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Being neurologically human today:

Life and science and adult cerebral plasticity (an ethical analysis)

ABSTRACT
Throughout the 20th century, scientists believed that the adult human brain is fully developed, organized in fixed and immutable function-specific neural circuits. Since the discovery of the profound plasticity of the human brain in the late 1990s, this belief has been thoroughly undermined. In this article, combining ethnographic and historical research, I develop an "ethical analysis" to show that (and in what concrete sense) the emergence of adult cerebral plasticity was a major mutation of the neurologically human—a metamorphosis of the confines within which neuroscience requires all those who live under the spell of the brain to think and live the human. [*neuroscience, adult cerebral plasticity, France, ethics, ethnography, science and technology studies*]

> Science is meaningless because it gives no answer to our question, the only question important for us: What shall we do? How shall we live?
>
> —Max Weber, *Science as Vocation*

The relation between life and science—a relation that has often been said to be no relation at all—surfaced many times in the course of my fieldwork among Parisian neuroscientists. A scene here, a word there, minor events , which indicated that the two are somehow related. But it was only toward the end of my stay that the relation as such, the question of what kind of relation this is, became a serious intellectual concern for me—a concern that was to profoundly reshape my comprehension of the contested emergence of adult cerebral plasticity, the actual topic of my research. What gave rise to this concern were three "ethnographic incidents."[1]

Life and science

Life and science I

Late one Friday evening in April 2003, I had a conversation with Philippe Ascher, perhaps the best-known Parisian electrophysiologist. Ascher had asked me to visit him in his lab, a curious space full of electronic devices, power cords, and computers. The reason for this invitation was our common interest in the history of French neuroscience. That evening we discussed the battles between electrophysiologists and molecular neurobiologists that were fought in the 1970s. At one point in our conversation, he made the following remark.

"The nervous machine," he said, pointing to his head, "is organized in fixed and immutable neural circuits. We know at least since the 1940s that these neural circuits are governed by chemical molecules and electronic signals. The arrival of molecular biology didn't add any basic insights. It did not revolutionize our conception of the brain. It was a new tool, that's all."

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Ascher's remark captured my attention because his claim that the brain is organized in fixed and immutable neural circuits ran counter to what, at that time, I had studied for more than a year. Since January 2002, I had worked in the lab of Alain Prochiantz, located at the École Normale Supérieure. The lab's research was organized around an obscure observation Prochiantz had made in the late 1980s, namely, that homeotic genes are expressed in the adult brain and that the proteins they code for—homeoproteins—have the capacity to transfer between cells (Joliot et al. 1991). The observation was obscure primarily because, at the time, homeotic genes were associated exclusively with embryogenesis. They were known to control the development of every known animal, even of plants, and have been frequently described as "developmental genes" or as "master control genes of organogenesis" (Gehring 1998; Nüsslein-Volhard 2004). Prochiantz's findings, however, suggested the curious possibility that some embryogenetic processes could go on in the mature nervous system, that is, that the adult brain could be the locus of a plasticity usually associated exclusively with embryogenesis (it is important to note here that the term *plasticity* indicates the brain's continuous embryogenetic potential to induce new nervous tissue and to thereby change its form).[2] To his fellow neuroscientists, this speculation seemed implausible. Anatomists had reported for almost a century that the adult nervous tissue is strictly fixed and immutable, devoid of any developmental processes. That morphogenetic transformations could occur in the adult, that is, that new neurons could emerge or that old ones could change their form or the form of their connections, that axons and dendrites could appear and disappear, that new synapses could grow, was, from a neuroscientific perspective, simply impossible. When Prochiantz stubbornly insisted that he had made a major discovery, he quickly became an enfant terrible. Gradually, his lab was pushed to the margins of the neuroscientific community, and within a year or two his group was left with almost no scientific contact with other national or international labs.[3] And then, shortly before I arrived in Paris, the unexpected happened: Around the year 2000, after Prochiantz had worked in isolation for almost a decade, several labs reported the birth, differentiation, and migration of new neurons in the adult mammalian brain.[4] The result of these reports was neither a full rehabilitation of Prochiantz nor the beginning of a new way of thinking the brain—at least not immediately. The initial result, rather, was a major and passionate debate about the significance of plasticity: Does it really exist? If so, then do plastic changes have a function? Are they minor phenomena, occurring only in "less important" brain regions? Would one really have to think of plasticity as the main feature of the brain, as Prochiantz (who figured centrally in the debate) claimed it to be? I was curious how—or if—Ascher, given his claim of adult fixity, would integrate Prochiantz into his history of neuroscience.

"What then," I asked him, "is the place of Prochiantz's work in the history of neuroscience? You know that he claims that the brain is profoundly plastic and that the challenge neuroscientists face today is to think the nervous system from the perspective of this plasticity."

Ascher nodded, serious. "Look," he replied, "I like Alain, but his work, you know, it is all speculation. From the perspective of electrophysiology his claims don't make sense. There simply is no space for plasticity in the adult. Where should it occur? The adult brain is already wired, it really is."

Watching Ascher as he explained that "the adult brain is already wired," I was struck by the scene. There he was, dressed in black, with huge black glasses and untamed grey hair, surrounded by black shelves full of electronic devices and computers, with colorful power cords hanging over his head and shoulders, explaining that the adult brain is already wired. It was as if he *met en scène*—bodily—what he said. Perhaps this impression was unremarkable, but at the time I noted it as an example of how science and life may intersect.[5]

Life and science II

Three days later, the relation between life and science surfaced a second time. I was sitting in the coffee corner of Prochiantz's lab, chatting with Laure Sonnier, one of Prochiantz's graduate students (Laure was working on the significance of homeotic genes for the birth of new neurons). At one point, Christo Goridis joined us. Goridis was director of a neurochemistry lab located on the same floor as Prochiantz's research group.

"Neurogenesis," he explained, after he had listened to our exchange for a while, "exists. It exists in the olfactory bulbs. Fine. But does it have a function as well in the hippocampus? Does it occur in the cortex? Far from clear, even doubtable. In any case, neurogenesis is a minor phenomenon. It might be interesting from a pharmacological perspective, as in Laure's work, but not from a scientific perspective. It will not change the way we think about the brain."

He stared at us provocatively, holding on to his coffee and his cigarette. Goridis was tall, very thin, and always a bit nervous—an impression that his intense way of smoking reinforced. His three-day-old beard lent him a sharp, angular look.

"Alain!" he raised his voice and head, "he believes in plasticity!"

He took his glasses off, vigorously rubbed his eyes, and then—apparently performing a lecture—continued to instruct us.

"The character of a person is basically formed around age 25. Rakic and many others have shown that this is the time when the nervous system assumes its definite form. Afterwards nothing really changes anymore. There's no escape!"[6]

With long, mechanical steps, he began walking up and down the little corridor, continuously smoking.

"Whether we like it or not," he went on, "we are machines. I know that many people don't like that idea; they waste their time and energy to show that man isn't a machine. Only, man is a machine. We are machines. In the adult, the neuronal circuits are fixed. They are—and hence we are—regulated by chemical processes. There is no plasticity, the brain is a chemical machine."

I wanted to affirm that people do not like to think of themselves as machines, but before I could finish my sentence Christo interrupted me with a gesture bordering on the grotesque. He raised his brows and shoulders and looked at me with an intensity that made me feel embarrassed. I tried to make him forget my apparently silly comment: "At least, when people are sick they like to be machines."

Goridis laughed, cranky, and added, "And why? Because machines can be repaired! However," he added in a somber voice while tapping on his head, "this machine is highly complex and we're far away from repairing it."

