much so that Andral felt compelled to note that “there exist organic lesions without symptoms, as there exist symptoms without perceivable lesions which could explain them [sc., the symptoms]”44—the guiding idea running through this volume of the textbook was that of finding the locus of lower and higher mental functions in (and on) the brain.

In the section where Andral dealt with two cases relating specifically to the loss of linguistic capacities, another clinical oddity manifested itself to the observer. An eighty-year-old woman was admitted to the Pitie clinic and examined. For three years she had been incapable of uttering the simplest word, but would show no impairment relating to her understanding of verbal messages addressed to her, nor were there any other observable disturbances:

When we began to examine this woman, she was strictly incapable of articulating one single word; however, she understood perfectly what she was told, as her physiognomic movements and the different gestures indicated. This dumbness had not been caused by the loss of the motility of the tongue, for the latter could move in any
direction. . . . Everything seemed to indicate that her intelligence had remained intact. All four limbs could move freely and easily; the patient also felt the painful impressions which we purposely caused on her skin. When asked whether she felt headaches, or whether she had earlier done so, she answered by a negative gesture. Hearing, sight, and sense of smell proved to function normally.45

Yet, when anatomically inspecting the brain, Andral did not spot the cerebral lesion where he had expected to find it on the brain’s surface, viz., somewhere on the posterior part of the frontal lobe. The lesion—portions of grayish, softened tissue resembling a concentrated solution of starch—showed up, rather, on some inferior parts of the left hemisphere as well as on the corpus callosum (or commissura magna cerebri, i.e., roughly speaking, the mass connecting the two cerebral hemispheres at their inner surface). Andral’s comment reads thus:

We have here a proper observation which is likely to cast doubt upon many opinions that have been set forth recently. The only existing cerebral accident is the loss of speech, and the only existing brain lesion we have been able to discover anatomically does not have its seat on any point of the brain to which the faculty of forming utterances has been ascribed.46

Nature had thus performed on a single brain a kind of experiment whose outcome did not fit prior inductions from a long series of convergent clinical observations. But this single case did not cause any severe theoretical (or even philosophical) headache to Andral, to whom the post mortem evidence, combined with the in vivo examination of the patient’s behavior, simply suggested that the localization of more or less complex mental functions was perhaps more arduous than expected, and not that the notion of localization had to be given up for good.

By 1850, Bouillaud had accumulated approximately five hundred reports of patients suffering from linguistic disorders due to cerebral lesions on specific “points of the brain,” to use Andral’s words. Many of these reports originated in clinics scattered throughout Paris and the French provinces.47 Thus, even if we (exaggeratedly) assume that some two hundred cases had been examined and studied by Bouillaud alone, we would still have to recognize that other physicians worked and thought within the selfsame paradigm of clinical brain research. Hence, this paradigm had reached stability and social visibility in the mid-century without the assistance of the traditional, pre-1800 metaphysics and its conception of the mind as being ontologically split off from the living matter.
7. Conclusion

The reconstruction of French clinical brain research makes clear that the human mind became gradually conceptualized as an ensemble of functions instantiated by spatially circumscribed and mutually connected portions of neural matter. This historical process took place in hospital wards as well as laboratories or laboratory-like places (such as slaughterhouses). Its actors were mostly medically and/or biologically skilled observers and researchers working according to sets of epistemically plausible and epistemologically accountable rules that prescribed how to gather the data, how to classify observations, how to relate anatomical findings to symptoms that manifest them on visible, graspable, measurable bodies and in behavioral particularities (dumbness, paralysis, drowsiness, etc.). These rules, to be sure, were not informed by grand metaphysical or ontological systems, but rather by the epistemically well-established fields of medical diagnosis, physiological experimentation, and logical reasoning. However, the results obtained by experimentalists and clinicians in the field of neurology could be turned into objects of philosophical debate.

One can crudely distinguish two types of such philosophical debates. On the one hand, there were debates focussing on the vanishing relevance of the philosophy of mind. If the bio-medical disciplines could account for the fabric of mental life, and could do so in a discursively sound manner, there was no need to resort to the beliefs of traditional philosophies of mind. On the other hand, there were debates as to whether empirical knowledge produced in medical institutions and physiological laboratories corroborated opinions that had originated in traditional philosophies of mind. Thus, Flourens’ dualistic theory of cerebral cum mental functions was used as a discursive weapon against anti-Cartesian, materialistic conceptions of Man. Yet, one should still keep broadly ‘philosophical’ or ‘ideological’ controversies apart from the production of knowledge concerning the ensemble of mental functions—on whatever level of the evolution of animal species. And within the realm of the production of knowledge, philosophies of mind simply disappeared from the arena of research or kept themselves alive by reacting to neurological, neuropsychological, or neurophysiological findings. To put it differently, the pre-1800 philosophical mind-body problem split away from the main research trend, which got along without philosophy. Thus, the philosophical beliefs that physiologists, physicians, moralists, or socially prominent intellectuals were proclaiming no longer had a positive impact upon the scientific mind-brain research. Seen from within research, the mind of (human and non-human) animals constituted a problem not essentially different from common bio-
logical problems one could raise in the study of processes such as digesting or dreaming or sleeping or growing or aging or dying. The problem was simply that of living matter.