He stubbed out his cigarette decisively and walked away with his long steps to continue his experiment or whatever else he was working on.

Again, I was struck by the curious relation of life and science. Goridis, while instructing us that the brain is a chemical machine, had seemed (to me) so intensely machinelike that it was almost frightening. His whole appearance—the way in which he smoked, walked, and talked—resembled that of a nervous machine. Like Ascher, he had performed a statement about the brain, given a bodily performance of adult cerebral fixity, enacted against the claim that the brain is plastic.

The two incidents made me wonder if I could recall a similar encounter with Prochiantz, that is, an encounter in the course of which he met en scène his plastic conception of the brain. But I could not think of a telling incident. Also, I was not sure what to look for. What would a bodily enactment of cerebral plasticity look like?

Life and science III

Only a day later, the relation between life and science surfaced yet a third time. It happened just when I entered the lab. Prochiantz must have been waiting for me, for as soon as he saw me walking through the door, he jumped from his chair, ran out of his office, and aggressively drew himself up in front of me.

"I am not coherent," he shouted. "I don't need a psychoanalysis!"

I was so baffled by the speed and the intensity of the confrontation that at first I could not react.

"I am not coherent!" he aggressively repeated. "I don't need a psychoanalysis!"

Why was he so furious? I tried to calm him down, but I could not appease him. As if caught in a loop, he repeatedly shouted that he was not coherent and did not need psychoanalysis. Eventually, I succeeded in maneuvering him into his office, all the while uttering that I understood, that I was not a psychoanalyst, and that, of course, he was not coherent. In his office, he refused to sit down and stared at me with a challenging posture. I stared back. What had made him work himself up into this rage? Apparently he was worried that I, the anthropologist in his lab, would transform him into something he did not want to be—a "coherent" person. I explained that I had no idea how he could possibly arrive at the assumption that I would try to make him coherent or to psychoanalyze him. "I am an anthropologist of your lab," I said, "not of you." His answer was strange laughter—something between hysterical and artificial, perhaps with a mild shot of relief.

"I am not coherent anyway," he added. "So it would be a waste of time."

He sat down and began to read his e-mails. "Isabelle," he said, without looking at me, "has very good data. Engrailed"—a homeoprotein—"seems to have an in vivo function in the guidance of adult retinal axons."

The intensity of Prochiantz's frightening outburst left me fascinated, for it appeared to me as the exemplary enactment of the plastic conception of the brain I had been looking for but had not been able to find: the enactment of a brain that is not a fixed something, an already developed and, hence, immutably wired machine but, instead, "a living organ" characterized by a "ceaseless morphogenesis" (his words). "Today," Prochiantz explained during a lecture he gave at the Cité de Science in La Villette, "the once hegemonic idea that the brain is an immutable organ, fully and irreversibly achieved by the end of puberty, is dead. In truth, the brain is undergoing a constant renewal. There is nothing that is stable. And therefore it would, from a strictly neuroscientific perspective, be wrong to say that we are coherent beings. There is no coherent ego—we are always in the process of becoming."

There is no coherent ego. Like that of Ascher and of Goridis, Prochiantz's conception of the brain was not an abstract conceptualization but a physically lived statement about it, enacted against what he called the "régime fixiste."[7]

The ethical

The three "incidents" gripped me. Although each one, taken by itself, was arguably of little more than anecdotal relevance (thin ethnographic data, so to speak), they appeared, if taken in relation to one another, to tell a story, a story about life and science, about how the two were intimately related. But what was the "message" of this story? I felt a bit like a philologist, sitting over an obscure ancient text fragment, struggling to identify its possible meanings. Gradually, my philological efforts—"culture is text"—helped me to understand why I found the story so highly fascinating and evocative.

It was highly fascinating insofar as it seemed to prove wrong the often-encountered Weberian dictum according to which science is irrelevant for the ethical question of how one should live (my use of the term *ethics* echoes Weber's use of the terms *leben* and *Lebensführung,* which was deeply informed by the ancient Greek conception of ethics, i.e., the effort to actively take up one's life, to give a certain form to it, usually by way of training to live in accordance with reason).[8] The story seemed to prove the dictum wrong because it clearly shows that—and to what considerable degree—neuroscience is an ethical (in the sense of actively lived) field. First, all three scientists, Ascher, Goridis, and Prochiantz, were apparently living a life under the spell of the brain. They were neurologically human, deeply and seriously convinced that it is the brain that makes us human. And, second, for none of them has this relation between the human and the brain—between life and science—been an abstract one. They all used neuroscientific knowledge, concepts, and metaphors to think about and give form to themselves. They were, this is to say, ethically at stake in their research. In fact, they were not only ethically at stake but physically as well: They physically lived the concepts they deployed; they enacted them (and not just in the episodes mentioned above).

The story was highly evocative because it suggested that different ways of thinking the brain imply different ways of being neurologically human (Ascher an already wired machine, Goridis a chemically regulated machine, and Prochiantz a plastic organism in continuous motion). More abstractly put, the three episodes suggested that each particular conception of the brain allows for a particular ethical space for being human (the space of possibilities within which a given conception of the brain requires one to think the human and be human; here, fixity in two cases, plasticity in one) and provides a particular ethical repertoire for living a life (the knowledge, concepts, and metaphors the given conception of the brain offers for making sense of oneself; here, electrophysiology–wires, neurochemistry–transmitters, and developmental neurobiology–plasticity). This was fascinating because it suggested something that had never before occurred to me, namely, the possibility that plasticity—insofar as Prochiantz's ethical and physical performance differed so profoundly from Ascher's and Goridis's—was a profound mutation of the neurological human, a metamorphosis of the confines within which neuroscience requires all those who live under the spell of the brain, deliberately or not, to think and to live their lives. Has the emergence of plasticity been essentially an ethical event? The possibility got me immensely excited. Let me explain.

During the course of my fieldwork in and around Prochiantz's lab, I had come to think of plasticity largely as a conceptual event. What led me to this conceptual perspective were my inquiries into the history of the neuronal study of the brain. Along my historical detours I began to recognize that two conceptual presuppositions had been constitutive of neuroscience since its emergence in the late 19th century: First, neurons are the main functional building blocks of the brain, and, second, the adult human brain is fully developed and, hence, fixed and immutable. These two presuppositions were constitutive insofar as they organized—structured—neuroscientific knowledge production for more than a century. For, if neurons are the sole functional building blocks of the brain and if neurons are organized in immutable function-specific circuits, then the only way to understand the nervous system is, on the one hand, the exhaustive documentation of these neural circuits and, on the other hand, the unraveling of the information transfer between neurons (hence, the significance of synapses). One can see why plasticity—the assumption of profound morphogenetic changes in the adult—is a conceptual event (indeed, a conceptual scandal): It sets in motion a schema that has organized the neuroscientific production of knowledge since the late 19th century—a schema that has organized experimental interest and has made the knowledge assembled meaningful and relevant. Throughout my research, it was precisely this conceptual motion that interested me: A new, not yet fully spelled-out way of thinking and knowing the brain was emerging, and I was determined to capture this emergence in all of its epistemological, experimental, and social nuances (Rees 2006).

Now, the three "ethnographic incidents" did not put my focus on motion in question—but they led me to think that an analysis of motion very different from the one I had practiced was possible, namely, an ethical analysis. As I had studied plasticity—or the history of neuroscience—from a conceptual vantage point, I could study it from an ethical vantage point. The incidents opened up, this is to say, the possibility of an ethical history—a kind of genealogy of morals—of neuroscience, a history of the neurological human, of how the human has been constituted by the temporal succession of the ethical spaces and ethical repertoires neuroscience provided in the course of its history.[9]

Such an ethical analysis promised to be exciting for at least two reasons, one particular and one general. It was particularly exciting because it made the passionate rejection of plasticity by a large part of the neuroscientific community appear in a new light: Perhaps the observation of morphogenetic changes in the adult was not so much rejected for conceptual (or scientific) reasons as for ethical ones? I found myself seduced into thinking that Prochiantz and his work caused such resistance, deep emotional anger, and fierce polemics simply because it changed the neurological human and thereby the way life had to be lived from a scientific (neurological) perspective. Was it Prochiantz's plastic way of being that aroused his colleagues' decisive disapproval of plasticity? The prospect of an ethical analysis was generally exciting as well because it opened up the possibility that my work was far more than an ethnographic study of a mutation in the neuroscientific order of knowledge—that it was an analysis of a profound metamorphosis of the neurological human, happening in the here and now, and, hence, an account of what kind of (neurological) humans "we" all are in the process of becoming.[10]

Though, were these thoughts of more than anecdotal relevance? More than the product of my (imaginative) mind?

The neurological human, from roughly 1890 to 1990

If one turns to the history of neuroscience from an ethical vantage point, then what does one find? How—in what concrete ways—did neuroscience require all those under the spell of the brain—deliberately or not—to live their lives? What were the confines within which the human had to be thought, in which it had to be lived? Can one write a history of these confines, of their various transformations? And can one write a history of the kind of relation between life and science these confines allowed for?

The shortest possible history of neuroscience

The field of neuroscience emerged at the intersection of two initially independent lines of inquiry, neuroanatomy and electrophysiology.[11]

Neuroanatomy—based on the assumption that neurons, not glia cells, are the main functional building blocks of the brain—gradually emerged in the second half of the 19th century. The decisive event in the emergence of the new discipline—among the many events that were to shape it—was the work of Santiago Ramón y Cajal, a Spaniard who was the first to provide a detailed anatomical understanding of the brain's neuronal organization (one still largely valid today).

At the beginning of Ramón y Cajal's oeuvre stood his work on the morphogenesis of the nervous system, conducted in the late 1880s (Ramón y Cajal 1894, 1991a). Transforming the brain's development into a linear series of ink-pen drawings, he observed that neurons migrate and gradually develop into neural circuits; that the connections between neurons—and this earned him a major dispute with several colleagues—are contiguous, not continuous; and that neurogenesis, the birth of new neurons, is a phenomenon of embryogenesis exclusively. The development of the fine structure may continue for some time, but, once adulthood is reached, the nervous tissue is fully developed and, hence, "fixed and immutable" (Ramón y Cajal 1995, vol. 1:40).[12]

The observation of adult fixity (paralleled by the insight that neurons do not regenerate) led Ramón y Cajal to formulate a structure-functional theory of the nervous system, according to which the key to understanding how the brain works is a detailed anatomical understanding of the geometrical patterns in which it appears to be organized. He spent the rest of his life drawing up a detailed multivolume atlas of the nervous system in its entirety—a construction plan of the various brain centers—carefully documenting the kind of neurons characteristic of each, their typical form and their patterns of connection. His *Histology of the Nervous System of Man and Vertebrates* (1995) became the key document of an anatomical comprehension of the neuronal organization of the nervous system.

Electrophysiology is much older than neuroanatomy. At least since the 1830s, when the idea of neurons did not even exist yet, physiologists had tried to prove that the brain is an electronic machine, organized in function-specific regions. Around 1890, however, neuroanatomy and electrophysiology intersected.

Only a few years after Ramón y Cajal had begun to provide detailed neuronal maps of diverse brain centers, the British physiologist Charles Scott Sherrington attempted what was then rather spectacular: to find a way of applying the tools and devices of electrophysiology within the new conceptual framework provided by Ramón y Cajal's theory and to measure how electricity flows from neuron to neuron and thereby organizes nervous actions.[13] In the course of his effort to fuse electrophysiology and neuroanatomy, Sherrington made a major observation, namely, that the gap across which neurons communicate with one another is not, as Ramón y Cajal had it, a mere anatomical fact; instead, it is the key functional element of the nervous system, which he suggested calling "synapse." Synapses, according to Sherrington (1906), react to diverse incoming stimuli, "integrate and coordinate" them, and thereby enable the organism to act as a unified whole.

With his *Theory of Integrative Action* (1906), Sherrington considerably transformed Ramón y Cajal's vision of the brain. To be sure, for both Sherrington and Ramón y Cajal, the brain was made up of neurons organized in contiguous, fixed, and immutable function-specific circuits. But where Ramón y Cajal had seen anatomical structures, Sherrington saw electronic circuits. He transformed the brain into a nervous machine made up of switching boards, with the synapse as the decisive switch. And thereby he introduced the machine logic constitutive of electrophysiology into the neuronal study of the nervous system.[14]

Perhaps one has to pause for a moment to recognize the beautiful complementarity of Ramón y Cajal's and Sherrington's work. The anatomical descriptions of the former were the precondition for the electrophysiological inquiries of the latter. For, only after Ramón y Cajal had expelled any plastic potential from the adult human brain—no new neurons, (almost) no new connections, no regeneration—could Sherrington succeed in thinking Ramón y Cajal's anatomical drawings as electronic circuits, with the synapse as switch.

From a conceptual vantage point, little has changed since the days of Ramón y Cajal and Sherrington. Until the late 1990s, the basic conceptual comprehension of the brain they came up with remained in place. In fact, it did not just remain in place. It also remained unchallenged.

To be sure, in the course of a hundred years, neuroscientists fundamentally disagreed and debated with one another; they fought fierce battles, in the course of which how the brain was imagined and conceptualized changed, in part dramatically so. However, my claim is not that the image of the brain, as such, remained the same (it did not). Rather, I claim that, no matter which image of the brain one considers, the conceptual premises on which it was based—on which it relied—were those Ramón y Cajal and Sherrington had came up with.

Whether one turns to electroencephalography, which began its success story in the 1920s,[15] or to the cytoarchitectural effort of localizing particular mental traits in particular layers of the cortex, which continued until the 1930s,[16] or to the physiological efforts to determine the nature of synaptic communication, which dominated neuroscience until the 1940s,[17] fixity remained the touchstone. All major approaches, no matter how much their particular tools and technical languages differed, were grounded in the conviction that strictly fixed neural circuits governed by synaptic—chemical or electrical—mechanisms are key to understanding the nervous system.