But the assertion that theories of living matter solve all problems raised with respect to the human mind would be too simple-minded. Indeed, if it were the case that the study of living matter on any level of its organization and complexity could offer the final answer on how organisms relate semantically to real and ideal objects, then one could just as well confine one’s investigations to the ‘tacit’ processes taking place in amoebean animals. But even for scientific materialists, the specificities of mental functions in human organisms ought not to be simply reducible to each and every process of living organic matter, as if the human brain reflected merely a huge heap of interconnected amoebean animals. To put it differently, the individuation of living matter turns out to be the crucial issue—and whether one calls this issue ‘philosophical’ or ‘theoretical’ or ‘biological’ does not really matter. The notion of ‘individuation’ seems to refer back to a philosophical tradition that one might call ‘Spinozistic’. For a ‘Spinozistic’ approach, the secret, abyssal, mysterious consciousness of the philosophies that think in terms of subjectivity, ego, soul, etc., dissolves into a set of determinations in virtue of which an individual is capable of exercising her or his mental faculties. This, anyway, is sufficiently rough stuff in itself—philosophically or otherwise—yet less rough than if it were phrased with the tools of the philosophy of the subject.

NOTES

1. That is, the layer concerned with the classification of diseases, here especially of neurological diseases and of neuropsychological disorders, such as the loss of the ability to speak or of the ability to grasp the meaning of utterances.


3. Note, however, that Napoleon, during his exile on the isle of Helena, confessed that he was proud to have reinforced those who fought against the (purportedly) false organological theory of Gall (see below); cf. Michael Hagner, Homo cerebralis. Der Wandel vom Seelenorgan zum Gehirn (Berlin: Berlin Verlag, 1998), pp. 308-9n. 42.

4. It should be mentioned that Haller operated with, in addition to these two types or classes of bodily parts, a third class of bodily parts, i.e., the class of tendons, which were said to be neither irritable nor sensitive, but solely...
elastic; hence, when broken, tendons were believed to contract and thereafter to remain immobile.


6. This assertion ought to be qualified. Indeed, for Jean-Baptiste Lamarck, e.g., the brain was *no longer* a problematic organ, for it was ontologically constituted like any other organ; cf. Lamarck, *Système analytique des connaissances positives de l'homme* (Paris: PUF, 1988; photomechanical reprint of the first edition), p. 139: “Each animal faculty, whatever it may be, is an organic phenomenon; it results from a system, or apparatus composed of organs, by which it is instantiated, so much so that it necessarily depends on this system.” See also p. 166: “The [mental] phenomena [in man] . . . are exclusively organic, hence entirely physical, constantly perfectly related to the state of the organ, or the organs, by which they are produced, and maintain their integrity, their resulting perfection, as long as these organs are maintaining theirs . . . .” The term ‘animal faculty’ in the prior quote does not denote the faculty of some infra-human animal, but rather any disposition or faculty involving the nervous system (instead of some part of the glandular or digestive system). All translations are my own unless a published English translation of the work is cited.

7. Though new theories do not emerge out of the blue all at once (their creators, it should be remembered, have received some education and have first been exposed to traditional, conventional, plainly normal scientific knowledge before experiencing some seminal ‘eureka!’ that lies at the bottom of what possibly turns out to be a novel or even a revolutionary theory), the stipulation of some base line from which a story of interrelated discoveries, theoretical constructions, and controversies unfolds, cannot do much harm to the endeavors in the history of science. The narrative must, after all, get started somewhere, and the antecedents, which may still be genealogically relevant in some other context (or from a different point of view), have to be bracketed for a while. Similarly, for evolutionary theories of a Darwinian, a Haeckelian, or some other type—think, for example, of Goldschmidt’s theory of hopeful monsters as the real actors of speciation—there is no doubt that all and not merely some modern species have a common ancestor. However, there is no need to rehearse the *whole* history of life when one addresses the story of what once was classified by Blumenbach as *Elephas primigenius*, and what today is classified as *Mammuthus*; for the history of this extinct animal, see Claudine Cohen, *Le destin du mammouth* (Paris: Seuil, 1994).

8. The conception—or should one rather say, the hypothesis—that mental functions are not randomly or haphazardly embodied by some part of the body is, of course, not an ‘invention’ of the late eighteenth and early nineteenth century. Thus, Ibn Sina (Avicenna) (980-1037) located what might be rendered as ‘the faculty of thinking’ or ‘cogitative propensity’ (*al-quwwa al-mutafakkira*) in the mid-portion of the human brain, and, in contradis-
tinction, what might be rendered as 'the faculty to retain' or 'the faculty to keep in memory' (al-quwwa al-dakira) in the posterior region of the human brain (al-mutafakkira) being derived from the root fkr semantically related to 'to think', and al-dakira being derived from the root dkr semantically related to 'to remember'). However, in the case of Avicenna (or of other physicians of that time), we are dealing with an epistemic regime that differs in nearly all respects from the epistemic regime of nineteenth-century clinical brain research. Hence, to perceive and interpret Avicenna’s conception straightforwardly as a speculative forerunner of modern neuropsychology (and of its neurophilosophical underpinnings) turns out to be misleadingly and Whiggishly anachronistic.