After World War II, new technical tools, especially the electron microscope and minielectrodes, led to major discoveries, but the conceptual interconnection of ideas within which it was possible to think the brain was left as unaltered as it had been during the first half of the century. Cybernetics, which had been a major inspiration to neuroscience after World War II, changed the basic insights of Ramón y Cajal and Sherrington as little as the emergence of neurobiology in the 1960s or the rise of synaptic plasticity as a major research arena in the 1970s. The equation of brain and computer, which brought about the analysis of neural circuits in terms of translation, feedback, entry, and re-entry was deeply rooted in the image of the brain as a fixed and immutable machine with the synapse as the main communicative device.[18] The biological studies of experience-dependent neural circuits, which became the prime examples of a neurobiological approach in the 1960s, exactly affirmed what Ramón y Cajal had claimed more than half a century earlier: Vertebrates are born with a fixed number of brain cells; the fine tuning of the nervous system may go one for some time, but it comes to a definite halt once adulthood is reached.[19] And the discovery of synaptic plasticity, which marked the neurosciences' return to behavioral questions (after more than half a century of withdrawal), was a breakthrough precisely because it allowed imaging how a fixed and immutable nervous structure can account for behavior changes: namely, by way of increasing or decreasing the intensity of synaptic communication (hence, a functional, not a morphogenetic plasticity).[20]

In the 1980s and 1990s, finally, molecular biology and cognitive neuroscience emerged as dominant approaches. As Ascher has pointed out, both these approaches were technological rather than conceptual events. Molecular cell biology allowed studying synaptic communication on the level of cellular events like translation and transcription.[21] And the various imaging techniques at the core of cognitive neuroscience—essentially a fusion of cognitive psychology and neurobiology—are all grounded in the assumption that the brain is divided into discrete, function-specific regions that are made up of function-specific synaptic circuits.[22] For this focus to be meaningful, one has to presuppose what all of neuroscience presupposed throughout the 20th century: (1) that the brain is a fully developed and, hence, fixed and immutable structure; (2) that this structure is organized in (of course, equally immutable) function-specific circuits; (3) that synapses—given that the rest of the structure does not change its form—are its main functional elements; and (4) that the language of the brain—be it chemical, electrical, or genetic—is machinelike.

In short, no matter how conceptions of the brain over the last hundred years might have differed from one another (and they differed quite a bit), all of them fundamentally relied on presuppositions Ramón y Cajal and Sherrington had initially formulated, most notably, the assumption of adult cerebral fixity. Neuroscience, throughout the 20th century, was characterized by a remarkable conceptual continuity (a continuity that is, in itself, highly fascinating and deserving of a separate study).[23] The question, of course, is what this continuity has meant for the constitution of the neurological human—and for the relation between life and science (for how science has required life to be lived).

The heroic

The changes the neurological human underwent in the course of the 20th century were—like the changes neuroscience underwent—both considerable and negligible. They were considerable as far as the ethical repertoire is concerned. Arguably, it makes quite a difference if one thinks of oneself in terms of the architectural logic of neuroanatomy, the engineering logic of electrophysiology, the chemical (or pharmacological) logic of neurochemistry, the cybernetic logic of communication science, or the genetic logic of neurobiology. Each one of these approaches offers a different kind of ethical equipment for living a (neuroscientific) life: different kinds of words, concepts, metaphors, and images for making sense of one's existence. And yet, from the perspective of ethical spaces, these differences were quite negligible. They were negligible because the ethical spaces these various approaches provided were essentially identical: No matter which terms one employed to think about or experiment on the brain, they always referred back to the fixed and immutable machine that Ramón y Cajal and Sherrington had invented. Consequently, the ethical challenge, no matter what the approach, was to think behavioral phenomena—thinking and living the human—within the narrow confines of the neuroscientific conception of the brain as something immutable, as organized in unchanging geometrical patterns (usually in terms of functional plasticity).[24] The kind of relation between life and science this challenge implied might well be captured with the concept of "the heroic," because 20th-century neuroscience required, at least from all those living under the spell of the brain, a "heroic act": that they submit their lives, their physical existence, their mental particularities to the narrow space the comprehension of the brain as fixed allowed for. This act was heroic not, as a romantic take would suggest, because life as such (in all of its perplexity, irrationality, and intensity) had to be subordinated to science as such (the sober, neutral, and rational). (Such generalizing binaries are not helpful.) It was heroic because science, the antiromantic, deillusionary, realistic practice that it has been for most of my interlocutors, required heroically facing the scientific truth—humans are fixed and immutable machines.[25] In Goridis's exemplary words, "Whether we like it or not, we are machines. I know that many people don't like that idea; they waste their time and energy to show that man isn't a machine. Only, man is a machine. We are machines. There is no escape!"[26]

The scandal of plasticity, or so the contrast between Prochiantz, on the one hand, and Ascher and Goridis, on the other hand, seems to suggest, is that it transformed the neurological human in such a way that the heroic, characteristic of a century of neuroscience, was thoroughly undermined.

Assembling plastic reason

To find out if Prochiantz's work has mutated the neurological human—and if so, then, in what concrete ways—one has to return to his obscure observation, made in 1989, that homeotic genes are expressed in the adult human brain. Why was the observation obscure in 1989? It was obscure because it could not be explained within the conceptual schema then dominant in neuroscience. How could one explain morphogenetic transformations in the mature nervous system as something significant if the conceptual schema claimed that the adult was fixed and immutable? From the perspective of adult cerebral fixity, plastic changes could only be irrelevant. Hence, the challenge—which Prochiantz and his colleagues clearly perceived—was to come up with an alternative way of thinking the brain, one that would make the impossible possible, the irrelevant relevant.

Prochiantz made the search for such an alternative conceptual horizon his personal task. While his researchers endeavored to determine the condition and function of the transfer of homeoproteins in vitro and in vivo, Prochiantz practiced what he referred to as "nocturnal work"—the effort of traveling by association, of traversing various fields (not only scientific ones)—to find authors, books, papers, sentences, words, and images that would help him think the brain in terms of a continuous morphogenesis. Among the many sources from which he assembled ideas and associations, two were particularly important: experimental embryology and "evo-devo" (evolutionary developmental biology, based on the assumption that the key to evolutionary change must be seen in mutations that affect developmental patterns).

What sparked Prochiantz's interest in experimental embryology was the battle biologists like Julian Huxley, Hans Spemann, Hans Driesch, Bernhard von Dürken, and others fought with the geneticists of the so-called Morgan school.[27]

In the 1920s, Thomas Hunt Morgan had claimed that each trait of an adult organism was determined by a particular, corpuscular gene located on a chromosome. To experimental embryologists, Morgan's claim was wholly unsuited for understanding embryogenetic processes, for it implied that development is merely a mechanical, physicochemical realization of preformed traits embedded in genes. What the embryologists had observed, however, was that development is not the result of a blueprint but a highly individual formation process.[28]

Spemann (1938), Huxley (1924), or Ross G. Harrison (1933), for example, described development as characterized by the coming into being of genuinely new and, in their particular realization, always unique forms, forms that cannot be reduced to previous stages in development. This coming into being—occasionally they speak of "emergent forms”—implies that developing organisms are characterized by a fundamental “formability,” which, in turn, implies that development is an “undetermined” and “open” process that cannot be reduced to any kind of deterministic preformationist concept. Organisms, so the claim went, are “molded” or “shaped” (all quotes in this paragraph are from Huxley and de Beer 1934). Hence, the key to understanding development was to understand these sculptural or plastic processes.[29]

For Prochiantz, it was precisely this plastic mode of reasoning—the fact that it was articulated as an alternative to a mechanical understanding of organisms—that mattered. It mattered because it was the looked-for alternative for thinking the brain. Plastic reasoning opened up the possibility of thinking the brain beyond the image of the immutable machine that dominated neuroscience, of thinking the nervous system as an emergent form, a form in formation, with homeoproteins as the key to ceaseless formation processes.

The emergence of evo-devo, the other major source of Prochiantz’s nocturnal work, is usually traced back to three authors, D’Arcy Wentworth Thompson, Richard Goldschmidt, and Gavin de Beer (Carroll 2005). What these three shared was their critique of and their proposed alternative to a central aspect of Darwinism. Whereas Darwin and the neo-Darwinians of the early 20th century had argued that evolutionary change, understood as the result of adaptation, is generally slow, steady, gradual, and continuous, with no apparent break between micro- and macroevolution, Thompson, Goldschmidt, and de Beer argued—each for a different reason and in slightly different terms—that gradual changes may lead only to diversification within already existent species but not to the emergence of new species. New organisms, according to their provocative claim, emerge only in a discontinuous and saltational mode, that is, in mutations that cannot be explained in exclusively adaptive terms.[30]

They saw the material basis of such saltational mutations in small-scale changes in the timing pattern of developmental processes. Such mutations, they argued, may have the effect of accelerating or retarding the development of a particular organ—or several of them—while the rest of the organism develops in “normative time.” The species they most often referred to as an example of such saltational emergence is *Homo sapiens:* The human animal reaches sexual maturity in normative time, but other parts of the organism, specifically the brain, remain in a larval or embryonic state (de Beer 1958; Goldschmidt 1940). According to de Beer, it is precisely the continued fetal state of the brain that makes humans human: It brings about, he speculated, an increase in “histogenic plasticity” (de Beer 1958:122) of the nervous system and thereby allows for lifelong adaptation in the form of learning and behavior changes.[31]

The relevance of evo-devo for Prochiantz’s work is apparent. It provided a (highly) plausible (biological) explanation for the continuation of embryological processes in the adult (they are the result of evolutionary change) and thereby made it possible—and plausible—to think the adult brain in terms of the plastic mode of reasoning the experimental embryologists had invented. Furthermore, de Beer’s speculation opened up a curious possibility: If human evolution is due to the emergence of adult cerebral plasticity, and if this plasticity is due to the continued expression of homeotic genes in the mature nervous system, then Prochiantz and colleagues discovered the event that makes all of us human: the nonautonomous transfer of homeoproteins in the adult brain.

These two spotlights on Prochiantz’s nocturnal work may suffice, or so I hope, to convey a sense of how he set the brain in motion: He extended basic developmental processes into the adult. In fact, he did more than that. For, once development is extended into the adult, it stops being development—insofar as it is no longer directed toward some end—and becomes motion, ceaseless morphogenetic motion. And this ceaseless morphogenetic motion is, according to Prochiantz, precisely the defining feature of the human brain, the perspective from which it has to be thought.[32]

The impact this new, plastic vision of the brain had on the neurological human as it had existed since the days of Ramón y Cajal and Sherrington was most profound.

The plastic

From an ethical perspective—or from the perspective of the relation between life and science—the significance of plasticity is that it literally pulled down the narrow walls within which the neurological human had existed for so long. Where new neurons proliferate, differentiate, and migrate, where synapses are born, where axons and dendrites appear and disappear, there is no need to think the human—no need to find means to think the human—from the perspective of fixed and immutable circuits. In fact, the contrary is the case: With the advent of plasticity, the challenge is to think the human—to live a life—from the vantage point of a brain that is believed to continuously change, to undergo ceaseless morphogenetic transformations. The inversion is almost one-to-one: Where once fixity reigned, now plasticity rules. Where once the basic feature of the neurological human was its relative immutability, it is now its openness toward the future, its capacity for ongoing adaptation.

The ethical consequences of this emergence are quite profound: Human beings cease to be fixed and immutable machines, cease to be already wired information-processing computers of sorts. With this cessation, a large part of the ethical repertoire that had been assembled in the course of more than a century of neuroscientific work becomes marginalized. The language of wires, of switching boards, of computers, of programs—once so central to thinking the human, to making sense of oneself and of one's experiences—does not apply to a brain that is understood to be a living organ, part of an organism that is in a constant interplay with a milieu to which it needs to adapt continuously. It is replaced by talk about growth, induction, differentiation, proliferation, flexibility, morphogenesis, adaptation, individuation.

In sum, the emergence of adult cerebral plasticity has been a major event in the history of the neurological human, an event that has changed both the ethical space neuroscience provides for being human (from fixed to plastic) and the ethical repertoire it provides for living a life (from mechanical and chemical terms to embryological and developmental ones). And, as such an ethical event, the emergence of plasticity has had severe consequences for the heroic because the heroic was heroic only as long as life had to be lived within the narrow confines of fixity, only as long as humans had to face the unpleasant but inevitable truth that we were fixed and immutable machines. With the rise of what I propose calling "the plastic"—the ethos of being a proliferating, polymorphous organism deeply committed to a body-conscious existence of continuous change—the heroic becomes a relic, a survival of a previous epoch in which the brain was misunderstood. And this descientification of the heroic—the historicization of the image of the human on which it was based—is precisely (at least, from an ethical perspective) what has made plasticity such a scandal.[33]

A battle about the human

Thinking through my field notes, working through biographies and autobiographies of neuroscientists, and, especially, reading masses of popular science books neuroscientists have written during the last hundred years, I gradually came to understand (or believe) that for much of the 20th century, the heroic effort to live a sober life within the narrow confines neuroscience provided for being human was the precondition for becoming—and remaining—a good and successful neuroscientist. The heroic, or so my tentative research results suggest, was an ethical practice on which neuroscientists judged each other, almost a code of conduct that helped them evaluate the seriousness of junior colleagues. The heroic was the normative ethos of the discipline. All those who did not submit to this heroic ethos—who made claims about the brain that were incommensurable with a vision of the brain as fixed—were regarded as unscientific or, when things became more intense, as irrational, as metaphysicians or enemies of science.[34]

From the perspective of the heroic, then, Prochiantz—his work, his way of being, his performance of neuroscience—was an outrageous affront. To be clear, what made this affront outrageous was not merely that it ran counter to the heroic code of conduct: It was outrageous, first and foremost, because it was grounded in the deeply serious and scientifically committed claim that the neuroscientific understanding of the human on which this code of conduct was based was wrong, that it was a relic, a survival, of a time when scientists did not yet really understand the brain. In other words, when Prochiantz claimed that the brain is plastic—when he aggressively began to live the plastic—he not only performed a new neurological human but he also confronted his colleagues with the claim that they were living an unscientific—a metaphysical—image of the human. Prochiantz's plastic way of being neurologically human, his descientification and historicization of fixity, is the key to the deeply emotional and personal polemic against his work. In fact, the term *polemic* is misleading here, for it was not a polemic, it was a battle, a fierce battle about the neurological human, about how to think it, about how to live it. And this battle was not an abstract one, a disagreement about concepts, but a lived one, in which both parties found themselves personally—as human beings, as bodies, as reflective subjects that make experiences—at stake. This accounts, or so I think, for the emotion—and the delayed, turbulent, and noisy arrival of plasticity. The three "ethnographic incidents" with which I began this article, are windows onto this lived debate.

Instead of a conclusion

Perhaps there is, at the end of this article, no need to return to the already said. Instead of rehearsing the idea that the emergence of adult cerebral plasticity was a profound mutation of the neurological human, a conceptual as well as an ethical event, I end with four clarifying remarks.

Remark 1

It would have been grossly misleading had I conveyed the impression that plasticity is a liberation. First, the central aspect of the relation between life and science—namely, that life is lived within the confines science offers—is untouched by the plastic (see N. 33). Plasticity—even though it allows the human to escape the narrow confines of fixity—is a scientific concept. Second, Prochiantz practiced science with exactly the same attitude as the colleagues with whom he fought. To him—perhaps even more than to them—science meant an antiromantic, deillusionary, essentially realistic practice. Third, instead of a liberation, it might be more appropriate to speak of a new "regime of living" (Collier and Lakoff 2005): Where, before, fixity—also in the positive sense of maturity, of being accomplished—was the norm, it is now the duty to change, to be open to the new, to be flexible. In fact, the coevolution of adult cerebral plasticity and the political and economic figure of flexibility is a striking—but largely unexamined—phenomenon (though see Harvey 1990; Martin 1994; Rose 2006).