9. Gall, for theoretical reasons as well as on strategic grounds, outspokenly rejected the name ‘phrenology’; cf. Michael Hagner, Homo cerebralis, pp. 99-118.

10. Patriotism and sociability are among the faculties described and classified by phrenologists.


12. This is Hegel’s anglicized term for ‘Knorren’; see G.W.F. Hegel, Phénoméno­logie des Geistes (Frankfurt am Main: Suhrkamp, 1986), p. 253; English translation, Phenomenology of Spirit, trans. A.V. Miller (Oxford: Oxford University Press, 1977), p. 202. In the concluding lines of the chapter where he deals with the issue of phrenology, Hegel opposes the encompassing power of the human spirit to understand itself in its unlimited power, on the one hand, to the mere contemplation of the physical, external manifestation of mental dispositions, on the other hand. Hegel thereby draws a parallel between the creative act of reproduction and the evacuation of organic liquid. The apotheosis of this mildly sexist comparison reads as follows: “The infinite judgment, qua infinite, would be the fulfillment of life that comprehends itself; the consciousness of the infinite judgment that remains at the level of picture-thinking behaves as urination” (p. 219).


15. Franz Joseph Gall and Johann Caspar Spurzheim, Recherches sur le système nerveux en général et sur celui du cerveau en particulier; Mémoire présenté à l’Institut de France, le 14 Mars 1808; suivi d’observations sur le rapport qui en a été fait à cette compagnie par ses commissaires (Paris: Schoell & Nicole, 1809), p. 29: “beginning the dissection with cuts of the upper part of the hemispheres, one merely succeeds at truncating them one by one, thus becoming unable to gain a true knowledge of them.
Indeed, it would not be an appropriate expedient for knowing a machine if one first destroyed its constituent parts, for the examination would then only bear upon the defective debris without the least relation to the whole

[en commençant la dissection par des coupes de la partie supérieure des hémisphères, on ne parvient qu'à les tronquer successivement, sans en pouvoir prendre une connaissance exacte. En effet, ce ne serait point un expédient heureux pour arriver à la connaissance d'une machine, que de commencer par détruire les parties dont elle se compose; car alors l'examen ne pourrait plus porter que sur des débris défectueux et sans aucun rapport d'ensemble]."

16. Cf. ibid., p. 262.
17. Ibid., p. 139.
19. This is the portion of the hindbrain immediately rostral to the spinal cord.
22. Ibid.
24. The rather tough, double layer of collagenous fiber that encloses the brain.
25. The membrane lying in between the dura mater and the pia mater.
26. The connective tissue clinging to the surface of the brain.
27. Flourens, Recherches, p. 17; quoted from Clarke and O'Malley, The Human Brain, p. 486.
28. Ibid., p. 18; p. 486.
29. This summary of critical remarks addressed to Flourens echoes the objections of his contemporaries.
30. Flourens, Recherches, pp. 121-2; quoted from Clarke and O'Malley, The Human Brain, p. 487.
31. Ibid., pp. 97-8; p. 487.
32. Cf. e.g., Antoine-Etienne Serres, Anatomie comparée du cerveau dans les quatre classes des animaux vertébrés appliquée à la physiologie et à la pathologie du système nerveux (Paris: Gabon, 1826), vol. 2, pp. 689-91.

34. Paralysis of one side of the body.

35. Serres, “Suite des recherches,” pp. 125-53. Note that Serres referred first to the impairments of the limbs due to a lesion of the spinal cord and thereby asserted that the paralysis occurring in such cases concerned the part “above” the lesion: “les altérations diverses de la moelle épinière . . . déterminent des paralysies des nerfs ou des membres placés au-dessus de la partie affectée . . .” This is most probably a printing error.

36. Ibid., p. 126.

37. Ibid.

38. Ibid., pp. 126-7.

39. Similar in terms of external pathology.


41. Jean-Baptiste Bouillaud, “Recherches cliniques propres à démontrer que la perte de la parole correspond à la lésion des lobules antérieurs du cerveau, et à confirmer l’opinion de M. Gall, sur le siège de l’organe du langage articulé,” *Archives générales de médecine* 8 (1825), p. 32.

42. The author used the old-fashioned word ‘viscère’ here to denote the entire brain.


45. Ibid., pp. 438-9.

46. Ibid., pp. 441-2.


49. Most of this article has been written during my stay as guest researcher at the Max Planck Institute for the History of Science, Berlin, in 1997-1998. I thank Charles Wolfe (Boston/Paris) for his enduring (and very
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