Remark 2

One might object that I have paid attention merely to a marginal, fairly irrelevant lab, to the conceptual dreamings of a single neuroscientist, and that I have illegitimately made huge claims about the neurological human as such, about how it is changing today. However, I have insufficient space here for a detailed account of the coming of plasticity research in general or of how Prochiantz's work has become significant to this new field of inquiry. It must suffice to indicate that in 1998, soon after the first reports on adult neurogenesis were published, Prochiantz left the margins and became a celebrity (in 2006, he was elected to the Collège de France, where he occupies a chair entitled "morphogenesis of the nervous system"). What was before regarded as nonsense was now viewed as avant-garde. It became avant-garde because Prochiantz provided a set of answers to questions that are only now being asked on a broader basis: how to think morphogenetic changes in the adult and how to explain their significance.

Remark 3

In this article, I have focused my efforts on illuminating the "ethical," or "lived," battle that surrounded the emergence of plasticity. However, an ethical analysis, insofar as it aims at shedding light on the kind of neurological humans we are in the process of becoming, would also require following the various ways in which a new ethical (neuroanthropological) norm, the plastic, travels away from the bench and into the world: how it gives rise, for example, to new understandings of neurodegenerative diseases, how these new understandings become part of biomedical platforms, of (psycho)therapies (neuropsychoanalysis), and how they become part of pharmacology. Likewise, it would require following the ways that lead from the bench to the science sections of newspapers, journals, and magazines (that convince readers that plasticity is a good thing).

Remark 4

Finally, and more generally, a frequent response to earlier versions of this article was that it offers a Kuhnian story of a paradigm shift. I think that this response is inappropriate for two reasons.

First, the aim of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) was to show that scientific knowledge does not progress in a continuous, linear way. Given this aim, Kuhn favored a language of discontinuity and incommensurability: A period of normal science (in its conceptual organization fixed and immutable) is interrupted by a revolution (equivalent to the process of dissolving and rearranging the existing conceptual order) that will give rise to a new period of normal science (one that is, in its conceptual organization, incommensurable with the previous one). In contrast to this almost architectural epistemology of paradigm shifts, I like to think of the previous pages as informed by a biological epistemology of knowledge. Where Kuhn sees stable structures separated by definite breaks, I see continuous lines of mutation of various elements of a given order of knowledge.[35] For the most part, these mutations are slow and small in scale and, therefore, go by unnoticed (which does not mean that they are not, in the long run, effective agents of change). But sometimes they occur quickly, or concern a key organizing element of a given order of knowledge (or both)—with an immediate and spectacular impact on the overall organization of that order.[36]

In more concrete words, the observation of plastic changes in the adult brain has caused neither a paradigm shift nor a revolution. Rather, it resulted in a mutation of one key organizing element of the neuroscientific order of knowledge (morphogenetic fixity), and this mutation has set in motion the whole order as once known (with the consequence that some of the formerly central elements of that order have become decentered and marginal while some new elements have gained center stage).

Second, Kuhn was a historian and philosopher of science, interested in the nature of scientific knowledge and its growth. In contrast, I am a philosophically inclined anthropologist, interested in emergent forms of thinking and knowing the human—emergent in the sense that these new forms are not reducible to already established categories of knowledge but that they derail those categories and thereby require a rethinking of the given (such established categories include "culture" and "society").[37]

Instead of a description of a paradigm shift (or a cultural–social analysis of science), therefore, the previous pages offer an experiment in philosophical anthropology, taking the key constitutive question of ethics—What shall we do? How shall we live?—and addressing it to contemporary neurobiology to find out what kind of neurological humans we are in the process of becoming. In other words, I have sought to analyze the emergence of a new form of knowing the neuronal human from an ethical perspective (with the side effect of documenting that—and to what an extraordinary degree—neuroscience is an ethically lived and bodily enacted field).

Notes

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1. The distinction between life and science is, when it comes to modern European thought, no minor matter. Even a brief literature survey cannot fail to notice the extraordinary career of the theme among European philosophers. For example, a considerable part of 20th-century German philosophy found its main preoccupation in delimiting life from—indeed, in philosophically occupying and defending life against—science. The evident starting point is the work of Max Weber. Weber (1958:143) assigns the idea of an unbridgeable gap between life and science to Leo Tolstoy. On Tolstoy's conception of life and science, see Gustafson 1986. On the relation between Weber and Tolstoy, see Hanke 1993. For a brilliant contextualization of Weber's distinction between life and science in post-Hegelian German intellectual culture, see Schnädelbach 1984. On the relevance of the distinction in Weber's personal life, see Radkau 2005. Relying on Weber, Karl Jaspers (1919) has sought to identify concrete human experiences for which science is irrelevant. Martin Heidegger (1967), taking up Jaspers, has argued that the task of philosophy is to think the human precisely from the perspective of those experiences to which science is irrelevant (on the significance of Georg Simmel's conception of "life" as irreducible to "science"—and on its relevance for, especially, Heidegger—see Großheim 1991). Hans Georg Gadamer (1960), a famous student of Heidegger, has insisted on the significance of nonscientific truths for things human. What holds true for German intellectual culture is equally true—if in different ways—for France, a country in which (from Claude Bernard and Henri Bergson via Georges Canguilhem and François Jacob to Michel Foucault and Gilles Deleuze) the relation of life and science has been a central concern of philosophers. And via Foucault—whether in the form of biopower or the ethical relation between knowledge and the subject of knowledge—the distinction–relation between life and science has become a central topic as well in American anthropology.

2. For Prochiantz, this plastic potential is, on the one hand, an expression of the brain's irreducible openness toward the future and, on the other hand, the material substrate of a continuous individuation by way of adaptation (Prochiantz 1988, 1993, 1995, 1997, 2001). Prochiantz's concept of "plasticity" is deeply indebted to Bernard (Prochiantz 1990) and Bergson (Prochiantz 2002).

3. If they had not been in France, where almost every researcher is a state civil servant with a fixed salary independent of research, he and his coworkers would have had to shut down their lab.

4. The articles that, quite literally, marked a breakthrough were those of Elizabeth Gould et al. (1997) and of P. S. Erikson et al. (1998). For reviews, see Gross 2000, Kempermann 2006, and Specter 2001.

5. For a historical account of the idea that the brain is essentially an electronic—and wired—machine, see Dierig and Bartsch 2000 and, more specifically, Dierig 2006 (which traces this idea back to Emile Du Bois Reymond). See as well Borck 2005.

6. Pasko Rakic, in a gigantesque series of experiments (more than 200 macaque monkeys were involved), investigated the neural development of the primate brain. Primates, he found, are born with a specific number of neurons that are structurally (almost) fully in place at the time of birth. Synaptogenesis—the birth of synapses and the establishment of synaptic links—dramatically increases until the age of sixteen (roughly). At that point, a massive decline of new synapses and of connectivity sets in. "To learn," as Jean Pierre Changeux has interpreted this process, "is to eliminate" (1985:301; see as well Edelman 1987). In the early twenties, this decline comes to an end, and thereafter the structure of the brain remains largely unaltered, although the organ exhibits a slight tendency toward decreased synaptic connectivity (an accumulative event supposed to account for dementia). On Rakic, see Dove 2005.

7. I have no investment in saying that Prochiantz is not a machine and that Goridis and Ascher are. In no way do I intend to delimit their lives by their science. What interests me here is the kind of language my interlocutors themselves used to imagine—enact—themselves as nervous systems–humans. And, here, one thing is striking: Whereas Ascher and Goridis—and many others, indeed—spoke about the brain as a fixed and immutable machine (a closed system), Prochiantz insisted that the brain is a "living organ," characterized by a "ceaseless morphogenesis" and "continuous growth" (an openness toward the future). Ascher and Goridis rejected plasticity because it undermined the basis of the machine that they conceptualized the brain to be, namely, fixity. And it was precisely this undermining that was attractive to Prochiantz: Plasticity literally destroys the idea of fixity—of an immutable machine—and allows for a different conception of the brain–the human. This different conception was, for him, captured by the terms *morphogenesis, living organism, individuation, adaptation,* and so on.

Certainly this does not mean that Prochiantz sees nature and technology as mutually exclusive ontological realms. In an interview in late 2002, he explained to me that "the equation of the brain with a machine, aside from being ridiculous, has become irrelevant, a relic of a past when we still had to build machines that mimic the brain. Today," he added, "thanks to genetic engineering, we can work with the real thing." Prochiantz's point—echoing Canguilhem (1952, 1955)—was that "machines don't self-reproduce. They neither induce nor generate" (for a similar argument, despite Neumann and Burks 1966, written largely against Haraway, see Hacking 1998). And he added, "Those who say that the brain is a machine are really the ones who establish the distinction between organism and machine. For by saying that it is a machine they anthropomorphize the brain."

8. For the immediate relevance of ancient Greek concepts of ethics for Weber's idea of "Lebensführung," see Hennis 2003. See as well Radkau 2005 and Rabinow 2003.

For the argument I seek to develop here, the distinction between "morals" in the modern sense and "ethics" in the ancient sense (often referred to as "virtue ethics") is critical. By "morals," one usually refers to a philosophical system that tries to define what is right or wrong in the abstract. Morality, this is to say, is about rules, about abstract universal rules, independent from concrete situations and lives. In this respect, the idea of a general moral system is inseparably related to European modernity (Jonsen and Toulmin 1990; MacIntyre 1967, 1981). Furthermore, morality is usually concerned with moral questions exclusively. It does not touch on all domains of life—or only insofar as they involve moral issues (Striker 1996; Williams 1985). "Ethics," at least in the way the ancients understood it, has little to do with what is generally right or wrong. Ethics, first and foremost, refers to the effort—the practice—of actively living one's life. It is the effort of giving a form to one's existence–being. Whereas morality requires obeying rules, ethics is an all-encompassing practice: work on the self in which one's very being is at stake. The ideal form of life—and this could only be achieved by continuous exercise—was one that succeeded in making *ethos* and *logos* coincide, in a life lived in accordance with reason. For a detailed account of the ancient Greek sense of the term *ethics,* see, above all, Rabbow 1954; Hadot 1995, 2002; and Foucault 2001. See as well Hadot 1969; Pohlenz 1942; and Nussbaum 1986, 1994.

9. The idea of using the ethical as an analytic device for understanding what kind of humans we are in the process of becoming had been prefigured by my reading of Simmel and Weber, specifically their conversation about Friedrich Nietzsche's *Genealogy of Morals* (a conversation first brought to my attention by Hennis 1987:172ff; see as well Hennis 1996:101–105). Weber

appears to have learned from Simmel's work on Nietzsche—specifically from his book *Schopenhauer und Nietzsche* (1995) and his essay "Friedrich Nietzsche. Eine moralphilosophische Silhouette" (1992)—to use "the ethical" as a key analytic device to find out "what kind of human beings" ("welcher Typus Mensch") we are in the process of becoming (cf. Hennis 1987:173–174).

I stress this use of "the ethical" as an analytical device to avoid misunderstandings. My discussion here is fairly distant from the various—and often brilliant—ways in which anthropologists have touched on ethics (for a general review, see Faubion 2001a:118–138, 2001b). It is guided neither by ethnographies of the ethics of Others (of which the ethnographic archive reveals quite a bit; see, e.g., Brandt 1954 or Geertz 1960, 1973) nor by the (often Foucault-inspired) anthropological inquiries into the ethicopolitical self-relations of Others (cf. Mahmood 2005 or the exceptional work of Charles Hirschkind [2001, 2006]). Also, this article is not related to anthropological inquiries into the complex relation between "ethics" and biology or biomedicine (Cohen 1999; Franklin 1995, 2003a, 2003b; Kleinman 1995; Rabinow 2003; Strathern 2005) or the important reflections on an ethically motivated anthropology (Das 2006; Farmer 2001, 2003; Kleinman 2006).

10. I say "we" here insofar as all humans are, in one way or another, "neurological," subjects (in one form or another) of neurological scrutiny.

11. Those interested in the history of the neuronal study of the brain are faced with a conceptual dilemma: How should one name this field, which emerged in the late 19th century? The term *neuroscience* was introduced in the 1960s. It was meant to unite the various disciplines engaged in the neuronal study of the brain: neurobiology, neurochemistry, neurophysiology, neuroanatomy, and so on. Therefore, it is, strictly speaking, not appropriate to call pre-1960s work "neuroscience." However, to simply call it "neurology" would be misleading as well—for that term refers today to a clinical subdiscipline of the field. So I decided to refer to the field in its entirety with the term *neuroscience*—in the singular.

The history of neuroscience that I present in the text focuses exclusively on research that tries to account for the human mind by way of studying the human brain. I leave untreated the study of what one might call the "Freudian brain" or the "Gestalt brain" or other alternative approaches to the human mind (for these approaches focused on the mind much more than on the brain).

12. Only a few years later, summarizing his histological studies, Ramón y Cajal declared, "Once development was ended, the fonts of growth and regeneration of the axons and dendrites dried up irrevocably. In the adult centers, the nerve paths are something fixed and immutable: everything may die, nothing may be regenerated" (1991b:750). On Ramón y Cajal's work, see Shepherd 1991. See as well Finger 2000 and DeFelipe 2006.

13. Around 1890, Sherrington began to anatomically study the neuronal organization of various reflex arcs, especially the knee jerk. He traced the sensory neurons (skin) to the motor nerves (spinal cord) and showed that they are related to muscles. And, further, he showed that this arc was governed by electronic impulses. The challenge in doing so was considerable, for it required, on the one hand, identifying function-specific neural circuits and, on the other hand, developing electrophysiological tools that would be small and precise enough to study them. See Sherrington 1906. On Sherrington, see Breidbach 1997 and Smith 2000, 2001.

14. The impact of Sherrington's theory on Ramón y Cajal's anatomical sketch of the brain was that of a functionalization or, to be more precise, a machinization in electronic terms.

15. A brilliant historical account of electroencephalography is offered by Borck 2005.

16. The main proponents of cytoarchitecture were Cécile and Oskar Vogt (1926, 1930). For a historical study of their work, see Hagner 2004.

17. The decisive work on identifying the mechanisms of synaptic communication was conducted by A. L. Hodgkin and A. F. Huxley (1945, 1947), John Eccles (1948), and Bernhard Katz (1948). For a general historical account on synaptology, see Bennett 2001.

18. Perhaps the most influential works of cybernetic neurology were Couffignal 1952, Grey 1953, Neumann 1957, and McCulloch 1965. On the liaison between cybernetics and neurology, see Hagner 2006a and Kay 2001.

19. The reference is to David H. Hubel and Torsten N. Wiesel's work, carried out in Stephen Kuffler's lab, on monocular deprivation in cats (see Hubel and Wiesel 1964; see also Hubel 1988).

20. For the theoretical preparation for this work, see Konorski 1948 and Hebb 1949. The decisive experimental work was done by Brenda Milner (1959), T. V. P. Bliss and T. Lømo (1973), and, eventually, Eric Kandel (1977, 1979). M. M. Merzenich and colleagues (Kaas et al. 1983; Merzenich et al. 1983; Wall et al. 1986) later expanded the idea of synaptic plasticity by providing evidence that it is more than just functional. Although the basic structure is immutable, they argued, new synapses can appear and old ones disappear. The discovery of synaptic plasticity (functional as well as morphogenetic) has been celebrated as a major breakthrough. Where, before, speculation reigned (how can an immutable brain account for memory?), neurologists could now experimentally study memory storage.

21. The molecular mechanisms of synaptic communication were discovered, almost simultaneously, by R. H. Scheller and colleagues (1982) and by Changeux (1985).

22. M. S. Gazzaniga (2004) offers a helpful textbook account of cognitive neuroscience. For an excellent anthropological assessment of brain scans, see Dumit 2004.

23. On conceptual continuity as a feature of 20th-century neurology, see Hagner 2006b, specifically, pages 9, 34–38, 170–179, and 205–209. For general accounts of 20th-century neuroscience, see Albright et al. 2000 and Cowan et al. 2000.

24. Ascher and Goridis may well exemplify continuity and change, respectively, in the neurological human: The former is an electrophysiologist and the latter a neurochemist, but they share a single ethical space—for both the brain and hence the organ that makes them human, has to be thought of as a machine organized in fixed structural circuits. But they operate with vastly different ethical repertoires—for Ascher, the terminology of electrophysiology (a machine governed by wires), for Goridis, the phrases of neurochemistry (a machine governed by neurotransmitters).

25. See Friedrich 1935 for an account of the rise of antiromanticism in France.

26. I underline that I do not equate a "developmentally fixed brain" with a "developmentally fixed human." Twentieth-century neurologists were quite busy finding out how an immutable brain may account for phenomena like memory storage or behavioral changes. So they did not foreclose a "sense of possibility." But, given that all notable neuroscientific approaches considered the brain to be developmentally fixed, the sense of "possibility" was extremely limited (especially when compared with the sense of possibility current conceptions of a plastic brain advertise). Therefore, Goridis could conclude that humans have to accept that, essentially, we are machines. On the absolute prevalence of the machine metaphor in 19th- and 20th-century neurology, see Kirkland 2002 and Dierig 2006.

27. The battle has been reviewed many times. See Allan 1987; Fox-Keller 2002; Gilbert 1991, 2000; Haraway 2004; Moss 2003; and Sapp 1987.

28. The concept of "individual development" was first introduced by Karl Ernst von Baer (1828). See Oppenheimer 1967 and Russel 1916.

29. Dürken, a colleague of Spemann and Driesch, captured the embryologists' conception of life in a poignant formulation when he equated this mechanism with "präformistische Starrheit" and opposed it to the approach of embryologists, which he equated with "epigenetische Plastizität." The classical point of reference here is Huxley and de Beer 1934. For historical assessments of embryology, see Hopwood 2009, Kirschner 2003, Maienschein 1991, Mocek 1974, and Querner 1977.

30. It is not quite correct to say that all three, Thompson, Goldschmidt, and de Beer, were saltationists. Thompson, for example, did not speak about mutations but about physical forces that change developmental patterns. And de Beer was only adapting some of Goldschmidt's theoretical reflections about saltationism. And yet, what all three shared was the idea that the key to evolutionary change must be seen in transformations of developmental patterns that cannot be accounted for in classical neo-Darwinian terms. For a more detailed discussion of the similarities uniting and the differences separating Thompson, Goldschmidt, and de Beer, see Rees 2006.

31. In de Beer's words,

> It is worth noting that man, whose phylogeny we have seen to be characterized by paedomorphosis, largely owes his success to the fact that he is not adapted to any particularly restricted mode of life at all. Instead, he is fitted for all sorts of habits, climates, and circumstances. Man himself is generalized, not specialized, and his body has retained a large number of primitive features which other mammals have lost. . . . Man is neotenous. [1958:122, 71]

In a word, man is plastic.

32. Prochiantz is quite conscious of this transformation of development into motion. "In reality," he wrote to me in an e-mail, "there are no forms. Forms would require some kind of stability. Though this is not the case. Everything is always, constantly in motion. All there is, is motion." In an interview I conducted in November 2006, he circumscribed his idea with the following phrases: "une modification infinie, sans cesse," "un renouvellement et une modification permanente de la matière cérébrale," and "le processus est sans fin, jusqu'à la mort de l'individu ou l'extinction de toute vie de la Terre, est, surtout, sans finalité."

33. I defined as heroic the kind of relationship between life and science to which the conception of the brain as fully fixed gave rise because it required humans to heroically face the fact that we were machines. With the rise of a plastic conception of the brain, a different kind of relationship between life and science emerges, one that I have suggested calling "the plastic." One could, alternatively, describe every effort to live a life within the confines science offers as heroic. Then "the plastic" would not be a departure from, but a reconfiguration of, the heroic. See Remark 1 in the final section of this article.

One of the peer reviewers of this article has suggested that some 20th-century approaches to the brain also displayed a plastic ethos of sorts, notably, cybernetics and neural net theory and their efforts to account for memory, experience, or behavioral changes. However, cyberneticians—just as neural net theorists did—clearly conceptualized the brain—on the level of its structural organization—as an immutable machine. Their fascinating question was, how can an immutable brain give rise to memory or experience? And their answer was, reverberation, feedback, and the like. Nowhere did they offer a conception of brain plasticity that is comparable to the concept of a lifelong embryological plasticity according to which the brain itself continuously abandons and generates new nervous tissue and thereby changes its form. (And how could they have? It would have put in question their systems approach.) It would be seriously misleading, therefore, to argue that they had a plastic ethos. Rather, they were machinists of the mind.

34. There is ample evidence of the vitality with which neuroscientists defended the concept of "fixity" throughout the 20th century. The response to Prochiantz was certainly not exceptional. Joseph Altman, who reported on the birth of new neurons in adult mice in the 1960s, was refused tenure at MIT (see Kempermann 2006). Michael Kaplan, who rediscovered the birth of new neurons in the 1970s (he had not known about Altman), was forced to give up research on plasticity (Kaplan 2001). Michael Merzenich's efforts (in the 1970s) to publish his observation that new synapses are born in the adult brain were aggressively prevented and subjected to severe attacks (Doidge 2007). Fernando Nottebohm, who yet again rediscovered adult neurogenesis (in the 1980s), recalls fierce polemics against him (Specter 2001). One could further refer to the cases of Martin Schwab, Arturo Alvarez Buylla, and, especially, Elizabeth Gould.

35. I owe the phrase "small lines of mutations" to Stephen Collier. See as well Deleuze 1997:x.

36. Without ignoring the apparent differences between Thomas Kuhn, on the one hand, and Gaston Bachelard, Georges Canguilhem, and Michel Foucault, on the other hand, it seems apparent that all four favored an epistemology of discontinuity. My inquiries into the history of neuroscience, though, have led me to think that none of the major events that have shaped that field thus far can be described as a definitive rupture (and even the assumption of stability seems problematic). Rather, I found continuous motion and growth in various directions, some fruitful and others doomed to decay. For a related account of the history of biology, see Rheinberger 2009.

37. Rabinow has articulated a key question of contemporary philosophical anthropology:

> Observing, naming, and analyzing the forms of anthropos is the logos of one type of anthropology. How to best think about the arbitrariness, contingency, and powerful effects of those forms constitutes the challenge of that type of anthropology, understood of Wissenschaft or science. To place oneself amidst the relationships of contending logoi is to find oneself among anthropology's problems. [2003:30]

See as well Rees in press.

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